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EDITOR

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

AT the meeting of the American Mathematical Society on December 27 and 28, 1918, it was voted that a committee of three should be appointed to report to the Council at the April meeting on "steps which should be taken to organise or promote the publication, in America or elsewhere, of an adequate and comprehensive survey of the current mathematical literature of the world." The committee appointed was E. W. Brown, G. A. Miller, and R. C. Archibald (*Amer. Math. Monthly*, 1919, **26**, 90). At this moment the present quarterly account of recent work in mathematics is the only one in English which aims at any high degree of completeness: it is not creditable to the Governments and scientific institutions of English-speaking countries that this work should have been left to private endeavour (*cf.* SCIENCE PROGRESS, 1917, **11**, 686–90).

An account of the latest doings of the Mittag-Leffler Institute for pure mathematics appears elsewhere in the present number.

Education.—E. J. Moulton (*Amer. Math. Monthly*, 1918, **25**, 429–34) considers the content of "a second course in calculus." B. A. Howard (*Math. Gaz.* 1919, **9**, 317–21) considers the teaching of geometry to first-year pupils (*cf.* SCIENCE PROGRESS, 1919, **13**, 521).

A report of the teaching committee of the Mathematical Association on the teaching of mechanics is published in the *Math. Gaz.* (1918, **9**, 265–92). The concluding part of a suggested introduction to infinite series by W. J. Dobbs is also published in the same *Gazette* (1919, **9**, 299–301).

In these accounts of Recent Advances, mention has previously (SCIENCE PROGRESS, 1916, **10**, 607) been made of the "Research Papers" of the University of Edinburgh. The

papers for 1916, 1917, and some of those for 1918 have now been published, and are, like the ones spoken of before, extracts from the *Proceedings of the Edinburgh Mathematical Society* and *Proceedings of the Royal Society of Edinburgh* of original contributions to mathematics made by Prof. E. T. Whittaker and his pupils. Accounts of the papers will be given in their proper places, but here it is necessary to mention the extremely valuable work which, it would seem alone in British universities, is being done at Edinburgh, chiefly by the Professor there, in encouraging and systematising original work.

History.—George Sarton (*Science*, 1919, **49**, 170–1) announces that the publication of *Isis*, his Belgian quarterly devoted to the history of science, which was interrupted in 1914, will shortly be resumed with collaboration—principally of Charles Singer (*cf.* also *Amer. Math. Monthly*, 1919, **26**, 118–19). *Cf.* also SCIENCE PROGRESS, 1917, **11**, 452, and 1919, **13**, 364.

Philip E. B. Jourdain (*Mind*, 1919, **28**, 123–4) adds some remarks in explanation of his previous article (*cf.* SCIENCE PROGRESS, 1916, **11**, 92) on Zeno's arguments. F. Cajori (*Science*, 1918, **48**, 577–8) draws attention to H. H. Joachim's translation of the tract on indivisible lines attributed to Aristotle. It contains five arguments in favour of the existence of indivisibles, twenty-six arguments supporting the contrary view, and twenty-four arguments intended to establish the impossibility of composing a line out of points. When Cajori says that he has not seen the tract used in any history of Greek mathematics, he overlooks the *Geschichte* of Hankel and various recent discussions on Zeno and continuity in *Mind* and elsewhere.

G. R. Kaye (*Scientia*, 1919, **25**, 3–16; *cf.* SCIENCE PROGRESS, 1919, **13**, 345) shows that it is very probable that India was indebted to Greece for perhaps all of its mathematical knowledge.

R. B. McClenon (*Amer. Math. Monthly*, 1919, **26**, 1–8) considers in some detail the work of Leonardo of Pisa and his *Liber Quadratorum*.

Cajori (*ibid.* 15–20) maintains, against J. M. Child (*The Geometrical Lectures of Isaac Barrow*, Chicago and London, 1916), that Barrow cannot be credited with inventing a *calculus*, although " he worked out a set of geometric theorems suggesting to us constructions by which we can find lines, areas, and

volumes whose magnitudes are ordinarily found by the analytical processes of the calculus." The real point of Child's work has been appreciated by most mathematicians, including Sir Ronald Ross (SCIENCE PROGRESS, 1919, **13**, 634–5).

C. Tweedie (*Math. Gaz.* 1919, **9**, 303–5) gives some notes on the life and works of Colin MacLaurin (*cf.* SCIENCE PROGRESS, 1916, **11**, 615).

Cajori (*School Science and Math.* 1918, **18**, 778–9) points out that Joseph Fenn, in a publication of 1769, antedated both Playfair and Ludlam in giving " Playfair's parallel-postulate." It may be mentioned that, as Cajori has since remarked to the present writer, this form of postulate was already given by Proclus. On Fenn, we may recall a note of W. Stott (SCIENCE PROGRESS, 1915, **10**, 214).

An interesting letter from Sir William Rowan Hamilton to Humphry Lloyd, written in 1856, which contains, among other things, some remarks on the theory of numbers and quaternions, is given in *Math. Gaz.* (1919, **9**, 302).

An account of recent publications on the history of mathematics is given in the *Boll. di bibl. e storia delle sci. mat.* 1918 [2], **1**, 62–4).

There are thorough accounts of the life and work of Adolfo Viterbi (1873–1917) by G. Vivanti, and Eugenio Elia Levi (1883–1917) by G. Fubini and G. Loria (*ibid.* 33–7 and 38–45 respectively). M. J. M. Hill (*Proc. Lond. Math. Soc.* 1919, **17**, xlii–xlix) gives a full account of the work of O. Henrici (*cf.* SCIENCE PROGRESS, 1919, **13**, 522). The following are among recent deaths of mathematicians : E. R. Neovius (September 26, 1917 ; age 67) ; J. H. Graf (age 66) ; C. Stephanos (age 60) ; O. Reichel (age 83) ; G. M. Green (January 25, 1919 ; age 28) ; K. O. E. Lampe (September 4, 1918, age 77)—editor of the *Jahrbuch* since 1885 ; S. I. Lattès (July 5, 1918, age 45)—*Amer. Math. Monthly*, 1919, **26**, 86, 89, 90. G. B. Mathews (*Nature*, 1919, **103**, 49) gives a short account of the work of Ludvig Sylow, who died at the age of eighty-five on September 7, 1918. Sylow's most noteworthy discovery is his well-known theorem (1872) on groups of substitutions ; he also wrote on other parts of pure mathematics and edited with Lie the second issue of Abel's *Œuvres* (*cf.* also *Amer. Math. Monthly*, 1919, **26**, 119). A short notice of the work of the late A. M. Liapounoff is given in *Nature* (1919, **102**, 509–10). In a notice

(*Rev. de. Métaphys.* 1919, **26**, 149–51) of Gaston Milhaud (*cf.* SCIENCE PROGRESS, 1919, **13**, 522), it is stated that he had finished a book, *Descartes Savant.*

Logic and Principles of Mathematics.—C. D. Broad (*Mind,* 1918, **27**, 389–404) shows that the degree of belief which we actually attach to the conclusions of well-established inductions cannot be justified by any known principle of probability, unless some further premiss about the physical world be assumed. Broad does not attempt to state this principle, but Jourdain (*ibid.* 1919, **28**, 162–79), who arrived independently at the need indicated, does. This "principle of causality," which is shown to be more fundamental than B. Russell's "principle of induction," is the necessarily *a priori* assumption that there is a one-one (not many-one, as with Mach) relation between the "universe" of mathematical physics and any part of it. In a continuation of this paper, Jourdain will maintain, against Broad, that *probability* is not a purely logical attribute of propositions which is independent of the nature of the particular world we happen to inhabit.

It is of interest that, according to B. Russell (*ibid.* 124), all the modifications in the *Principia Mathematica* made to the notations of Russell's (1902) logic of relations are due to A. N. Whitehead.

Jourdain (*Nature,* 1919, **103**, 45) gives in detail a very particular case of his theorem (*cf.* SCIENCE PROGRESS, 1918, **13**, 178) for which critics asked and in which chains of type ω are constructed out of chains of respectively all the finite ordinal types. The most notable point is the emphasis laid on the fact that, in the arrangement of the "chains" of M in classes of "direct continuations," there are here none of these classes left over in each of which are not members of *all* finite types. The class (k_n) of all those chains which are of ordinal type n (which exists, by hypothesis, for all finite n's) assigns, without any arbitrary choice, *one* chain to each of the classes of direct continuations already formed—k_n forming, by the rule given, classes exactly like those formed already, but with additions.

D. M. Wrinch (*Monist,* 1919, **29**, 141–5) practically repeats much of B. Russell's theories about "existence." R. A. Arms (*ibid.* 146–52) considers the relation of logic to mathematics, and V. F. Lenzen (*ibid.* 152–60) maintains that White-

head and Russell, for example, fail to give a correct theory of mathematical deduction.

Arithmetic, Theory of Numbers, and Algebra.—L. R. Ford (*Proc. Edinburgh Math. Soc.* 1916–17, **35**; Research Paper, 1917, No. 5) proves Hurwitz's (1891) theorem on rational approximations to irrational numbers by considering the geometry of the classic modular division of the half-plane, and thus exhibits anew the remarkable connection between this geometry and the theory of numbers. J. H. Grace (*Proc. Lond. Math. Soc.* 1919, **17**, 247–58) proves a theorem, allied with those of Hermite and Minkowski, on rational approximations, and gives a classification of such approximations. Grace (*ibid.* 316–9) gives a result on Diophantine approximation.

L. R. Ford (*Proc. Edinburgh Math. Soc.* 1916–17, **35**; Research Paper, 1917, No. 2) finds some striking properties of an interesting class of continued fractions in which any real number can be developed in an infinity of ways, and contrasts these fractions with the continued fractions usually employed.

H. Hallberg (*Journ. Indian Math. Soc.* 1918, **10**, 454–72) concludes his paper (*cf.* SCIENCE PROGRESS, 1918, **12**, 543) on infinite series and arithmetical functions, in which he tries to show how asymptotic formulæ in the theory of numbers may easily be obtained in a manner somewhat different from the usual one, which rests chiefly on the theory of Dirichlet's series.

G. N. Bauer and H. L. Slobin (*Amer. Math. Monthly*, 1918, **25**, 435–40) give several theorems on a certain system of algebraic and transcendental equations. H. T. Burgess (*ibid.* 441–4) explains a scheme which works with ease and simplicity in practice for finding a fundamental system of solutions of a system of linear equations. E. T. Whittaker (*Proc. Edinburgh Math. Soc.* 1917–18, **36**, 103–6; Research Paper, 1918, No. 2) gives a formula for the solution of algebraic or transcendental equations.

Whittaker (*ibid.* 107–15; and No. 3) gives some theorems on determinants whose elements are determinants. Whittaker also (*ibid.* 1916–17, **35**; Research Paper, 1917, No. 1) finds what Sylvester called "the latent roots" of all compound determinants and Brill's (1870) determinants in terms of the latest roots of the determinant of which they are formed. Haripada Datta (*ibid.* 1915–16, **34**; Research Paper, 1916, No. 5) obtains theorems on zero-axial skew Pfaffians and sym-

metric determinants. Sir Thomas Muir (*Proc. Roy. Soc. Edinburgh*, 1918, **38**, 219–25) draws attention to a notation that promises to be helpful in investigating the quadratic relations between the determinants of a 4-by-8 array.

G. A. Miller (*Trans. Amer. Math. Soc.* 1918, **19**, 299–304) extends, especially along the line of substitution groups, his theorems of 1915 (*ibid.* **16**, 399) relating to sets of independent generators of a group of finite order. Miller (*Amer. Journ. Math.* 1919, **41**, 1–4) determines fundamental properties of the groups generated by two operators whose relative transforms are equal to each other.

L. R. Ford (*Proc. Edinburgh Math. Soc.* 1916–17, **35**; Research Paper, 1917, No. 6) explains the fact that more small money is required for the transaction of business in consequence of a uniform rise of prices.

J. L. Walsh (*Trans. Amer. Math. Soc.* 1918, **19**, 291–8) starts from Böcher's theorem (1904) concerning a problem in statics and its relation to certain algebraic invariants, obtains theorems on the location of the roots of the Jacobian of two binary forms, and applies his results to the roots of the derivative of a rational function.

Analysis.—K. Ananda Rau (*Proc. Lond. Math. Soc.* 1919, **17**, 334–6) completes the proof of a theorem stated, but not fully proved, by G. H. Hardy (*ibid.* **12**, 174–80) as to the conclusion of the convergence of a certain series from its summability.

There is a very thorough review of S. Pincherle's *Lezioni di calcolo infinitesimale* of 1915 by E. Bortolotti (*Boll. di bibl. e st. delle sci. mat.* 1918, [2] **1**, 46–51).

W. H. Young (*Proc. Lond. Math. Soc.* 1918, **17**, 195–236) obtains many important results on the convergence of the derived series of Fourier series by arguments based on reasoning analogous to that which he has already employed in a previous communication to the Society mentioned. This paper is connected with two of 1917 (*Proc. Roy. Soc.* A, **93**, 276–92, 455–67), with Young's (*ibid.* 1918, A, **95**, 22–9) paper on the Cesàro convergence of restricted Fourier series, and with Young's (*Proc. Lond. Math. Soc.* 1919, **17**, vi–ix, 353–66) very important proof that there is a certain sub-class (" R. F. series ") of restricted Fourier series such that almost all the more important properties of Fourier series hold good for R. F. series in the in-

terval or intervals to which we restrict ourselves. The results lead to a notable extension of our knowledge of the behaviour of power series on their circle of convergence. Again, connected with this are also Young's (*ibid.* x–xi) investigations on the connection between Legendre's series and Fourier series, and (*ibid.* xi–xiii) on series of Bessel functions (*cf. Proc. Roy. Soc.* 1918, A, **94**, 292–5).

G. H. Hardy and J. E. Littlewood (*Proc. Lond. Math. Soc.* 1919, **17**, xiii–xv) indicate a proof of a theorem given without proof in their paper on Abel's convergence-theorem and its converse, because this theorem contains the complete solution of the problem of Cesàro summability for the Fourier series of a bounded function.

T. A. Brown (*Proc. Edinburgh Math. Soc.* 1915–16, **34**, Research Paper, 1916, No. 1) shows the relation between Whittaker's (1915) " cardinal function " of the functions cotabular with $f(x)$ (SCIENCE PROGRESS, 1916, **10**, 435) and Fourier's repeated integral, gives a new derivation of Fourier's integral formula, and extends the notion of the Fourier integral to the case in which the variables involved are complex.

E. J. Wilczynski (*Amer. Math. Monthly*, 1919, **26**, 9–12), starting from the well-known fact that the existence of a scale of relation is a necessary and sufficient condition that a power series defines a rational function, gives the corresponding condition for the expansion in a power series of a general algebraic function ; this condition seems to have hitherto escaped notice. W. F. Osgood (*Trans. Amer. Math. Soc.* 1918, **19**, 251–74) discusses the nature of an analytic transformation in the neighbourhood of a singular point. M. J. M. Hill (*Proc. Lond. Math. Soc.* 1919, **17**, 320–33) applies the same method that he used in 1902, for obtaining by ordinary algebraic expansion the continuations of series which have one and two singular points, to the case of the hypergeometric series—which has three singular points.

Representations of analytic functions by means of continued fractions have certain advantages over those by power series, but the great impediment to the use of continued fractions in the theory of functions and differential equations is the want of algorithms for adding, multiplying and differentiating them. Whittaker (*Proc. Roy. Soc. Edinburgh*, 1916, **36**, 243–55 ; Research Paper, 1916, No. 6) supplies in some

measure this deficiency: the paper is occupied only with the formal algorithms, and questions relating to the convergence of infinite processes are therefore not considered. Haripada Datta (*Proc. Edinburgh Math. Soc.* 1915–16, **34**; Research Paper, 1916, No. 4) shows that for many important known functions the continued-fraction-expression can be obtained directly and practically from the power series by using a known transformation which involves determinants, and by evaluating these determinants. By this method a considerable number of known isolated results are connected together and exhibited as parts of a systematic theory, and some new results are obtained. Datta (*ibid.* 1916–17, **35**; Research Paper, 1917, No. 4) makes a further contribution towards the same object and shows, amongst other things, that the continued fractions given by Gauss and Heine for the quotient of two hypergeometric or generalised hypergeometric functions may be obtained by a direct use of Heilermann's (1845) transformation. Datta (*ibid.* and No. 7) examines cases of failure of Heilermann's theorem.

W. Gibb (*ibid.* 1915–16, **34**; Research Paper, 1916, No. 3) obtains certain integral relations connected with Whittaker's "confluent" hypergeometric function. G. N. Watson (*Proc. Lond. Math. Soc.* 1919, **17**, 241–6) gives the integral formula for generalised Legendre functions in a form different from that given by E. W. Barnes in 1907.

E. B. Stouffer (*ibid.* 337–52) completes his discovery (*cf.* SCIENCE PROGRESS, 1916, **11**, 269) of a complete system of seminvariants for a certain system of linear homogeneous differential equations by the calculation of complete systems of invariants, semi-covariants, and covariants. The methods used largely avoid the solution of the complicated systems of partial differential equations which arise by the Lie theory. E. B. Elliott (*Proc. Lond. Math. Soc.* 1919, **17**, 306–15), in connection with some papers of 1912 published in the *Proceedings* (1913) of the fifth International Congress of Mathematicians held at Cambridge in 1912, and in subsequent papers, gives examples of the formal "analysation" of solutions of differential equations of certain classes. A. B. Coble (*Amer. Math. Monthly*, 1919, **26**, 12–5) determines a particular integral of the linear differential equation with constant coefficients in a certain special case (*cf.* H. P. Manning, *ibid.* 113). A. Milne

(*Proc. Edinburgh Math. Soc.* 1915–16, **34**; Research Paper, 1916, No. 2) discusses briefly a point in the method of Peano (1888) and Baker (1902) of solving linear differential equations. E. T. Whittaker (*Proc. Roy. Soc. Edinburgh*, 1917, **37**, 95–116; Research Paper, 1917, No. 3) obtains interesting results on the "adelphic integral" of the differential equations of dynamics.

A. S. Merrill (*Amer. Journ. Math.* 1919, **41**, 60–78) discusses completely the conditions for an extreme in the case of isoperimetric problems with variable end-points. G. A. Bliss (*Trans. Amer. Math. Soc.* 1918, **19**, 305–14) deals with the problem of Mayer in the calculus of variations with variable end-points. Eleanor Pirman (*Proc. Edinburgh Math. Soc.* 1917–18, **36**; Research Paper, 1918, No. 1) proves certain results on the solutions of a modified Stirling's (1730) difference equation. T. P. Ballantine (*Amer. Math. Monthly*, 1919, **26**, 53–9) shows that, by a suitable extension of the conception of the difference quotient, the method of interpolation by means of algebraic polynomials for the case when the ordinates are equally spaced can be generalised for the case when the ordinates are spaced according to any law.

Whittaker (*Proc. Roy. Soc.* 1918, A. **94**, 367–83) obtains solutions of integral equations of Abel's and of Poisson's type in forms which can be made the basis of numerical calculation.

Geometry.—G. Loria (*Amer. Math. Monthly*, 1919, **26**, 45–53) gives, in a series of letters, some reflections, historical, educational, and scientific on certain constructions of descriptive geometry.

J. W. Clawson (*ibid.* 59–62) states a general theorem in the geometry of the triangle of which the well-known theorem on the Wallace (or Simson) line is a very special case, and gives a further discussion and application of the theorem. D. F. Barrow (*ibid.* 108–11) examines the envelope of the Wallace lines of an inscribed quadrangle. N. Altshiller (*ibid.* 65–6) remarks that some of his theorems given in this *Monthly* for 1918 (242–6) were proved in 1906 by J. Neuberg.

J. H. Grace (*Proc. Lond. Math. Soc.* 1919, **17**, 259–71) finds the possible relations between the inscribed and circumscribed spheres of a tetrahedron and also some general properties of quadrics in relation to a tetrahedron.

E. Ciani (*Boll. di bibl. e st. delle sci. mat.* 1918, [2], **1**, 52–62) gives a very full account of F. Enriques's *Lezioni sulla teoria*

geometrica delle equazioni e delle funzioni algebriche. A. Emch (*Amer. Math. Monthly,* 1919, **26**, 63–5) develops a theorem of Appell (1918) into a known (Steiner, 1828) theorem of closure on an equilateral hyperbola. A. Lodge (*Math. Gaz.* 1919, **9**, 322–6) calls attention to some simple means of interpolating points in a cubic graph, and of indicating gradients without actual calculation.

A. E. Jolliffe (*Proc. Lond. Math. Soc.* 1918, **17**, 184–94) proves that any cross-ratio of the pencil formed by the four tangents from any point to a nodal cubic is connected by a rational algebraic equation with the corresponding cross-ratio of the pencil formed by joining the points of contact to the node ; and discusses some properties of the quadrangle formed by the points of contact of these tangents. W. P. Milne (*ibid.* 237–40) obtains, in connection with a previous paper published by him in the same *Proceedings,* a symmetrical condition for co-apolar triads on a cubic curve. Milne also (*Proc. Edinburgh Math. Soc.* 1918, **36**, 84–90) writes on the apolar locus of two tetrads of points on a conic. W. R. W. Roberts (*Proc. Roy. Irish Acad.* 1919, A. **34**, 62–6) discusses the equation of the tangent at a given point on a uni-nodal quartic curve.

A. M. Howe (*Amer. Journ. Math.* 1919, **41**, 25–48) discusses all the different algebraic (1, 3) point correspondences between two planes. T. R. Holcroft (*ibid.* 5–24) gives a classification of general (2, 3) point correspondences between two planes.

V. Snyder and F. R. Sharpe (*Trans. Amer. Math. Soc.* 1918, **19**, 275–90) study the space involutions defined by a web of quadrics, which have been treated synthetically by Reye in 1876 and 1879, but only incidentally in connection with line congruences. C. H. Sisam (*Amer. Journ. Math.* 1919, **41**, 49–59) classifies completely the types of algebraic surfaces which are generated by an algebraic system of cubics that do not constitute a pencil. C. V. H. Rao (*Proc. Lond. Math. Soc.* 1919, **17**, 272–305) discusses the curves which lie on the quartic surface in space of four dimensions, and the corresponding curves on the cubic surface and the quartic with a double conic.

APPLIED MATHEMATICS. By S. Brodetsky, M.A., Ph.D., A.F.R.Ae.S., University, Bristol.

There is perhaps no phase of mathematical literature that is less satisfactory than that of notation. We are often told that

science is universal and that it forms one of the most potent links in international relations. One would, therefore, expect to find a truly international scientific language, *i.e.* in mathematics a notation should exist intelligible to all scientists of all countries and trained at all centres of scientific activity. Unfortunately, this is far from being the case. It sometimes happens that persons deriving their mathematical knowledge from one source have considerable difficulty in following the arguments and results of investigations inspired by a different source, and this is often the cause of discomfort and annoyance in the reading of mathematical literature.

Variation in notation can be of two forms. The more obvious consists in the adoption by different workers, or different schools of workers, of different sets of symbols to represent the same physical concepts. A common example is afforded in the discussion of uniform acceleration in dynamics. Whereas we are all agreed in denoting the acceleration due to gravity by the symbol "g," yet some of us denote a uniform acceleration in general by the symbol "a," whilst others use "f." If we compare various books and papers on fluid resistance we find that the statement: Resistance varies as the square of the velocity, is symbolised variously as $R \propto V^2$, $R \propto U^2$, $R \propto u^2$, etc., and that sometimes there is no consistency even in the same book. In hydrodynamics many mathematicians agree to make the velocity components *negative* space differential coefficients of the velocity potential; but not all do so, and in a recent work on aeronautics the authors adopt the negative sign in general but drop it in at least one place.

Whilst it is a pity that such artificial obstacles should exist in the way of the intelligent and comfortable reading of mathematical analysis when applied to physical problems, there is another respect in which notational differences arise, causing considerable confusion in the mind of the mathematical student. This is in the choice of algebraical signs for geometrical concepts. An important case is the choice of a set of axés in three dimensions for problems in astronomy, rigid dynamics, electromagnetism, etc. We are all accustomed to consider the anti-clockwise direction as the positive sense of rotation in dealing with angles in trigonometry, or areas in the integral calculus; yet, when we come to three-dimensional analysis, there is confusion. Thus, in mechanics it is usual in

English books to choose a set of axes which can be defined at any point on the earth's surface by the instructions : take the x axis due east, the y axis due south, and the z axis vertically upwards. In considering rotations about these axes, we adopt the cyclic order $x-y$, $y-z$, $z-x$, so that the screw motions symbolised by positive x, y, z components of velocity and positive x, y, z components of rotation are necessarily on left-handed screws. But this clashes with some fundamental physical ideas in electromagnetism in which we use right-handed screws. Thus in mechanics a rotation in the $y\,z$ plane is positive when it is clockwise as seen from the x axis ; in electromagnetism the rotation is positive when it is seen anti-clockwise.

One is led to speculate whether it would not be an advantage to adopt a uniform notation in this respect. Although the matter is of little importance as regards the prosecution of new research, it is nevertheless of great importance both from the point of view of pedagogy and from the point of view of economy of mental effort, which is so vital to progress in mathematics. It is taken up by Lecornu (*Comptes Rendus*, **166**, 630–32, 1918) in a paper " Sur le signe de rotation." He mentions the fact that in mechanics rotations are reckoned positive when seen clockwise by the observer from the positive direction of the " axis " of rotation, whilst the reverse is the case in astronomy. Yet he decides against any attempt to bring mechanics and astronomy into line with one another. He bases himself on the fact that the *apparent* diurnal revolution of the important heavenly bodies, such as the sun and moon, is clockwise when seen from the north: east to south to west. Thus shadows cast by the sun, as in the case of the gnomon, move on the earth in a clockwise direction, and in this way all mechanical rotations have assumed the clockwise sense. But theoretical astronomy, which considers the *actual* revolutions and rotations in the heavenly bodies, necessarily adopts the opposite direction, namely, the anti-clockwise sense, for positive.

Lecornu considers that it would be contrary to all the usages of practical life to impose on applied mechanics a convention derived from astronomy and opposed to the practical convention that we all adopt almost automatically in practice, and of course theoretical mechanics must not be inconsistent

with applied mechanics. Lecornu is also opposed to any attempt to introduce consistency in the positive sense of rotation in two-dimensional and three-dimensional geometry, and concludes by urging us to let the astronomer and the applied mathematician continue using their respective conventions.

The necessity for a standardised notation has been recognised in the application of mathematics to aeronautical problems, and a committee appointed by the Royal Aeronautical Society has recommended the adoption of a set of conventions and symbols, which, it is hoped, will be used by all workers in the subject. In this system the axes adopted are the ordinary left-handed system mentioned above, with clockwise rotations as seen from the axes, thus following the notation used by Routh and in the books based on his treatises. The actual axes adopted as fixed in the aeroplane, for the purpose of the equations of motion with moving axes, are as follows. Imagine the pilot seated in the aeroplane in flight and facing forwards. If the flight is "normal," *i.e.* horizontal, then the x axis is from the pilot *backwards*. the y axis is horizontal and to the *left*, the z axis is vertically upwards. All velocities and force components are used with the positive sign along the positive directions of the axes, and all rotations and couple components are taken with the positive sign round the cyclic order $y-z$, $z-x$, $x-y$. These axes and conventions were used by Bairstow and his co-workers in the classical wind-channel work carried out during the past ten years at the National Physical Laboratory.

It nevertheless seems doubtful whether this choice is a wise one, especially as regards the positive direction of the x axis. There may be some justification for this from the point of view of wind-channel experiments, for in these experiments the model is at rest and the air is made to move past it, in the opposite direction to the actual flight of a machine. But, as is pointed out by Bryan (*Aer. Journ.* xxii. 1918, 51–3), the effect of the choice is to make the velocities of all aeroplanes negative, and to make all the air-resistance effects positive algebraically but actually negative. This seems to be a very just criticism, especially as Bryan, who was the first to develop the rigid dynamics of the aeroplane, had devised a notation which satisfies most requirements and avoids these peculiar and troublesome sign confusions.

The question of notation in aeronautics is now also one of pedagogical interest, for the great developments that all interested in aviation are expecting in the near future are also mirrored in our academic life. In several colleges and universities provision has been or is being made for aeronautical teaching. In at least two universities courses of lectures have been delivered on the rigid dynamics and the stability of the aeroplane, and the effect will no doubt take the form of increased theoretical and practical activity in aeronautical research.

A notable advance in the study of aerodynamics by mathematical analysis is indicated in a brief note by Bryan (*Aer. Journ.* xxii. 1918, 255–7) containing the equations of motion for a compressible fluid. A differential equation is deduced for the velocity potential—an extension of the well-known Laplacian form when the fluid is incompressible. If mathematical analysis is capable of attacking the problem of the compressible fluid in practical applications, then we have another line of research that should yield results of great value.

Mathematicians in general are paying more attention to the mechanics of the aeroplane, and an interesting sign of the times is the paper by W. Burnside (*Proc. Lond. Math. Soc.* xvii. 1918, 42–53), entitled: "On the Efficiency of a Surface of Pressure Discontinuity regarded as a Propeller."

A paper of considerable interest at the present juncture in aeroplane development is one by W. F. Durand (*Aer. Journ.* xxii. 1918, 183–201) on "Some Outstanding Problems in Aeronautics." Whereas the problem of stability has been studied theoretically and practically with success, not much has yet been done on the control of aeroplanes.

The problem of aeroplane control is, of course, intimately bound up with that of the behaviour of an aeroplane when its steady flight is disturbed, either by a change in the state of the engine, or by a change in the machine, or by a change in the air conditions produced by a sudden squall or a modification of the velocity and direction of the wind. We can say, *a priori*, that any attempt at a complete solution of this problem, applicable to considerable changes in the condition of flight, must end in failure. Not much success has yet been attained in even the simple problems of three-dimensional rigid dynamics, and the complication imported by the involved functions repre-

senting the resistance forces and couples in aeroplane flight render it doubly improbable that general integrals are obtainable. Two methods suggest themselves. One is an extension of that used in the discussion of stability, and consists in assuming that the disturbed motion differs only slightly from the steady flight. The other is based on the method of initial motions, and attempts to solve the problem by means of expanding all the variable quantities in ascending powers of the time as measured from the instant when the steady flight is disturbed, and then equating coefficients of powers of the time in the equations of motion.

Both methods have their advantages and disadvantages. The former needs very much arithmetic, and is, after all, only applicable to a very restricted domain of divergence from the steady flight. The latter has the advantage that with sufficient labour it will yield results for any divergence from steady flight, yet it has the disadvantage that the algebra is very involved, and that no beautiful symmetrical results are possible in terms of determinants. Both methods have already been used—the former by E. B. Wilson in America, the latter by the writer of these notes. In an account of a "Symposium on Aeronautics" (*Proc. Amer. Phil. Soc.* lvi. 1917, 161–257), Wilson discusses the question in some detail. Other information of value in aeronautics is to be found in the symposium.

The construction of aeroplane wings and other frameworks liable to great stresses has given rise to much work on the behaviour of beams under loads. The following are some recent investigations :

W. L. Cowley and H. Levy, Critical Loading of Struts and Structures, *Proc. Roy. Soc.* **94**, A, 1917, 405–21.

W. H. Barley, and H. A. Webb, Design of Aeroplane Struts, *Aer. Journ.* xxii. 1918, 313–29.

H. A. Webb, and J. R. Airey, The Practical Importance of Confluent Hypergeometric Functions, *Phil. Mag.* vi, **35**, 1918, 129–41.

In this paper it is shown that the use of the functions there tabulated renders possible the solution of many problems, such as that of the vibration of a circular disc, or the whirling speed of a shaft, and many aeronautical problems :

H. H. Jeffcott, The Periods of Lateral Vibrations of Loaded Shafts, *Proc. Roy. Soc.* **95**, A, 1918, 106–15.

J. J. Guest, Curved Beams, *Proc. Roy. Soc.* **95**, A, 1918, 1–21.

J. Prescott, The Buckling of Deep Beams, *Phil. Mag.* vi, **35**, 1918, 297–314.

In matters of more general interest in our subject it is useful to note the progress made in the application of mathematics to applied mathematical problems. Thus F. Hitchcock (*Phil. Mag.* vi, **35**, 1918, 461–71) continues his work on the Laplacian operator in connection with homogeneous functions. In a paper of mathematical interest, T. Kubota (*Tohôku Math. Journ.* **14**, 1918, 20–7) discusses the conditions that the centre of gravity of a convex curve shall coincide with that of any of the convex parallel curves. The investigation is extended to surfaces. A. Buhl (*Comptes Rendus*, **166**, 1918, 454–6) finds the moments of inertia of some layers of matter on a surface by means of a certain integral. P. Appell (*Comptes Rendus*, **166**, 1918, 513–16) discusses the means of representing the Newtonian mechanics for a system of particles or a rigid body. H. Vergue (*Bulletin des Sc. Math.* (2) **41**, 1918) completes his articles on the general equations of mechanics. K. Ogura (*Tohôku Math. Journ.* **13**, 1918, 172–204) gives a geometrical study of the mechanics of a particle from the standpoint of line geometry. R. Bricard (*Comptes Rendus*, **166**, 1918, 734–5), in a paper "Sur le mouvement à deux paramètres autour d'un point fixe," discusses the rigid dynamics of a body about a point or of one sphere on another fixed sphere. Of great interest is the paper by E. H. Neville, on moving axes with variable angles (*Quart. Journ. of Math.* xlviii. 1918, 136–41). An account of Neville's powerful method of treating problems involving moving axes in such subjects as dynamics and differential geometry was given at the Mathematical Congress in Cambridge in 1912. This new paper contains a brief statement of the ideas of the method and the formulation of the fundamental equations.

The following additional papers have been published recently:

Statics. E. V. Huntingdon, Bibliographical Note on the Use of the Word "Mass" in Current Textbooks, *Amer. Math. Monthly*, **25**, 1918, 1–15.

S. Brodetsky, The Elementary Theory of Statical Stability, *Math. Gaz.* ix. 1918, 233–6.

E. M. Horsburgh, An Approximate Formula for the Length of an Arc of a Suspended Rope, *Proc. Edin. Math. Soc.* xxxvi. 1917–18, 94–5.

B. de Fontviolant, several notes on the effect of a wind on a bridge, *Comptes Rendus*, **166**, 1918, *passim*.

A. W. Conway, On an Expansion of the Point Potential, *Proc. Roy. Soc.* **94**, A, 1918, 436–52.

R. F. Muirhead, Rolling Loads: A New Graphical Method, *Proc. Edin. Math. Soc.* xxxvi. 1917–18, 96–102.

R. F. GUYTHER, A Doctrine of Material Stresses, *Phil. Mag.* vi. **35**, 1918, 490–2.

D. N. MALLICK, Elastic Solids under Body Forces, *Phil. Mag.* vi. **35**, 1918, 321–6.

T. H. GRONWALL, Elastic Stresses in an Infinite Solid with a Spherical Cavity, *Ann. of Math.* ii. **19**, 1918, 295–6.

K. ULLER, Elastische Oberflachen-planwellen, *Ann. de Phys.* **56**, 1918, 463–96, in which the author concludes that there are three types of elastic surface waves: the pure shear wave, the pure expansion wave but so damped that the effect is only local, and a combination of both.

Dynamics. J. ARNOVLIEVITCH, Sur les théorèmes de projection et des moments des quantités de mouvement, *Nouv. Ann. de Math.* 1918, 139–41.

F. W. REED, On Integral Invariants, *Amer. Journ. of Math.* xl. 1918, 97–107.

C. MICHEL, Mouvements plans dans lesquels le tangente a une vitesse angulaire constante, *Nouv. Ann. de Math.* 1917.

K. OGURA, Determination of Central Forces acting on a Particle whose Equations of Motion Possess an Integral Quadratic in the Velocities, *Tohôku Math. Journ.* **14**, 1918, 155–60.

D. BUCHANAN, Orbits Asymptotic to an Isosceles Triangle Solution of the Problem of Three Bodies, *Proc. Lond. Math. Soc.* xvii. 1918, 54–74.

E. H. BARTON and H. M. BROWNING, Variably Coupled Vibrations, iii., *Phil. Mag.* vi. **35**, 1918, 36–47.

E. H. BARTON and H. M. BROWNING, Forced Vibrations Experimentally Illustrated, *Phil. Mag.* vi. **36**, 1918, 169–78.

A. EMCH, On Closed Curves described by a Spherical Pendulum, *Proc. Nat. Acad. of Sci. U.S.A.* 1918, 218–21.

E. L. REES, Concerning the Motion of a Rigid Body, *Amer. Math. Monthly*, 1918, 126–7.

F. REICHE, Zur Quantelung des asymmetrischen Kreisels, *Phys. Zeit.* 1918, 394–9.

E. E. TOURNAY HINDE, On a New Gyroscopic Phenomenon, *Proc. Roy. Soc.* **94**, A, 1918, 218–22.

P. STRANEO, Sur l'extension des principes de l'homogénéité et de la similitude et sur une remarquable rélation entre les constantes universelles d'une théorie, *Comptes Rendus*, **167**, 1918, 360–3.

G. TIERCY, Sur la fonction de résistance de la balistique, *L'Enseignement Math.* 1917, 309–13.

A. N. WHITEHEAD, Graphical Solution of High Angle Fire, *Proc. Roy. Soc.* **94**, A, 1918, 301–7.

M. Doyère, Remarques sur la résistance à la marche de navires géométriquement semblables, *Comptes Rendus*, **166**, 1918, 384–7.

C. CAILLER, Sur les trajectories d'une mobile soumise à une force centrale et à une résistance de milieu, *L'Enseignement Math.* xx. 1918, 93–6.

S. BRODETSKY, The Stability of the Parachute, *Tohôku Math. Journ.* **14**, 1918, 116–23.

Mechanics of Fluids. J. G. LEATHEM, On Two-dimensional Fluid Motion, with Free Stream Lines, past an Obstacle of Curved Outline, *Proc. Roy. Irish Acad.* xxxiv. A 2, 1918, 11–39.

T. BOUSSINESQ, several papers on the motion of fluids and semi-fluids, *Comptes Rendus*, **166**, 1918, *passim*.

T. R. PANNELL, The Wind Channel: Its Design and Use, *Aer. Journ.* xxii. 1918, 211–40.

W. R. D. SHAW, Computation of Aerodynamic Support (for circular arc camber), *Aer. Journ.* xxii. 1918, 109.

SIR G. GREENHILL, The Rankine Trochoidal Wave, *Proc. Roy. Soc.* **94**, A, 1918, 238–49.

T. H. HAVELOCK, Periodic Irrotational Waves of Finite Height, *Proc. Roy. Soc.* **95**, 1918, 38–50.

G. GREEN, On Ship Waves and on Waves on Deep Water Due to the Motion of Submerged Bodies, *Phil. Mag.* vi. **35**, 1918, 48-63.

H. JEFFREYS, Problems in Denudation, *Phil. Mag.* vi. **35**, 1918, 179-90, in which the author concludes that surface water motion is affected by gravity and friction, but only to a negligible extent by hydrostatic pressure and inertia.

ASTRONOMY. By H. SPENCER JONES, M.A., B.Sc., Royal Observatory, Greenwich.

The Present Position of the Nebular Hypothesis.—An article by Mr. J. H. Jeans bearing this title appeared in a recent issue of *Scientia,* in which are discussed the arguments for and against the famous nebular hypothesis of Laplace. It is remarkable that this hypothesis, put forward by Laplace more than a century ago to explain the origin of the solar system, has not yet definitely been either proved or disproved. It may be recalled that Laplace supposed a hot, nebulous mass of gas in rotation to be gradually cooling and shrinking, the velocity of rotation correspondingly increasing in accordance with the principle of conservation of momentum. The increase in the speed of rotation causes a flattening of the mass, and, after a certain stage, Laplace considered that a ring of matter would be thrown off from the equator which would break up and collect into a number of finite masses. A continuance of the shrinkage would result in these smaller masses similarly breaking up, and in this way he supposed that the existence of the planets and planetary satellites in the solar system could be accounted for.

To test this theory by mathematics is a matter of extreme difficulty, and the first attempts were limited to the study of the behaviour of a rotating, homogeneous and incompressible mass of matter. The evolution was traced by Darwin and Liapounoff to the well-known pear-shaped figure. Jeans has recently finally proved that the pear-shaped figure is unstable, and that the furrow rapidly deepens until the mass divides into two portions, which probably approximate to equality in mass. Each of these masses will itself be rotating and shrinking, and Russell has shown that the larger mass will break

up first, and that the distance apart of its two components will be small compared with that between the original pair. Some of the many binary and multiple systems in the sky appear to show these characteristics, and it is possible that they were formed in this way, although Jeans has recently concluded (*M.N.*, *R.A.S.* **79**, April 1919), from a statistical study of the distribution of eccentricities and periods of binary systems, that the probability is against these systems having been so formed.

The conditions postulated in these researches are doubtless very far removed from reality, and they do not therefore provide a criterion as to the validity or otherwise of the nebular hypothesis. In 1855 Roche discussed the other extreme limiting case in which the matter is so compressible that it is practically concentrated close to its centre of gravity, the outer parts being merely of the nature of an "atmosphere" of small density. In this case it is found that, after a time, a lens-shaped figure is formed, which develops a sharp edge round its equator, from which, after a certain critical velocity has been attained, any further increase in velocity results in the ejection of matter from the periphery. The behaviour of such a mass is therefore very similar to what Laplace predicted, but again we are faced with the fact that the case is an extreme one.

Jeans has now shown, in some brilliant mathematical investigations, that in intermediate cases a compressible mass will behave similarly to Roche's model, provided that its density is less than about a quarter of that of water, but that above this limiting density there is a sudden change, and the behaviour approximates to that of an incompressible mass. He concludes, therefore, that young masses in rotation break up in the way pictured by Laplace, and old masses in the way pictured by Darwin.

In this division into young and old masses, the majority of multiple systems observed in the sky would fall into the latter category, having masses above a quarter of that of water. Their behaviour is therefore in accordance with Jeans's conclusions.

Jeans considers that the other class—rotating bodies of small density throwing off matter from their equators—are represented by the spiral nebulæ. Photographs of spiral nebulæ which are edgewise-on to us show that the central

nucleus is actually lenticular in shape, and the rotation of certain spiral nebulæ has recently been conclusively established by Slipher. Moreover, such evidence as is available supports the view that the matter in the arms is in motion outwards from the nucleus and not inwards to it. The matter is in all cases thrown off from two opposite points on the periphery, and not in the form of a ring, as conjectured by Laplace. This could be accounted for by supposing the existence of tidal forces from neighbouring masses, a comparatively small force only being required to cause the ejection of matter when the mass reaches its critical state of equilibrium. Thus the matter will be ejected in two symmetrical filaments. The characteristic equiangular spiral shape of the arms cannot at present be accounted for in a fully satisfactory manner.

Jeans further finds that the arms will become unstable and break up into several masses if they are on a sufficiently massive scale, but that if their mass is small the gravitational attraction is not sufficient to enable them to cohere together, and they will diffuse away into space. In the former case, the nuclei will form at approximately regular intervals, and such nuclei are actually found in the arms of most spiral nebulæ. The order of distance apart of these nuclei can be calculated, and also their masses. Jeans finds that the masses will be comparable with the mass of our sun, so that it is very improbable that the planets of the solar system could have been formed in this way. If a nebula of mass as small as our sun had thrown off arms they would have dissipated into space.

The work of Jeans has therefore considerably advanced our knowledge of the nebular hypothesis, and it appears probable that while the sequence of events pictured by Laplace may have happened in the case of the spiral nebulæ, they could not have happened in the case of the solar system, to explain the existence of which the hypothesis was formulated.

The Application of Photoelectric Photometry to Astronomy.—Although astronomy is the oldest of the sciences, it is nevertheless true that many of the principal advances which have been made in it of late years have been due to the application of new methods. It is only necessary to instance the application of photography, which has made possible the rapid determination, amongst other things, of the positions of faint stars,

of photographic magnitudes, of types of stellar spectra, of radial velocities, which has led to the discovery of numerous variable and binary stars, and which has enabled stellar parallaxes to be determined with ease and accuracy. It seems probable that a new step forward will result from the application of photoelectric phenomena. The principles of photoelectric photometry have already been used by Guthnick in Germany and by Stebbins in America for the study of variable stars whose range of brightness is very small. A paper by A. F. and F. A. Lindemann in the March number of the *Monthly Notices, R.A.S.*, giving an account of the various possible applications of the new method as well as of details of the necessary apparatus should, therefore, be read with interest by all who are interested in the matter. It is worth while remarking that, whilst many of the most recent astronomical developments have been limited in their application to telescopes of large aperture and having lenses or mirrors with the highest perfection of figure, this is not the case with the new method; it does not necessitate a very good lens or mirror, nor even a very accurate driving clock, and although large apertures are advantageous, as they enable observations to be made more rapidly, its use is not restricted to them. It is, therefore, eminently suitable for application by amateur astronomers possessing telescopes with apertures of, say, 6 inches and upwards.

The principle of the method is that light falling upon a metal surface causes it to emit electrons whose energy is proportional to the difference between the frequency of the light and a frequency characteristic of the metal. With most metals the latter is large, and the current obtained is weak, but with the alkali metals a well-marked selective effect is obtained in the region of the visual spectrum. The important property of the phenomena is that the emission current is proportional simply to the amount of the incident light and is not dependent upon its intensity per unit surface. The practical application consists in placing the photoelectric metal close behind the focus of the telescope, so that the light from the object under observation falls upon the metal. The number of electrons emitted is measured, and the ratio of this number to the number emitted under the influence of a standard light determines the energy received.

For astronomical applications it is necessary to use the

alkali metals, potassium, rubidium and cæsium. These are deposited on the walls of special glass cells containing a rare gas. The method of construction of the cells is described in the paper. Accurate methods of measurement are essential, and for this purpose the authors constructed a specially sensitive string galvanometer. A single observation can be made in about four or five minutes, each observation consisting of a reading of the electrometer deflection when the telescope is set upon the stellar object, followed by a reading when a standard voltage is switched on.

The practical applications suggested in the paper are numerous. (1) The determination of stellar magnitudes is first referred to: the work of Guthnick and Stebbins has already shown that a very high order of accuracy is obtainable, and that the variability of stars whose range of brightness does not amount to more than a few hundredths of a magnitude can be established with certainty. Stebbins has found the new method considerably preferable to the selenium photometer for this purpose, and has abandoned the latter in favour of photoelectric photometry. (2) The determination of stellar temperatures is possible provided that a potassium and a cæsium cell are used alternately. These have maximum sensitiveness at wave-lengths of 4,400 and 5,500 tenth-metres respectively, and so the energies corresponding to two different wave-lengths can be deduced. From this, the temperature can be calculated on the assumption that Planck's radiation law is obeyed. An investigation of the theory of the method indicates that an error of 1 per cent. in the measurement leads to an error of only about 100° in the temperature. (3) One of the most important features of the method is that it enables an accurate measurement to be made of the light received from extended surfaces such as nebulæ or comets. Thus the equivalent stellar magnitude of a nebula can be determined, and, provided that its distance can be estimated, an approximation can be made to the number of luminous stars which it contains. The temperature of nebulæ can also be determined. A knowledge of this is of importance on account of its bearing upon the question of stellar evolution, for, if the temperatures should be found to be approximately the same, it would indicate that the life of the galaxy is great compared with the light of a single star. Somewhat similar

applications of the method to stellar clusters may be made. (4) Another important application is the photometry of comets. If sufficiently accurate measurements can be made, it will be possible to determine whether the effect of radiation pressure is sufficient to cause any deviation from the inverse square law and also to determine comets' masses. (5) The accurate photometry of the planets relative to fixed stars will enable any variation in the sun's brightness to be determined. Hence the question as to whether or not the sun is a variable star could be cleared up. In order to distinguish between solar variability and a change of the planet's albedo it would be necessary to observe regularly at least two planets. (6) By making measurements on a part of the moon's surface not directly illuminated by the sun, the albedo of the earth could be determined, which is of considerable importance for meteorology. (7) Many solar phenomena lend themselves to photometry by this method, such as the brightness of the corona or of solar protuberances and faculæ.

Various other possible applications are touched upon in the paper, but the above will suffice to indicate that the method is one possessing great possibilities, and which should provide very valuable information on many important matters. Unfortunately, it is not at present possible to obtain the cells or a sufficiently sensitive electrometer commercially in this country, and until this drawback is remedied the use of the method is likely to be considerably restricted.

The following is a selection of the more important papers which have recently appeared:

Historical. FOTHERINGHAM, J. K., The New Star of Hipparchus and the Dates of Birth and Accession of Mithridates, *M N., R.A.S.* **79**, 162, 1919.

DREYER, J. L. E., The Place of Tycho Brahe in the History of Astronomy, *Scientia*, **25**, March 1919.

Dynamical Astronomy. BLOCK, H. G., Sur une équation différentielle du problème de la rotation des corps célestes, *Ark. för Mat. Ast. och Fys.* **14**, No. 3, 1918.

SPAČEK, V., Über den Einfluss der Krümmung der Erdoberflache und der Schwerkraftsänderungen auf die Pendelbewegung, *Ast. Nach.* No. 4974, 1918.

BOSLER, J., L'Influence des météorites sur l'excentricité de l'orbite terrestre, *Bull. Astr.* **35**, 242, 1919.

STROOBANT, P., La Constitution de l'anneau des petites planètes (2me partie). *Ann. de l'Observatoire royal de Belgique*, **14**, 1918.

LAGRULA, J., Procédé de calcul rapide de l'orientation du grande cercle de recherche des asteroids, *Bull. Astr.* **35**, 228, 1919.

CHAZY, J., Sur certaines trajectoires du problème des *n* corps, *Bull. Astr.* **35**, 321, 1919.

JEANS, J. H., The Configuration of Rotating Compressible Masses, *Phil. Trans.*, ser. A., **218**, 157, 1919.

PALMER, MARGARETTA, The Definitive Orbit of Comet 1786. II, *Astron. Journ.* **31**, No. 744, 1918.

Variable and New Stars. PICKERING, E. C., Maxima in 1919 of Variable Stars of Long Period, *Harv. Circ.* 212.

EDDINGTON, A. S., On the Pulsations of a Gaseous Star and the Problem of the Cepheid Variables, Pt. I., *M.N., R.A.S.* **79**, 1, 1918; Pt. II., *ibid.* **79**, 177, 1919.

DAWSON, B. H., The Period of 004872 V Tucanæ, *Astroph. Journ.* **48**, 310, 1918.

MARTIN, C., and PLUMMER, H. C., The Short Period Variable R R Ceti, *M.N., R.A.S.* **79**, 190, 1919.

TURNER, H. H., Note on the Nebulosity round Nova Persei, *M.N., R.A.S.* **79**, 23, 1918.

NEWALL, H. F., On the Spectrum of Nova Aquila, 1918, *M.N., R.A.S.* **79**, 31, 1918.

Stellar Distributions and Motions, etc. CHARLIER, C. V. L., Studies in Stellar Statistics, IV. Stellar Clusters and Related Celestial Phenomena, *Lund Medd.* Ser. II., No. 19, 1918.

GYLLENBERG, W , On the Motion and Distribution of the Long Period Variable Stars, *Ark. för Mat. Ast. och Fys.* **14**, No. 5, 1918.

SHAPLEY, H., Studies based on the Magnitudes in Stellar Clusters. 8th paper, On the Luminosities and Distances of 139 Cepheid Variables, *Astroph Journ.* **48**, 279, 1918 9th paper, Three Notes on Cepheid Variation, *Ibid.* **49**, 24, 1919. 10th paper, A Critical Magnitude in the Sequence of Stellar Luminosities, *Ibid* **49**, 96, 1919.

PERRINE, C. D., On the Cause of the Distance Velocity Equation in Stellar Motions, II, *Astroph Journ.* **48**, 295, 1918.

HERTZSPRUNG, E., Bemerkungen zur Statistik der Sternparallax, *Ast. Nach.* No. 4975, 1919.

JEANS, J. H , The Evolution of Binary Systems, *M.N., R.A.S.* **79**, 100, 1918.

BERGSTRAND, Ö., Sur le groupe des étoiles à hélium dans la constellation d'Orion, *Nova Act. Reg. Soc. Scient. Upsala*, Ser. IV., **5**, No. 2, 1919.

DYSON, F. W., and THACKERAY, W. G., Proper Motions of the Stars in Zone 24°–32° N. Dec. in Relation to Spectral Types, *M.N., R.A.S.* **79**, 201, 1919.

JEANS, J. H., The Present Position of the Nebular Hypothesis, *Scientia*, **24**, 1918, Oct.

SCHOUTEN, W. J. A., The Parallax of Some Stellar Clusters, *Observatory*, **42**, 112, 1919. The Distribution of the Absolute Magnitudes among the Stars in and about the Milky Way, *Proc. Amst. Acad. Sci.* **21**, 518, 1918.

BOSS, L., The Vertex of Stellar Motions, *Astroph. Journ.* **32**, No 747, 1919.

Miscellaneous. GYLLENBERG, W., The Theory of the Equatorial, *Ark. för Mat. Ast. och Fys* **13**, No. 19, 1918. Die bestimmung der Einstellungsfehler eines Aquatorials durch Differential-messungen, *Ibid.* **13**, No. 9, 1918. Sur la réduction des clichés astrophotographiques et la conversion des mesures en A. R. et D, *Lund Medd.* II. No. 18, 1918.

BAILLAUD, B., Sur les relations entre les co-ordonnées rectilignes normale d'une même étoile sur deux clichés différents, *M.N., R.A.S.* **79**, 151, 1918.

COURVOISIER, L., Über Saalrefraction und ihre Wirkung auf das Deklinations-system, *Ast. Nach.* **207**, No. 4966, 1918.

LUNT, J., The Radial Velocities of 119 stars observed at the Cape, *Astroph. Journ.* **48**, 261, 1918.

CAMPBELL, W. W., The Total Solar Eclipse of June 8, 1918, *Lick Obs. Bull.* No. 317, 1918.

BERGSTRAND, Ö., Études sur la distribution de la lumière dans la couronne solaire. (Stockholm, Almqvist & Wiksells, 1919.)

BARNARD, E. E., On the Dark Markings of the Sky, with a Catalogue of 182 such Objects, *Astroph. Journ.* **49**, 1, 1919.

BIGOURDAN, M. G., Projet de réforme du calendrier civil actuel, *Comptes Rendus*, **168**, 21, 1919.

CHAPMAN, S., The Energy of Magnetic Storms, *M.N., R.A.S.* **79**, 70, 1918.

CONRADY, A. E., The Five Aberrations of Lens Systems, *M.N., R.A.S.* **79**, 31, 1918.

PHYSICS. By JAMES RICE, M.A., University, Liverpool.

Atom and Electron Structure.—The particular shape of the electron, or fundamental unit of negative electricity, and the distribution of the charge throughout the volume occupied by it, are matters which may be overlooked in the discussion of many problems, where it is sufficiently accurate to regard the electron as a "point charge." But, for the closer discussion of those problems of atomic structure which are agitating the minds of physicists to-day, no such simple view of electronic structure is adequate. Discussion has, perhaps naturally, centred most closely around an electron assumed to be spherical or spheroidal in shape. In particular, Lorentz has made powerful use of the concept of an electron which, having a spherical shape when at rest, suffers when in motion a contraction in length in the direction parallel to its velocity and of an amount $1 : \sqrt{1-v^2/c^2}$, where v is the electron's velocity and c is the velocity of light. In fact, this assumption is but the logical extension to these minute corpuscles of the Lorentz-Fitzgerald contraction of matter in bulk—the hypothesis by which those two physicists endeavoured to explain the failure of the Michelson-Morley (and other) experiments to detect the earth's motion through the ether. This "contractile" electron leads to a variation of mass with velocity which is in extremely good agreement with the most trustworthy experiments on β particles from radioactive materials and high-speed cathode particles.

As is well known, however, serious difficulty is being experienced to present a theory of *atom* structure which shall

embrace all the known results of such phenomena as chemical combination and valency, molecular cohesion, radiation, diamagnetism and paramagnetism, ionisation, Zeeman effect, series laws of spectral lines. The earlier view of Thomson as to the atom consisting of a "sphere of positive electricity," with coplanar rings of electrons embedded in it, and in motion around the centre, has gradually given way to the "nucleus" theory of Rutherford, in which the positive charge which serves to bind the electrons in their orbits is no longer diffused through the whole volume "occupied" by the atom, but is concentrated within a very small portion of its volume at the centre, having in fact a size which must be smaller than that of the electron itself if we are to account for the whole mass of the atom on purely electromagnetic grounds. This view, in one form or another, holds the field to-day, and, despite its manifest difficulties, it has had a remarkable success. For instance, it explains very readily the scattering of α rays which is experienced when these particles are passing through gases or thin foils of metal, a fact with which it is difficult to reconcile the Thomson theory of atom structure. The most serious obstacle to its acceptance is the instability of the electrons in their orbits around a minute centre of force attracting as the inverse square law. For one thing, any sort of stability in the orbits is impossible at all unless the orbits are coplanar, and it is difficult to see how even these would survive some of the shocks to which every atom must be exposed even in its ordinary thermal encounters with other atoms. Added to this is another difficulty. All electrons in orbital motion are being accelerated, and such acceleration is accompanied by radiation of the electron's energy leading gradually to contraction of the orbit and ultimate coalescence with the atomic nucleus—a state of affairs quite irreconcilable with the constant properties of atoms as we meet them in physical and chemical experiments. Yet orbital motion is a postulate we cannot abandon without at the same time encountering just as apparently insurmountable difficulties in explaining the magnetic properties of matter, the Zeeman effect, etc. Of course, all these difficulties arise in the application of the classical Newtonian dynamical principles to atomic models as pictured above. The Newtonian dynamics has had great success among those phenomena where the velocities of moving

matter are small compared with that of light, but on several grounds it has recently been borne in on the mind of the physicist that it cannot be accepted as more than an approximation in cases where the velocities are large, and that in consequence the motions of cathode particles, radioactive particles, and electrons within the atom, where the speed is comparable with that of light are subject to a wider body of dynamical laws than those propounded by Newton. Such an admission, while not removing the difficulties mentioned above, does, at least, encourage the hope of resolving them by methods of attack unthought of until recent years. For example, Bohr in 1913, by applying the Quantum hypothesis of Planck to the Rutherford model, scored a remarkable initial success in accounting for the spectral series of hydrogen and helium, and in giving a rough, general explanation of the Rydberg law for the series of lines in the spectra of other elements. Considerable work has been carried on in connection with Bohr's theory during the years of the war, especially directed towards the high-frequency or X-ray spectra of the elements, and I understand that Bohr himself is shortly to publish the results of a prolonged research which, it is stated, has extended very considerably the success of its earliest applications. Of course, it must be borne in mind that all this success depends on the application of the Quantum hypothesis. Now this hypothesis is not a system of dynamics. Its success in explaining the nature of "Full" Radiation, the relation between specific heat and temperature, the photoelectric law, etc., may be reasonably held to support the view that it will one day be embraced within the scope of our future laws of dynamics as an important deduction from them—as important, perhaps, as the principle of energy is to-day with respect to the Newtonian laws from which it is deducible.

That being the state of affairs, considerable interest still attaches to all attempts to explain atomic structure on the older and more orthodox views, and it may be of interest to readers of Science Progress to become aware of an attempt recently made by A. L. Parson to overcome difficulties of *atomic* structure by postulating a new structure for the *electron*. His paper entitled "A Magneton Theory of the Structure of the Atom" was published in the *Smithsonian Collections*, vol. lxv. No. 11 (1916). On grounds which cannot be entered

into here it is perhaps desirable that the name "magneton" should not be applied in this connection, and that "A Ring Electron Theory, etc.," would describe the author's views in more accurate terms. For that is his essential postulate, viz. that the electron itself is magnetic, having, in addition to the usual properties attributed to negative electricity, the properties of a current circuit whose radius is of the same order of magnitude as, but somewhat smaller than, that of a hydrogen atom. "It may be pictured by supposing that the fundamental negative charge is distributed continuously around a ring which rotates on its axis with a peripheral velocity of the order of that of light, and presumably the ring is extremely thin."

Parson himself adopts in his paper the Thomson positive sphere for the massive part of the atom, but it appears that the conception may be associated with the Rutherford nucleus as readily. It is in its explanation of the chemical and magnetic behaviour of matter that this model has its most signal success. As mentioned above, orbital motion of electricity within the atom seems essential for any explanation of the magnetic properties of matter, yet such orbital motion implies radiation of energy, and ultimate collapse of the atom if the electricity is regarded as in the form of minute spherical or spheroidal charges rotating in orbits large in comparison with the size of these charges. It has long been known that if the number of electrons in motion in one orbit were great enough, the radiation would be much reduced, even six making the loss of energy extremely slow. But there are serious objections to the employment of this result to help us out of the radiation difficulty. The lighter atoms, such as those of Hydrogen, Helium, Lithium, do not contain enough electrons to form even one such orbit—at least enough of the outer and more loosely bound electrons which play a part in chemical, magnetic and optical phenomena. Again, for diamagnetic atoms it is necessary to assume the existence of independent orbits in the atom that are so great in number or else undergo such rapid variations that they can be considered as having their axes distributed uniformly in three dimensions in order to account for a zero resultant magnetic moment. Separate rings of this sort cannot maintain their individualities unless they differ very markedly in radius, and this condition would

limit too seriously the possible number of rings and the chance of their resultant moment being zero (most substances are diamagnetic); while, if their radii are not very different from one another, an altogether chaotic motion would result quite inconsistent with the precise and definite properties which the atom of each element possesses. This means, for stability, coplanar rings—a state of affairs quite inadequate to account for the facts of stereochemistry.

Further, on Parson's view purely electrostatic forces of attraction and repulsion between the individual electrons and the positive parts of the atom are insufficient to account for valencies. With his electron shaped as a rapidly rotating ring, he can appeal as well to electromagnetic forces with a magnitude comparable under certain circumstances with the electrostatic. Thus, it is simple enough on electrostatic grounds to explain the bond between a hydrogen atom and a chlorine atom in the molecule of hydrochloric acid, but not so easy to explain the bond between the two hydrogen atoms in the hydrogen molecule. This, however, is rendered more easy if the electromagnetic attraction of two similar electronic orbits is brought into consideration.

By his ring structure hypothesis, Parson gets rid of two difficulties *ab initio*—the difficulty of stability and that of energy waste by radiation. His hypothesis is frankly designed for this purpose. Its merits must be judged by its subsequent success in dealing with all the other difficulties which have met the other theories and all the problems which have been raised by the multitude of experimental results available. As regards chemical considerations, no one will deny that the introduction of the ring electron furnishes a means of avoiding many serious difficulties which are met when the attempt is made to apply earlier conceptions of the electron to problems of chemical constitution and stereochemistry. In particular, Parson has shown that a group of eight of his ring electrons forms an extremely stable system of low magnetic energy. They have their centres on the corners of a cube, and wide limits of orientation are possible for the rings without upsetting the stability of the group. Now this compact group of eight is just what is required to account for the presence of eight groups in the periodic table, and a considerable portion of Parson's paper is devoted to a close discussion of the periodic

table from this point of view, the hypothesis being that corresponding members of different "periods" in the table differ by the numbers of the stable groups of eight electrons contained in their atoms. Success in explaining certain magnetic properties of matter is also more or less assured from the beginning by the nature of the hypothesis.

However, it seems to be the view of most competent physicists at present that in the directions other than those in which success might reasonably be assumed, Parson's hypothesis has proved somewhat disappointing and unfertile. In the February number of the *Proceedings of the Physical Society* there appears a very able résumé by Dr. H. S. Allen of the evidence in favour of the ring electron theory, but the discussion following Dr. Allen's paper cannot be said to have been very favourable to the hypothesis. According to Dr. Allen the ring electron (with some modifications of Parson's original idea) can account for the facts of diamagnetism and paramagnetism; involves no loss of energy in its rotatory motion; explains certain facts concerning asymmetry of some types of radiation, the effect of magnetisation of iron on its absorption coefficient of X-rays, the small amount of ionisation of gases due to X-rays; it can be adapted to the Quantum hypothesis, and Bohr's theory can be restated so as to apply to it; the Zeeman effect can be brought within its compass; and the problems of chemical constitution and stereochemistry may be solved by it, in particular, *stationary* valence electrons being possible (a great desideratum). As stated above, the discussion by other members of the Physical Society hardly supported Dr. Allen in his claims. The general trend of the discussion may be best illustrated by a few quotations from it.

"Judged by its power of predicting new phenomena, the hypothesis of the ring electron is disappointing," and "in striking contrast is the fertility of Bohr's hypothesis of the atomicity of angular momentum." "The value of a model such as the ring electron will be judged by the number and importance of the facts that it accounts for, and by the ability it shows to predict undiscovered phenomena, or to suggest new lines of research. The ring electron does not appear to have been especially fortunate in this latter respect; and its claim to consideration must perhaps rest mainly on arguments such

as those marshalled by Dr. Allen. In considering a claim of this kind it must always be taken for granted that the proposed model will explain at least one or two facts perfectly. The model has been devised *ad hoc.* What we have to examine is whether it explains other extraneous facts, and particularly whether it does so without destroying more of the old fabric of science than it builds of new. . . . It appears that the ring electron may be welcomed mainly as giving a vivid picture of certain magnetic phenomena. There seems to be no clearly established case in which it successfully explains any phenomenon outside magnetism, and it is quite out of the question to suppose that the ring electron is going to reconcile the classical dynamics with phenomena which are demonstrably inconsistent with the classical dynamics."

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Electrolytic Dissociation and the Law of Mass Action.—In an earlier report (Science Progress, April 1916) attention was drawn to an investigation by Schlesinger and Coleman in which the degree of ionisation of certain metallic formates dissolved in anhydrous formic acid was determined. The salts employed were of the univalent type. Anhydrous formic acid is a strongly dissociating solvent, and these electrolytes are largely ionised. Nevertheless the law of mass action, the "Dilution Law," is obeyed with a high degree of accuracy, the extent of ionisation being determined as usual by the electrical conductivity method.

Recently Schlesinger and Mullinix (*J. Amer. Chem. Soc.* 1919, **41**, 72) have extended the investigation to the formates of the divalent metals, calcium and strontium. Again, the degree of ionisation is large. The concentration range examined extended from 0·04 to 0·4 gram-equivalents per litre, It is now found that whilst the law of mass action is obeyed over a considerable range—from 0·1 to 0·3 gram-equivalents per litre—the law no longer applies to regions above and below these limits. The curve expressing the law consists of three portions. The chief interest attaches to the portion which refers to the most dilute region, for according to these authors the curve shows a sharp break and not a gradual transition in the region of 0·1 normal. It is suggested that intermediate

ions are formed, a suggestion which in itself is by no means improbable; but, as the authors themselves point out, this is not a complete explanation of the behaviour observed, for on this basis a gradual transition from one curve to the other would be expected. The problem, therefore, of the mechanism of ionisation in this solvent has still to be solved.

The Polymerisation of Water.—It is now generally recognised that water in the liquid state consists of three kinds of molecules, monhydrol (H_2O), dihydrol (H_2O_2), and trihydrol $(H_2O)_3$, in different proportions in equilibrium with one another. The relative amounts of the three kinds vary with the temperature, the monhydrol increasing with rising temperature at the expense of the trihydrol. (Ice consists practically entirely of trihydrol; steam of monhydrol). These conclusions have been reached as a result of the examination of several characteristic properties of water itself and of aqueous solutions and the variation of these properties with temperature.

The problem has been reinvestigated by T. W. Richards and Palitszch (*J. Amer. Chem. Soc.* 1919, **41**, 59) from the point of view of the compressibility of aqueous solutions, especially of urethane in water. This substance was chosen because of the fact that it is neither associated nor appreciably dissociated in aqueous solution. It has the property of lowering the surface tension of water, a fact of importance in view of the possible interrelation between surface tension and compressibility. The compressibility of the solutions was determined as a function of the concentration, measurements of density, viscosity, and surface tension being carried out at the same time.

Pure water at 20° C. has a compressibility of $43{\cdot}25 \times 10^{-6}$. It was found that the compressibility of the solutions at first diminishes, as a result of adding urethane, to a minimum value of about $38{\cdot}9 \times 10^{-6}$, and then rises as the concentration increases. On the other hand, the surface tension and the viscosity show no minimum in their course. There is also no minimum in the curve expressing the solution volume—a quantity which is computed from the difference between the volume of the solution and of the water, reckoned per gram of urethane.

The first point to be considered is the minimum exhibited

by the compressibility curve. The descending portion must be ascribed to a change in the nature of the water itself, for the comparatively small amount of urethane present can scarcely be effective. Now it is known that the compressibility of water diminishes as the temperature rises, a fact which has been attributed to the breaking down of the trihydrol molecules into simpler forms. It is reasonable, therefore, to suppose that urethane—and in fact any solute whatsoever—possesses the property of dissociating the trihydrol, and consequently diminishing the compressibility as an initial effect. The observation that the position of the well-known minimum volume of water can be shifted towards lower temperatures as the pressure increases is likewise due to the changing molecular complexity of water. The decomposition of the trihydrol also affords an explanation of the generalisation made several years ago by Tammann, namely, that the volume of a solution varies with the temperature in the same sense as the volume of the pure solvent under a higher pressure.

The ascending portion of the compressibility curve in the case of urethane is easily attributable to the fact that urethane itself is more compressible than dihydrol, and consequently when the concentration of the urethane becomes sufficiently great the compressibility of the mixture as a whole increases.

As regards the solution volume curve, Richards points out that its form "is precisely what one would expect if water contained an appreciable amount of trihydrol in dilute solutions and an appreciable amount of monhydrol in concentrated solutions with dihydrol present throughout." This is the conclusion arrived at by Bousfield and Lowry in 1910.

Next, in connection with the surface tension curve it is found that at first the falling off is very marked, and that later, in solutions containing 40 per cent. or more of urethane—from which presumably the trihydrol has been eliminated—the surface tension decreases much less rapidly but more regularly. Here, again, the two markedly different branches of the curve must be ascribed to the changing constitution of water. When the region, 40 per cent. – 50 per cent. urethane is reached—at which point the solvent has become relatively simple in constitution—Richards finds that the surface tension γ and the compressibility β are related to one another by

the expression $\beta \gamma^{4/3} =$ constant, a relationship which he had already found to hold good for pure substances.

Finally, as regards the viscosity curve, it is evident that urethane itself possesses a much greater viscosity even than trihydrol, so that, from the first, the addition of the solute causes an increase in this property which more than compensates for the diminution which would be brought about by the dissociation of the trihydrol. The dissociation of the trihydrol is shown, however, by the bending of the curve at its lower end. That the above assumption is not unreasonable is shown by the enormous increase in viscosity, from 100 to nearly 250, as we pass from pure water to a 50 per cent. solution of urethane.

The interest of this investigation lies in the fact that several distinct properties which at first sight appear to have little in common can all be related to one another on the basis of the single assumption that the presence of a solute occasions the dissociation of trihydrol into simpler molecular forms.

The Tri-atomic Hydrogen Molecule.—As is well known, Sir J. J. Thomson first discovered evidence for the neutral molecule H_3 as a result of investigation by means of positive rays. More recently Duane and Wendt (*Phys. Rev.* 1917, **10**, 116) have obtained further evidence for the existence of this polymer of hydrogen.

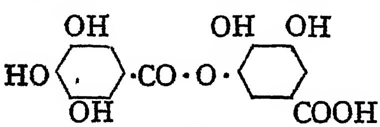
Bohr (*Medd. K. Vet. Akad. Nobelinstitut*, 1919, **5**, No. 28) has studied the stability of the molecule H on the basis of his theory of atomic structure, and has shown that such a structure is quite capable of permanent existence. The configuration of the molecule is shown to be such that the three electrons rotate at equal angular intervals in a common circular orbit, while the three nuclei are placed on the axis of this orbit, one at the centre of the circle and the two others equidistant from the central one, the three nuclei being in the same straight line. Bohr shows, further, that a molecule possessing this structure has the capacity of binding a free electron and still giving rise to a stable system. In this configuration the three nuclei are, as before, in the same straight line, while the electrons are distributed in two distinct circular orbits of equal radii, each orbit containing two electrons, and each orbit being situated around each of the terminal nuclei.

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School, London.

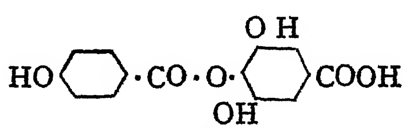
The synthesis of two pentadigalloyl glucoses by Fischer and Bergmann (*Berichte*, 1918, **51**, 1760) marks a most important advance towards the elucidation of the constitution of the tannins. Previous to the year 1912, ordinary oak bark tannin had been regarded as being merely the anhydride formed by the abstraction of one molecule of water from two molecules of gallic acid :

$$2C_6H_2(OH)_3COOH - H_2O = C_{14}H_{10}O_9.$$

Nobody seemed to doubt the accuracy of this assumption, and all investigations concerning the chemistry of tannic acid were directed towards (*a*) establishing the exact mode of union between the two molecules and (*b*) suggesting a formula which would account for the optical activity of the compound. The fact, however, that natural tannin was always associated with a certain small percentage of glucose led Fischer to suggest that tannin was not merely the simple anhydride of gallic acid, but that it was in reality some kind of compound of this substance with glucose. With the object of testing this view he synthesised (Fischer and Frendenberg, *Berichte*, 1912, **45**, 915) a pentagalloyl glucose which, although a derivative of gallic acid, exhibited a remarkable similarity with natural tannic in its physical and chemical properties. Since then Fischer has made repeated attempts to synthesise the two isomeric *m* and *p* digallic acids (I) and (II)



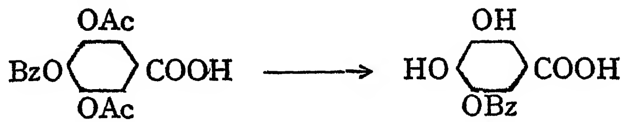
(I)



(II)

with the object of combining them with glucose, but found that, no matter what method was employed for synthesising these two acids, only the meta acid was obtained; owing to the fact that molecular rearrangement took place whenever the acyl derivatives of the para acid were hydrolysed. Thus, for example (Fischer, Bergmann and Lipschitz, *Berichte*, 1918, **51**, 45), 4 benzoyl 3 : 5 diacetylgallic acid (III) on hydrolysis yielded 3 benzoyl protocatechuic acid (IV), in other words, the

meta benzoyl derivative of gallic acid instead of the para-benzoyl derivative



(III) (IV)

and similarly penta-acetyl-*p*-digallic acid on hydrolysis gave *m*-digallic acid. The penta-acetyl derivatives of *m*- and *p*-digallic acids are, however, stable, and from their acid chlorides it has been found possible to synthesise the two corresponding penta-(penta-acetyl digalloyl)-glucoses, and to obtain from these by careful hydrolysis the two penta-(digalloyl)-glucoses. Of these the penta-(*m*-digalloyl)-β glucose is found to be remarkably similar to Chinese tannin and to differ from it only in regard to its specific rotation in aqueous solution ; this difference, however, need not be regarded as serious in view of the fact that the solutions are colloidal and are liable to be considerably influenced by small factors. The same authors have also effected the synthesis of 1-galloylglucose of the formula

$$C_6H_2(OH)_3 \cdot CO \cdot O \cdot \underbrace{CH \cdot (CHOH)_2 \cdot CH}_{O} \cdot CHOH \cdot CH_2OH$$

which is found to be identical in all respects with the natural product glucogallin first isolated from Chinese rhubarb by Gilson in 1903 (*Comptes Rend.* **136**, 385).

An entirely novel method for isolating the products of the acid hydrolysis of proteins has been devised by Dakin (*Biochem. J.* 1918, **12**, 290). Caseinogen was hydrolysed by means of sulphuric acid, the excess of acid being removed as barium sulphate. The neutral concentrated solution was then submitted to continuous extraction by means of butyl alcohol, whereby five fractions were obtained. (1) Monoamino acids which, although insoluble in butyl alcohol, are nevertheless extracted and are deposited as a cream-coloured powder in the extraction flask ; their extraction is due to the passage of a certain amount of water into the butyl alcohol since the extraction is not satisfactory if the aqueous solution of the amino acids contains an excess of salts, such as calcium chloride. (2) Proline, which is soluble in ethyl alcohol, and is also extracted

by butyl alcohol. (3) Peptide anhydrides or diketopiperazines, also extracted by butyl alcohol but separable from (2) by the fact that they are insoluble in alcohol, whereas proline is soluble. (4) Dicarboxylic acids not extracted by butyl alcohol; and (5) Diamino acids, likewise not extracted by butyl alcohol but separable from dicarboxylic acids by phosphotungstic acid and other methods. Considerably higher yields of many of the amino acids were obtained by this method, as much as 8 per cent. of proline being obtained from caseinogen. Moreover, it has enabled the author to isolate two new cleavage products from caseinogen, namely, β-hydroxy glutamic acid ($COOH\,CH\,(NH_2){\cdot}CHOH{\cdot}CH_2COOH$) and *d*-iso-leucyl-*d*-valine anhydride, a peptide of the formula

$$(CH_3)_2{\cdot}CH{\cdot}CH\left\langle\begin{matrix}CO{\cdot}NH\\NH{\cdot}CO\end{matrix}\right\rangle CH{\cdot}CH\left\langle\begin{matrix}CH_3\\C_2H_5.\end{matrix}\right.$$

It is possible by this method to obtain a dry, almost neutral amino-acid mixture which might serve as a basis for nutrient media, with or without the addition of tryptophan, and might even be employed for dietetic purposes owing to the absence from the mixture of most of those acids which give rise to glucose in the diabetic organism.

Boswell and Dickson (*J. Amer. Chem. Soc.* 1918, **40**, 1779) have made the interesting observation that fused sodium oxide is an active agent for effecting oxidations; thus, for example, carbon monoxide in contact with fused sodium hydroxide at 410–30° is oxidised to carbon dioxide, an equivalent quantity of hydrogen being set free at the same time ; similarly, sodium formate and sodium oxalate are oxidised almost quantitatively to carbon dioxide with evolution of hydrogen at temperatures of 275 and 290° respectively. This action is explained by assuming that it is really the water present in the fusion which acts as the oxidising agent, the caustic soda serving to break up the water catalytically. It is also suggested that the general reaction by which carboxyl is replaced by hydrogen on fusion with caustic soda—as, for example, the production of benzine from sodium benzoate—is likewise an example of simultaneous oxidation and reduction by the hydrogen and oxygen of water.

The oxidation of organic compounds by silver oxide has been the subject of investigation by Behrend and Dreyer

(*Annalen*, 1918, **416**, 203) with a view to establishing some generalisations; it is found that, in order that a substance may be oxidised by silver oxide in ammoniacal solution, it must contain a = CHOH group combined with two —CH_2OH, = CHOH or —COOH groups, or with any two of these groups. Several other conclusions which have been arrived at may be found by reference to the original paper.

Reference was made in these columns (SCIENCE PROGRESS, 1918, xii. 392) to the synthesis of Zingerone the pungent principle of ginger by Nomura. The same author has recently (J. Chem. Soc., *Abstr.* 1918 (I) 438) undertaken an investigation into the taste of ketones allied to this compound and finds that certain unsaturated ketones have hardly any taste, but on reduction yield more soluble compounds which are distinctly pungent. The degree of pungency is considerably influenced by the position of the hydroxyl group in relation to the ketonic side-chain and is specially marked when there is a methoxy group in the meta position.

An interesting paper on colour variation in Anthocyanins is published by Shibata, Shibata and Kasiwagi (*J. Amer. Chem. Soc.* 1919, **41**, 208), but for particulars the reader is referred to the original, as reasons of space do not permit of its abstraction here.

MINERALOGY. By ALEXANDER SCOTT, M.A., D.Sc.

G. A. RANKIN and H. E. Merwin (*Amer. Jour. Sci.* **45**, 301, 1918) have investigated the ternary system $MgO - Al_2O_3 - SiO_2$, so far as the equilibrium between the various solid phases and the melt are concerned. The substances stable in contact with the melt comprise the three simple oxides, the four compounds previously found in the ternary systems, and one ternary compound $2MgO$, $2Al_2O_3$, $5SiO_2$. This last, like clinoenstatite ($MgSiO_3$), dissociates below its melting-point and is capable of taking up a considerable amount of silica in solid solution. It exists in two modifications, the low-temperature form being obtained by the crystallisation of a glass below 950° C. and undergoing an apparently irreversible transformation to the other form at somewhat higher temperatures. The properties of both forms agree closely with those of the natural mineral cordierite, but the latter differs from the former

in that it usually contains some ferrous oxide and is also hydrated. Natural cordierite, heated to 1440° C. for thirty minutes, breaks down to glass and sillimanite in a manner similar to the behaviour of the form of the ternary compound mentioned above.

The application of these results to petrology calls attention to several important problems which have not yet been solved; these include the nature of the alumina-bearing molecule in the non-alkaline pyroxenes and amphiboles, the part played by water in many pyrogenetic minerals, and the influence of ferrous oxide in the ferromagnesian metasilicates. The ternary compound mentioned above can hardly constitute the aluminous molecule in the pyroxenes, as, though it can apparently take up silica, and to a small extent spinel, in solid solution, it shows no signs of being isomorphous with the metasilicate. As no other ternary compound is stable in contact with the melt, the alumina-bearing molecule isomorphous with the metasilicate must, so far as the dominantly magnesian pyroxenes and amphiboles are concerned, be one which dissociates below its melting-point. Hence the fact that Tschermak's hypothetical silicate, which has been suggested as the aluminous molecule in these minerals, dissociates on fusion (*Sitz. Akad. Wiss. Wien.* **121**, 897, 1912) is not a valid argument against its existence.

The instability of $MgAl_2SiO_6$ at high temperatures has been verified by V. Schumoff-Deleano (*Centr. Min.* 1917, 290), who has attempted to determine the constitution of aluminous augite by synthetic means. This writer finds that diopside ($CaMgSi_2O_6$) can take up 15 per cent. of alumina in solid solution, a similar result being obtained in the case of enstatite ($MgSiO_3$) and pseudowollastonite ($CaSiO_3$). So far as enstatite is concerned, this result is at variance with the work of G. A. Rankin and H. E. Merwin, who state (*loc. cit.* p. 307) that in the ternary system $MgO-Al_2O_3-SiO_2$, magnesium metasilicate is a compound of constant composition, while the result for the lime compound is contradicted by the work of G. A. Rankin and F. E. Wright (*Amer. Jour. Sci.* **39**, 1, 1915), who find that the amount of alumina that $CaSiO_3$ can hold in solid solution is less than 1 per cent. So far, the experimental evidence is insufficient to allow discrimination amongst the numerous theories that have been advanced.

With reference to the presence of water, the close resemblance between natural cordierite and the synthetic anhydrous compound indicates that the water in the former is not an essential part of the molecule, as was suggested by the synthesis of anhydrous cordierite by Morozewicz (*Tscher. Min. Mitt.* **18**, 28, 1898) and by the work of Weibull (*Geol. Fören. Forhandl.* **22**, 33, 1900). It is very probable that the water in the natural cordierite is analogous to that in such amphiboles as tremolite, where it is supposed by E. T. Allen and J. K. Clement (*Amer. Jour. Sci.* **26**, 101, 1908) to be present in solid solution.

It is significant that the minerals which contain this "occluded" water are usually not those stable at the highest magmatic temperatures, and hence it seems feasible that one function of the water is to lower the temperature range at which the rock solidifies, with the consequent production of the forms stable at lower temperatures (*e.g.* amphibole instead of pyroxene). The water, then, is occluded in the mineral and plays no essential part in the molecule. It is possible that this explanation also holds for the hydrated form of cordierite.

Although most naturally occurring rock-forming metasilicates contain iron, most of the synthetic work which has been done has been carried out with iron-free silicates, and hence the effect of the presence of ferrous oxide, etc., on the equilibrium of the various systems is little known. Several attempts have been made to work out the system $FeO-SiO_2$ (cf. Hofman, *Trans. Amer. Inst. Min. Eng.* **29**, 701, 1899), but these have not been very successful owing to the difficulties introduced by the existence of the various iron-oxides and the fact that the relations between these are not yet completely known (cf. R. B. Sosman and J. C. Hostetter, *Jour. Amer. Chem. Soc.* **38**, 807, 1916). In a recent paper, A. McCance (*Jour. Iron and Steel Inst.* **47**, 239, 1918) obtains results very different from those of Hofman. In a number of melts obtained by fusing a mixture of ferrous carbonate and silica, crystals form which are stated to be grünerite ($FeSiO_3$). As, however, the composition is not verified by analysis and the identifications are made by metallographic in place of the more usual petrographic methods, it is very doubtful if the crystals are really ferrous metasilicate. In any case it is highly improbable that grünerite, which is an amphibole and seems generally to be of metamorphic origin

(cf. A. Lacroix, *Bull. Soc. fr. Min.* **9**, 40, 1886; **40**, 62, 1917), could be obtained by the cooling of a melt of the composition $FeSiO_3$. The fact that, except for the very few occurrences of grünerite, ferrous metasilicate has not been found in anything like a pure state in nature suggests its instability at its melting point. The evidence of igneous rocks where ferrous and magnesium compounds constantly occur in isomorphous admixture both as orthosilicates and metasilicates, does, however, indicate that the stability of the magnesium silicates is not greatly modified by the presence of ferrous oxide.

Although a large volume of work has been done on the crystallisation of silicates from melts, very little is known concerning the nature of the compounds which exist in the fused condition. Most of the attempts to gain information about the latter have involved the measurement of certain physical properties of undercooled liquids (glasses) composed of the mixtures examined. Thus E. S. Larsen (*Amer. Jour. Sci.* **28**, 263, 1909) found that the refractive indices and specific gravities of the albite-anorthite series (continuous solid solution) lay on straight lines. A similar result was obtained for the refractive indices of $CaSiO_3-MgSiO_3$ (which form one compound $CaMgSi_2O_6$ stable at its melting-point) while the density curve, although not straight, showed no cusps or other critical points. This work has been re-interpreted by E. W. Tillotson (*Jour. Amer. Cer. Soc.* **1**, 76, 1918), who maintains that both properties in the latter system show breaks in the curves, indicating the existence of compounds of the compositions $Ca_4MgSi_5O_{15}$ and $CaMg_3Si_4O_{12}$, the latter corresponding to tremolite. Similar investigations of the ternary systems $Na_2O-BaO-SiO_2$ and $Na_2O-CaO-SiO_2$ indicate the existence of several compounds in the former system and of one in the latter ($2Na_2O$, $3CaO$, $7SiO_2$). This latter compound has not the composition, $2Na_2O$, $3CaO$, $5SiO_2$, of the compound, which is indicated by the only maximum on the freezing-point diagram (Kultascheff, *Zeit. Anorg. Chem.* **35**, 187, 1903), while in the case of the sodium barium series R. Wallace (*ibid.* **63**, 8, 1909) found a continuous series of solid solutions. There seem to be two serious objections to the deductions made from this work. Firstly, the experimental errors are of a magnitude comparable with that of the deviations from the continuous curves, and secondly, since the work is carried out at

ordinary temperatures, the compounds in the glass may not be those stable at high temperatures.

The latter of these is overcome by A. L. Field and P. H. Royster (*U.S. Bureau of Mines, Tech. Paper* **189**, 1918), who have investigated the temperature-viscosity relations of part of the system $CaO - Al_2O_3 - SiO_2$. The compositions of the mixtures examined lay within the stability fields of calcium metasilicate and gehlenite ($Ca_2Al_2SiO_7$). In a binary mixture of the two silicates, the isothermal viscosity curves show a minimum for the eutectic mixture, and a maximum nearer the wollastonite end of the series, the discontinuities being much sharper at lower temperatures (*e.g.* 1350° C.) than at high temperatures (*e.g.* 1600° C.). In the ternary system, maxima occur at the quintuple points, and minima at the binary eutectics. Since the stability fields of the compounds are determinable from viscosity measurements, it follows that the compounds exist in the liquid state. Although this is apparently in opposition to the view of R. B. Sosman (*Trans. Faraday Soc.* **12**, 170, 1916) that such compounds as diopside are dissociated in the liquid state, the two cases are not strictly analogous. Gehlenite, for example, is probably the calcium salt of an aluminosilicic acid and is therefore analogous to such a salt as potassium ferrocyanide, while diopside, on the other hand, is probably a "molecular compound" and is analogous to such double salts as potassium zinc sulphate. Further, the system $CaSiO_3 - MgSiO_3$ is complicated by the fact that clinoenstatite ($MgSiO_3$) breaks up below its melting-point to the orthosilicate and free silica.

Some further work on the interesting question of the stability of the silica minerals has been carried out by J. B. Ferguson and H. E. Merwin (*Amer. Jour. Sci.* **46**, 417, 1918), who have redetermined the melting-points of tridymite and cristobalite. The former is found to melt at 1670° ± 10° C. and the latter at 1710° ± 10° C. The authors also succeeded in converting pure quartz to a mixture of tridymite and cristobalite by the action of dry heat at 1350° C., and tridymite similarly to cristobalite at a temperature in the neighbourhood of the melting-point of the former. This work affords some verification of le Chatelier's contention (*Bull. Soc. fr. Min.* **40**, 44, 1917) that tridymite is stable above the temperature, 1470° C., given by C. N. Fenner (*Amer. Jour. Sci.* **36**, 331, 1913)

as the tridymite-cristobalite transition temperature, while at the same time it indicates that cristobalite has a stable temperature range above that of tridymite (cf. *Trans. Cer. Soc.* **17**, 148, 1917).

The formation of cristobalite by the devitrification of crown glass is noted by C. N. Fenner and J. B. Ferguson (*Jour. Amer. Cer. Soc.* **1**, 468, 1918), who ascribe the "milkiness" of certain artificial glasses to the separation of crystals of this mineral (cf. F. Gelstharp, *ibid.* p. 559).

Several papers dealing with the crystals found in various types of artificial glass have been published recently. N. L. Bowen (*Jour. Wash. Acad. Sci.* **8**, 88, 1918) discusses the analogies between the occurrence of heterogeneity in glass and certain phenomena observed in igneous rocks. In a second paper (*ibid.* **8**, 265, 1918) a hitherto unknown orthorhombic silicate, $BaSi_2O_5$, is described as occurring in barium crown glass and data are given for the melting-point and the optical properties. A third paper (*Jour. Amer. Cer. Soc.* **1**, 594, 1918) is concerned with the investigation, by means of petrographic methods, of the various types of stones (crystalline particles) which are found in glass. The identification of the crystals, together with the examinations of the structure and texture, allows the origin of the stones to be determined.

An interesting paper by G. V. Wilson (*Jour. Glass Soc. Tech.* **2**, 177, 1918) describes the formation of certain minerals in glass furnaces. The crystallisation of the glass itself results in the development of quartz, tridymite, wollastonite (β-$CaSiO_3$), and augite, while the action of the glass on fragments of limestone gives, in addition to the metasilicates mentioned above, melilite and tricalcium silicate ($Ca_3Si_2O_7$). The corrosion of the materials of the furnace by the glass gives such minerals as sillimanite, corundum, magnetite, oligoclase-felspar, and biotite (apparently anhydrous). Various deductions are made concerning the conditions under which the minerals formed.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., East London College, University, London.

Morphology.—An extensive study of shoots has appeared from the pen of Prof. Warming (*Mém. d. l'Acad. Roy. d. Sci. et Lett. d. Danemark*), in which he distinguishes the follow-

ing types: (*a*) *Stolons*, in which the plagiotropic shoots are overground, with long internodes, and bear foliage leaves; (*b*) *Suboles*, where likewise the internodes are elongated, but the leaves are reduced to scales and the structure as a whole is pale-coloured. A modification of this type is presented in those cases where the terminal region is enlarged as a food reservoir; (*c*) *Rhizomes*, a term restricted to thick plagiotropic shoots with short internodes from which the aerial shoots arise; (*d*) *Rhizodes*, which resemble suboles except that the lateral shoots develop from definite main buds, as in *Hippuris* and *Eleochoris*; (*e*) *Creepers* and *leaf-bearing suboles*, in which the plagiotropic shoots, whether overground or underground, do not arise from the base of an erect shoot; (*f*) Rootstocks in which the underground stem is erect. Numerous examples of all these types are described, together with a consideration of their biological features. The length of the internodes appears to be largely dependent upon the density of the surrounding soil, whilst the direction of growth, though mainly related to gravity, is in some cases even more dependent upon moisture (e.g. *Tussilago*). The level of growth is related to a variety of environmental factors, such as the physical condition of the soil, its aeration, moisture, temperature, chemical reaction, amount of nutrients and biotic relations.

The underground shoot, which is regarded as a modification of the aerial, is particularly prevalent in certain types of habitat. Thus in Denmark, of the species exhibiting this modification, 34·5 per cent. occur in woods and hedgerows, 30·09 per cent. on wet soil and the remainder in a variety of habitats.

The present writer has investigated the floral structure of *Eranthis hyemalis*, *Ficaria verna*, *Anemone nemorosa*, *Aconitum*, spp., and other Ranunculaceæ (*Ann. Bot. Journ.*). In most of these the "curve" of meristic variation for the andrœcium and gynæceum is multi-modal in character, the modes corresponding usually to some multiple of three. The minimum is also often a multiple of that number. These facts and the sequence of dehiscence of the stamens, the orientation of the supernumerary perianth members, and the frequent occurrence of partially bifurcated organs, suggest that the Ranunculaceæ were primitively trimerous, though the feature has become obscured by fission, fusion and abortion. Positive correlation

has been shown to obtain between the different whorls, and it is pointed out that this is inconsistent with the view that supernumerary perianth members are the outcome of transformation of stamens. The perianth is regarded as having been derived either entirely from bracts, or in part from these and in part from stamens.

Taxonomy.—E. D. Merrill has described a new species of *Oreomyrrhis* from Mount Kinabalu which adds another to the already known Australian types from that locality (*American Journ. Bot.*). To the same journal J. C. Arthur contributes the final portion of his paper on the Uredinales of Guatemala, in which 9 new species of *Puccinia* are described, and also additions to the form genera *Uredo* (4 spp.) and *Aecidium* (2 spp.). In the *Journal of Botany* for January Canon Bullock-Webster describes a species of *Nitella*, closely allied to *N. flexilis* from Lough Shannagh. In the concluding parts of his paper on the genus *Manettia*, Dr. Wernham describes 26 further species.

Seven additions to the genus *Sedum* are diagnosed by Praeger, and Miss Lister describes two new varieties of *Lamproderma.*

Three new species of *Phyllanthus* are described by Fawcett and Rendle. J. F. Rock adds five more species to the genus *Cyrtandra* and several varieties all from Hawaii (*Amer. Journ. Bot.* Feb. 1919).

New species of *Carmichœlia* have been described by Prof. Cockayne (*Trans. N.Z. Institute*, vol. l.).

S. F. Blake (*Contrib. Gray Herb.* 1918, **53**) has described several new genera belonging to the Flacourtiaceæ (*Hecatostemon*), Compositæ (*Oxycarpha*), and Loasaceæ (*Schismocarpus*), and a number of new species. In the same journal J. F. MacBride describes new species of *Tricyrtis*, *Atriplex*, *Cirsum*, *Lomatium*, *Lotus* and *Lycium.*

Heredity and Genetics.—A most useful summary of the present state of knowledge of heredity and variation in *Pisum* with a bibliography of over 100 papers, has been compiled by O. E. White (*Mem. Brooklyn Bot. Garden*, No. **19**). In the introductory matter this author lays stress on the importance of recognising that the so-called " characters " are the visible expression, not of the hereditary factors alone, but a combined effect of these and the environment. For each group of

characters the different factors are considered in turn, the crossing experiments that have been performed with them and the interpretation of the results observed.

The same author contributes a study of inheritance in *Ricinus communis.* Amongst the pairs of characters dealt with were: Red-blush and green stems; bloom and absence of bloom; dehiscent and indehiscent pods; oval and orbicular seeds, in each pair the first-named behaving as a dominant. In respect both to dehiscence and seed-shape the F2 analyses suggest that two pairs of factors are involved since a 9 : 7 ratio was approximated. In the former case the pure dominant probably represents a combination of thin capsule wall with dehiscence. Crosses between large and small seeded types yielded seeds of intermediate size in F1, and in F2 all gradations between the two extremes. The author emphasises the commercial value of the indehiscent strain and of the high productiveness of F4 hybrids.

In a recent study of height in *Pisum,* White (*Contributions Brooklyn Bot. Garden,* No. 20) has shown this to be dependent on the presence or absence of at least five genetic factors. He recognises three types of internode length of which "long" and "very long" are found in tall peas and the short type in dwarfs; moreover, the internode number may have three ranges in talls, viz. 20–30, 20–40 or 40–60, but in dwarfs ranges from 10–20.

W. N. Jones, in a suggestive paper on the subject of the nature of Fertilisation and Sex (*New Phytologist,* Dec. 1918), compares fertilisation with the attack of a parasite upon its host, infertility being compared with immunity and the fusion product with symbiotic union. The postulated "gynoplasm" and "androplasm" are regarded as more or less segregated at gamete formation. Stress is laid on the necessity for distinguishing between factors determining sporophytic sex and those determining gametophytic gender.

Ecology.—Harvey and True in a paper of considerable interest to ecologists (*Amer. Journ. Bot.* Dec. 1918) describe experiments in which plants of Squash, Soya Bean and Sweet Corn were grown in solutions of different salts with a view to ascertaining the concentration at which absorption and excretion of electrolytes are in equilibrium. The results obtained seem to indicate an absorption minimum for each species which is

independent of the kind of nutrient salt, its original concentration or the volume employed.

In relation to the more beneficial effect of liming on Barley than on Rye, Hartwell and Pember have tested the two species in water, and sand cultures to which sulphuric acid had been added (*Soil Science*). For both types of culture the depression of growth was almost the same for the two species. Soil extract, however, from an acid soil had a much more deleterious effect on the Barley than on the Rye, and the same effect was produced when the ash of the soil extract was brought into solution. It was found that the different effect of acidity on the two species was due to aluminium. The latter appears to have a more pronounced toxicity for Barley than Rye, and it is suggested that the advantages of phosphating and liming soils may be due to precipitation of active aluminium.

The ecology of lichens is the subject of a paper by R. Paulson, dealing especially with Epping Forest (*Essex Naturalist*, 1918). Of the sixteen species recorded from the trunks of the hornbeam, eight are members of the Graphidiaceæ. The terricolous lichens are similar to those of Oak woods but less numerous and luxuriant. In the woods north of London the Graphidiaceæ are very poorly represented and mostly found in *Quercus sessiliflora* woods on young trees 15–25 years old. The lichen flora of Beech woods is not abundant, except in well-lighted spots on the North Downs, but *Lecidia crustulata* and *Rhizocarpon confervoides* occur, particularly on the pebbles in the Beech areas of Epping. Lists of lichens from five *Calluna* heaths comprise twelve species of which half are *Cladonias*, two belong to the genus *Bæomyces*, two to *Lecidia* and one species to each of the genera *Cetraria* and *Peztigera*.

Pathology.—The specialisation of fungal parasites belonging to the genus *Septoria* has been the subject of investigation by W. S. Beach (*Amer. Journ. Bot.* Jan. 1919). As the result of numerous inoculation experiments, he has demonstrated that some species are probably differentiated into biologic forms. Most of the species are limited in their power of infection to one or few closely related hosts. Thus *Septoria brunellæ* is probably restricted to *Prunella vulgaris*, but *S. lactricola* from *Lactuca canadensis* not only infected other species, but also *Sonchus oleraceus* though not *S. asper*. The age of maximum susceptibility varies with the host. In the

case of *Malva rotundiflora, Convolvulus arvensis* and *C. sepium* the oldest leaves were most quickly attacked, but those of *Lepidium virginicum* are equally susceptible whether young or mature. Infection of the upper-leaf surface took place more readily in *Polygonum persicaria, Erigeron annuus* and *Lactuca scariola,* whilst *Solanum carolinense* was only infected from the lower surface.

Observations on the variations of spore-length of the same species of fungus grown under different conditions gave striking results. Variation curves are furnished of the spore-lengths of *Septoria verbascicola* grown on Agar and on *Verbascum blattaria.* On moist Agar the range was from 30μ—90μ with the mode at 61μ, whilst on dry Agar the mode was 54μ and the range 32μ to 80μ. On the Mullein leaves the difference between the spores on leaves in dry and damp conditions was even more marked, the range in the former case being 25μ—65μ, and in the latter from 30μ—91μ. Growth on a different host but with similar environmental conditions caused practically no alteration in the spore dimensions. As the author justly remarks, these results raise considerable doubt as to the value of spore measurements for specific determination.

PLANT PHYSIOLOGY. By Ingvar Jorgensen, Cand. Phil., D.I.C., University College, London. (Plant Physiology Committee.)

Plant physiological investigations present at this time a rather interesting aspect. A general survey gives evidence of a good many currents of thought, and although the resultant at first sight might give rather the appearance of confusion, yet it seems certain that we are at the beginning of a period of progress in biological research.

The realisation that the life phenomena of the plant are not as simple as would appear at first sight, and that the plant experimenter could not control his material in the same rigid way as the chemist and physicist, and that under "natural" conditions only a very limited number of combinations of external conditions is operative, gave rise to a transitional period where different working hypotheses and guiding principles were utilised in the collection of experimental data. On the one hand we have the evolution of rather philosophical conceptions and units, and on the other hand we have the

introduction of semi-mystical terms, "cloaks for ignorance," as an American writer has termed them: "biological error," "limiting factor," "inhibition factor," "antagonism," and even the term "stimulus" has been subjected to much ill-use.

The main development of this phase of plant physiology has taken place on the Continent of Europe, with its sequel of lively controversies, but it may be interesting if examples are cited from this country and from America representing extreme views on the matter.

As an example of the philosophical school of thought, one may quote from the recent work of a physiologist (J. S. Haldane, *The New Physiology*, London, 1919): "The problem of physiology is not to obtain piecemeal physico-chemical explanations of physiological processes, but to discover by observation and experiment the relation to one another of all the details of structure and activity in each organism as expressions of its nature as an organism." "For biological phenomena the schematised physical conceptions are insufficient practically, and we must therefore make use of special biological conceptions, the relation of which to the physical conceptions must for the present remain more or less obscure for the lack of data." "That a meeting-point between biology and physical science may at some time be found there is no reason for doubting, but we may confidently predict that if that meeting-point is found, and one of the two sciences is swallowed up, that one will not be biology."

As a contrast to this one may refer to the recent researches of J. Loeb on Regeneration in *Bryophyllum*. This writer has obviously taken the point of view that the rather philosophical method of observation of European writers has not led to any great success. All previous work is therefore ignored and the author starts afresh to carry out quantitative experiments which can be interpreted according to the ordinary laws of physics and chemistry. To quote this author ("The Law Controlling the Quantity of Regeneration in *Bryophyllum calycinum*," *Journ. Gen. Physiol.* 1918, **1**, pp. 81–96): "This law shows that the problem of regeneration is part of the problem of growth, and that it falls under the law of chemical mass-action." Whether this drastic statement can be substantiated by further research remains to be seen, but one seems to detect in subsequent papers on the same subject

signs of the creeping in of those semi-mystical expressions referred to above.

The facts obtained in Loeb's investigations so far have appeared in a number of papers during the last two years: "On the Association and Possible Identity of Root-forming and Geotropic Substances or Hormones in *Bryophyllum calycinum*," *Science*, 1916, **44**, 210–11; "Further Experiments on Correlation and Growth in *Bryophyllum calycinum*," *Bot. Gaz.* 1916, **62**, 293–302; "A Quantitative Method of Ascertaining the Mechanism of Growth and of Inhibition of Growth of Dormant Buds," *Science*, 1917, **45**, 436–49; "The Chemical Basis of Regeneration and Geotropism," *Science*, 1917, **46**, 115–18; "Influence of the Leaf upon Root Formation and Geotropic Curvature in the Stem of *Bryophyllum calycinum* and the Possibility of a Hormone Theory of these Processes," *Bot. Gaz.* 1917, **63**, 25–50; "Chemical Basis of Correlation. I. Production of Equal Masses of Shoots by Equal Masses of Sister Leaves in *Bryophyllum calycinum*," *Bot. Gaz.* 1918, **65**, 150–74; "The Law Controlling the Quantity and Rate of Regeneration," *Proc. Nat. Acad. Sci.* 1918, **4**, 117–21; "The Law Controlling the Quantity of Regeneration in the Stem of *Bryophyllum*," *Journ. Gen. Physiol.* 1918, **1**, 81–96; "The Physiological Basis of Morphological Polarity in Regeneration," *Journ. Gen. Physiol.* 1919, **1**, 337–62.

The chief conclusion which Loeb draws from his investigations is that the mass of a tissue regenerated by an isolated piece of an organism is directly proportional to the mass of growth material contained in the sap or blood of the isolated piece, other conditions and time being constant. The experiments were made on *Bryophyllum calycinum*, which is particularly suitable for the purpose. When leaves of this plant are isolated from the stem they regenerate shoots in some or many of the nodes. When a piece of stem is cut out from a plant it will form shoots from two of its most apical buds, the mass of shoots regenerated being in direct proportion to the mass of a leaf attached to the stem in the latter case, and to the mass of the isolated leaf in the former case.

In order to explain some other facts, namely, that as a rule, only the apical bud of an isolated piece of stem grows out into a shoot, and none of the buds situated more basally in the stem, and how it is that the same bud which grows out

when the piece of stem is cut out from the whole plant does not grow out as long as the piece is part of a whole and normal plant, the author suggests that the growing apex as well as the leaves of a plant continually produce and send towards the base of a plant substances which inhibit the growth of dormant buds. When a piece of stem is cut out, the inhibitory substances then present in the stem will continue to move towards the base, so that the most apical buds will be the first to become freed from the inhibitor, with the result that they are the first to grow out into shoots. As soon as this happens these latter send out the inhibitory substances, as a consequence of which none of the more basally situated buds are able to develop further.

Similar considerations are presented in regard to the phenomena of geotropic curvature in *Bryophyllum*, where the author assumes that a hypothetical geotropic hormone is associated, or identical, with a root-forming hormone, which is responsible for the geotropic growth which takes place in the cortex of the lower side of a horizontally suspended stem.

A somewhat different outlook, which perhaps approaches the happy medium between the two extreme views cited above, and which perhaps best illustrates the modern tendencies in plant physiological research, is found in a paper by G. Gassner ("Beiträge zur physiologischen Charakteristik sommer- und winterannueller Gewächse insbesondere der Getreidepflanzen," *Zeitsch. f. Bot.* 1918, **10**, 417–80). The general characteristic of winter and summer annual plants is the length of the vegetation period, winter annual plants germinating the autumn of the year preceding that of maturation. A closer experimental examination reveals that the conditions are not so simple. Winter annual plants can be brought to germinate in the spring of the same year in which maturation occurs, for instance, by allowing the seeds to germinate at a temperature of $+ 1°$ to $+ 2°$ C. and then transferring them to the open. One cannot, therefore, say that the characteristic of a winter or a summer annual plant is due to some hereditary property independent of external conditions. The life period under natural conditions is determined by a certain co-operation between certain internal characteristics and external natural conditions. We cannot thus speak of a specific autonomous life period of annual plants, nor of a rhythmic rest

period of winter annual forms, as this rest period can be eliminated under suitable experimental conditions. We may be able to speak of properties which, in co-operation with climatic conditions, determine the natural vegetation period. Such properties are, in addition to a characteristic after-ripening period in the seed, a specific low-temperature requirement, and a specific frost-resistivity, but also here difficulties arise on closer examination. A specific frost-resistivity of an organism does not exist. Experience shows that the same plant has different powers of resistance to frost at different stages of development. Consequently we cannot speak of an autonomous frost-resistivity, but only about inheritance of a specific property to produce under certain external conditions more or less resistance to frost. Similarly concerning the requirements in regard to low temperature the author has shown that the flowering of winter annual grasses is bound up with the action of low temperature, but it must be observed that, according to the age of the plant, different low temperatures must be applied. But even with plants of the same age the same low temperature acts differently according to other external conditions. A temperature which, in light and air rich in carbon dioxide, will result in flower formation at a later stage, does not do this if illumination or carbon dioxide are insufficient. The author arrives at a conclusion in regard to the inheritance of plant properties similar to that of some investigators in genetics, *e.g.* Baur ("Einführung in die Exakte Vererbungslehre," Zweite Aufl., Berlin, 1914), who cites the case of *Primula sinensis rubra* and *P. sinensis alba,* in which the colour of the flower is bound up with a certain temperature, for if *P. sinensis rubra* is cultivated at about 30° C. the colour becomes white. As Baur says : " Only a certain specific type of reaction to the external world is inherited, and what we perceive with our senses as external characteristics are only the result of this reaction on a chance combination of external conditions under which the individual has developed."

ZOOLOGY. By Prof. Chas. H. O'Donoghue, D.Sc., F.Z.S., University of Manitoba, Winnipeg, Canada.

Invertebrata.—The theory put forward by Child that there is an axial metabolic gradient in *Tubularia* has been reinvestigated by Banus (" Is the Theory of Axial Gradient in the

Regeneration of *Tubularia* supported by facts?" *Jour. Exp. Zool.* vol. xxvi. No. 2, July 1918). The principal evidence adduced by Child was that if a polyp was cut into two and decapitated, the oral end would grow a new head more quickly than the basal end, but the present author by means of further experiments of the same sort claims that this is not the case and that the two ends can regenerate a head at equal speeds.

Gatenby in a paper on "The Cytoplasmic Inclusions of the Germ-Cells: Part 3, The Spermatogenesis of some other Pulmonates" (*Quart. Jour. Micro. Sci.* vol. lxiii. Part 2, August 1918) continues his investigations into the behaviour of certain bodies appearing in the cytoplasm during the processes of maturation of the spermatozoa in the pulmonate Mollusca. A very useful inclusion in the paper itself is a discussion of the terminology that has been employed by various writers and a glossary of it. Certainly to any one who was not actually dealing with the problems, the number and usage of the terms met with in the different memoirs on the subject offered a serious stumbling-block. This will be got over if the terminology here proposed should be generally adopted. Various bodies and their changes have been investigated, including Mitochondria, Chondrioplasts, and a new set of structures termed post-nuclear granules, and also their reactions to stains and fixatives discussed. Finally, it is concluded that "it will be seen that strong evidence is being collected against the view that the Mitochondria take any part in the transmission of hereditary characters."

As the result of "Studies on the Physiological Significance of Certain Precipitates from the Egg Secretions of *Arbacea* and *Asterias*" (*Jour. Exp. Zool.* vol. xxvi. No. 3, August 1918), Woodward concludes that the secretion, which is of the nature of a nitrogenous celloid, contains two distinct substances. The first is a sperm agglutinin which activates and reversibly agglutinates sperm of the same or allied species. The second is a parthenogenetic agent destroyed by boiling capable of dealing with an unsaturated fatty acid from the egg. "The Development of *Echinocardium cordatum*: Part 2, The Development of the Internal Organs" (*Quart. Jour. Micro. Sci.* vol. lxiii. Part 2, August 1918) by MacBride constitutes, together with his previous paper on the same species, the first complete account of the development of a Spatangoid. It is interesting

since it shows that a record of the changes which transformed a regular sea-urchin into a Spatangoid are indicated in the post-larval development of the latter. Thus, although it is devoid of teeth in the adult, five dental sacs are formed in the larva; the intestine in the young image is thrown into a reverse coil as in the regular Urchin; its spines (with certain exceptions) are similar to those of the regular Urchin and so on. The madreporite vesicle originates from a dorsal outgrowth of the right anterior cœlom. The left anterior cœlom is modified into an axial sinus which does not develop and is vestigeal in the adult.

Other papers include: Crozier, "The Amount of Bottom Material ingested by Holothurians (*Stichopus*)" (*Jour. Exp. Zool.* vol. xxvi. No. 2, July 1918).

Two points in the embryology of *Peripatus* as described by Sedgwick have led to considerable discussion, and the author himself laid a great deal of stress on them in his subsequent writings; these have been reinvestigated by Glen; "A Revision of Certain Points in the Early Development of *Peripatus capensis*" (*Quart. Jour. Micro. Sci.* vol. lxiii. Part 2, August 1918). It has been found that in the early stages of development there is no doubt as to the presence of cell walls—a fact denied by Sedgwick. Again, this author maintained that the whole of the nephridium was mesodermal in origin, but Glen has shown that, as in Annelids, the tube is completely ectodermal and only the funnel is mesodermal. Thus the structure of the excretory organ in *Peripatus* is homologous with that in the Annelids.

"The Segregation of the Germ-Cells in *Trichogramma evanescens*" has been described by Gatenby (*Quart. Jour. Micro. Sci.* vol. lxiii. Part 2, 1918). The author previously described a number of the developmental stages in this form and has now been able from further material to add to that, the present account of the cytological changes involved in the segregation of the germ-cells. The former description of a cloud of granules constituting the germ-cell determinant, which seem to appear spontaneously in the egg before the extrusion of the first polar body, is confirmed. These granules are partly and perhaps even entirely composed of cytoplasmic material and their function is nutrimental. There is an early separation between germ-cells and somatic cells. It seems now to be an estab-

lished fact that in certain forms, as here, no centrosome or normal spindle exists at special periods of the segmentation. The same author deals also with the subject of "Polyembryony in Parasitic Hymenoptera: A Review" (*ibid.*). As its title indicates, this is a summary of the facts that are known concerning the occurrence of polyembryony, that is, the process whereby one egg may produce several score of embryos, as it is encountered in the parasitic Hymenoptera, small insects about 1 mm. in length. About 10 eggs are laid without vital injury to the host eggs. A tropho-amnion is formed as the result of the division of the nuclei of the polar bodies, together with a certain amount of the egg cytoplasm, and its function is to nourish the inner embryonic mass. The primary embryonic cell, while the tropho-amnion has been forming, has divided into many germinal masses, each of which, after a large number have been produced, proceeds to form a separate embryo. Later, the larvæ break out of their membranes and live freely for a time and finally pupate inside the host's body. The review terminates with a discussion of the significance of the phenomenon.

Other papers include: Kunkel, "The Effects of the Ductless Glands on the Development of the Flesh-flies" (*Jour. Exp. Zool.* vol. xxvi. No. 2, July 1918); Allee and Stein, "Light Reactions and Metabolism in May-fly Nymphs" (*ibid.* August 1918); Rich, "The Gill-chamber of Dragon-fly Nymphs" (*Jour. Morph.* vol. xxxi. No. 2, September 1918); Hampson, "A Classification of the Pyralidæ, subfamily Hypsotropinæ" (*Proc. Zoöl. Soc.* Parts 1 and 2, August 1918); and Cleghorn, "First Report on the Inheritance of Visible and Invisible Characters in Silkworms" (*ibid.*).

Vertebrata.—Johnson has studied the "Structure and Development of the Sense Organs of the Lateral Canal System of Selachians (*Mustelus canis* and *Squalus acanthias*)" (*Jour. Comp. Neur.* vol. xxviii. No. 1, August 1918). He finds that "The sensory canal system of *Squalus* and *Mustelus* comprises two sets of organs known as canal organs and surface or pit organs respectively. The two sets are genetically equivalent, but development in the former is carried to a further advanced stage, the organs of this type forming in the adult an essentially continuous column of sensory epithelium along one side of an epithelial canal." Other papers include: Smith, "On the

Process of Disappearance of the Conus Arteriosus in Teleosts" (*Anat. Rec.* vol. xv. No. 2, September 1918); Pinney, "A Study of the Relation of the Behaviour of the Chromatin to Development and Heredity in Teleost Hybrids" (*Jour. Morph.* vol. xxxi. No. 2, September 1918); and Newman, "Hybrids between Fundulus and Mackerel: A Study of Paternal Heredity in Heterogenic Hybrids" (*Jour. Exp. Zool.* vol. xxvi. No. 3, August 1918). "The Development of the Hypophysis of the Anura" has been worked out by Atwell (*Anat. Rec.* vol. xv. No. 2, September 1918), who finds this body to consist of three epithelial lobes and a neural lobe. The former form the anterior lobes, the pars intermedia, and the pars tuberalis. The caudal end of the primitive infundibulum thickens and becomes transversed by a ridge which later is constricted from the infundibulum dorsally and at the sides, and so comes to form a definite neural lobe. The pars intermedia develops from the caudal tip of the hypophyseal rudiment; the anterior lobe proper comes from the main central part of the rudiment, and the pars tuberalis takes origin from the lateral lobes. Other papers include: Johnson, "On the question of Commissural Neurones in the Sympathetic Ganglia" (*Jour. Comp. Neur.* vol. xxix. No. 4, August 1918); Matsumoto, "Demonstration of Epithelial Movement by the use of vital staining, with Observations on Phagocytosis in the Corneal Epithelium" (*Jour. Exp. Zool.* vol. xxvii. No. 1, October 1918); McClure, "On the Behaviour of *Bufo* and *Rana* toward Colloidal Dyes of the Azo Group (trypan blue and dye No. 161)" (*Amer. Anat. Mem.* No. 8, July 1918), and Bhatia and Prashad, "The Skull of *Rana tigrina* Daud." (*Proc. Zool. Soc.* Parts 1 and 2, August 1918).

It has been shown by Redfield, "The Physiology of the Melanophores of the Horned Toad *Phrynosoma*" (*Jour. Exp. Zool.* vol. xxvi. No. 2, July 1918), that there are three types of colour change, the first corresponding with daily changes of temperature and illumination, the second an adaptation to the general environment, and the third connected with nervous excitement. The contraction of the melanophores during nervous excitement is in part due to the activity of the sympathetic nervous system and in part to the secretion of adrenin by the adrenal glands.

Other papers include: Boulenger, "Description of a new

Snake of the Genus *Oligodon* from Upper Burma" (*Proc. Zool. Soc.* Parts 1 and 2, August 1918); Lantz, "Reptiles from the River Tajan (Transcaspia)" (*ibid.*); and Procter, "On the Variation of the Pit-viper, *Lachesis atrox.*"

Papers on birds include: Riddle, "A Demonstration of the Origin of two pairs of Identical Female Twins from the two Ova of high-storage Metabolism" (*Jour. Exp. Zool.* vol. xxvi. No. 2, July 1918) and Slonaker, "A Physiological Study of the Anatomy of the Eye and its accessory parts of the English Sparrow (*Passer domesticus*)" (*Jour. Morph.* vol. xxxi. No. 3, December 1918).

"On the External Characters of the Lemurs and of *Tarsius*" (*Proc. Zool. Soc.* Parts 1 and 2, August 1918) is an account by Pocock of the results of a detailed examination of the external characters of a number of different species which have led to certain conclusions with regard to the systematic position of these forms. *Tarsius* is removed from the Lemurs and placed with the higher Primates. The Primates are thus divided into two groups, for which the author proposes the names Strepsirhini, embracing the Lemurs, and Haplorhini, comprising *Tarsius* and the Pithecoidea. The last name is suggested as being preferable to the name Anthropoidea. The Strepsirhini may then be divided into Lemuroidea and Chyromoidea. "Neuromeres and Metameres" is the title of a paper by Neal (*Jour. Morph.* vol. xxxi. No. 2, September 1918), summarising the observations based on the nidular relations of the cranial nerves in *Squalus* embryos fixed in various ways. The author concludes that "The nervous connection of four rhombomeres with a single visceral arch (the hyoid) and of three visceral arches with one rhombomere (the seventh) are facts not easily reconciled with the assumption that rhombomeres are metameric structures. If they are, the factors which have brought about their present non-metameric relations are wholly obscure." Stewart writes "On the Thymus IV. (so-called) and the Ultimo-Branchial Body of the Cat (*Felis domestica*)" (*Amer. Jour. Anat.* vol. xxiv. No. 2, July 1918). More than seventy embryos were examined and the author could find no evidence for thinking that it is only the diverticulum ventrale of the fourth branchial pouch that contributes to the epithelial primordium of the internal thymic lobule of the thyroid. It appears more probable that the entire caudal pharyngeal complex is

involved save that part concerned producing parathyroid IV. The internal thymic lobule is almost of constant occurrence in the cat but is to be regarded as a regressive structure.

Other papers include: A series of papers on the sympathetic nervous system as follows: Ranson, "An Introduction to a Series of Studies on the Sympathetic Nervous System"; Billingsley and Ranson, "On the Number of Nerve Cells in the Ganglion Cervicale Superius and of the Nerve Fibres in the Cephalic End of the Truncus Sympatheticus in the Cat, and on the Numerical Relations of Preganglionic and Postganglionic Neurones," and also "Branches of the Ganglion Cervicale Superius in the Cat"; three papers by Ranson and Billingsley, "The Thoracic Truncus Sympatheticus, Rami Communicantes, and Splanchnic Nerves in the Cat"; "An Experimental Analysis of the Sympathetic Trunk and greater Splanchnic Nerve in the Cat" and "The Superior Cervical Ganglion and the Cervical Portion of the Sympathetic Trunk," all of which are in *Jour. Comp. Neur.* vol. xxix. No. 4, August 1918; Allen, "Surface View of several Injected Intestinal Villi of a Rabbit" (*Anat. Rec.* vol. xv. No. 1, August 1918); Atwell, "The Development of the Hypophysis Cerebri of the Rabbit (*Lepus cuniculus*)" (*Amer. Jour. Anat.* vol. xxiv. No. 3, September 1918); Casparis, "Lymphatics of the Omentum" (*Anat. Rec.* vol. xv. No. 2, September 1918); Curtis, "The Morphology of the Mammalian Seminiferous Tubule" (*Amer. Jour. Anat.* vol. xxiv. No. 3, September 1918); Danchakoff, "Equivalence of Different Hematopoietic Anlages (by method of stimulation of their stem cells): II. Grafts of Adult Spleen on the Allantois and Response of the Allantoic Tissues" (*ibid.* July 1918); Hollister, "East African Mammals in the United States National Museum: Part I. Insectivora, Chiroptera and Carnivora" (*U.S. Nat. Mus. Bull.* 99, 1918); Jordan, "A Description of a case of False Hermaphroditism" (*Anat. Rec.* vol. xv. No. 1, August 1918); "The Histology of Lymph, with special reference to Platelets" (*ibid.*), and "A Contribution to the Problems concerning the Origin, Structure, Genetic Relationship, and Function of the Giant Cells of Hemipoietic and Esteolytic Foci" (*ibid.* July 1918); King, "Studies on Inbreeding: I. The Effect of Inbreeding on the Growth and Variability in the Body Weight of the Albino Rat" (*Jour. Exp. Zool.* vol. xxvi. No. 1, May 1918); Marshall, "An Unusual Right Lung" (*Anat. Rec.*

vol. xv. No. 2, September 1918); Miller, "A New River-Dolphin from China" (*Misc. Coll. Smithson. Inst.* vol. lxviii. No. 9, 1918), and "Mammals and Reptiles collected by Theodore de Booy in the Virgin Islands" (*Proc. U.S. Nat. Mus.* vol. liv. 1918); Radash, "A Branching or Duplication of the Neural Canal" (*ibid.*), and Risquez, "Un casa de ectopia extratoraxica" (*ibid.* July 1918).

General.—The papers include: Allen, "An Inexpensive Projection and Drawing Apparatus" (*Anat. Rec.* vol. xv. No. 1, August 1918); Mast, "Effects of Chemicals on Reversion in Orientation to Light in the Colonial Form, *Spondylomorum quaternarium*" (*Jour. Exp. Zool.* vol. xxvi. No. 3, August 1918); and Miller, "A Differential Injection Mass for use with Stereoroentgenograms" (*Anat. Rec.* vol. xv. No. 1, August 1918).

ARTICLES

THE WATER ECONOMY OF MARITIME PLANTS

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Since 1904 particular attention has been given by the botanical department of University College, London, to the problems associated with the conditions of existence of maritime plants.' The investigations alluded to were initiated at Erquy on the coast of Brittany, and were continued at Blakeney in Norfolk and in the departmental laboratories. Amongst the results obtained is a certain amount of information regarding the absorption and transpiration of water by halophilous plants, more particularly by such characteristic forms as *Salicornia* and *Suæda*.

In the present article an attempt is made to weave into a consecutive story the results already made public together with certain unpublished observations; attention to other work is given only so far as is considered necessary.

THE ACTION OF SODIUM CHLORIDE

Sodium chloride being an all-important factor in the configuration of a salt-marsh vegetation, attention may first be directed to certain effects of this substance on plants.

The work of several investigators has shown that a solution of pure sodium chloride has a pronounced toxic effect upon plants; and this is true not only for ordinary mesophytes, but also for marine plants. If, however, other salts be dissolved in the salt solution, the solution, although it contains as much sodium chloride as before, does not have any such ill-effect.

Under natural conditions, plants are not subjected to the action of pure solutions, and if sodium chloride be present in the soil, there also are present other salts which exert a pro-

tective influence. But notwithstanding this, there are a large number of plants which are intolerant of this salt except in the smallest amounts ; such plants will never be found growing in a salt marsh. On the other hand, many plants can and do flourish in soils which are excessively saline. Between these two extremes plants of an intermediate constitution obtain ; and in some cases it appears that plants normally intolerant may be educated to endure certain salts. Thus Strange [1] found that the cells of the root-tip of the white lupin and of the scarlet runner quickly died if placed directly into a solution of ·5 per cent. potassium nitrate ; but if they were placed in water and the salt added so that the concentration was gradually increased, they could endure a strength of 1 per cent. without succumbing. With regard to the limits of endurance of salt marsh plants, Halket [2] found that *Salicornia ramosissima* and *Suæda maritima* could withstand the presence of 17 per cent. of sodium chloride in the soil water.

Typical maritime plants can not only live in such highly saline soils, but may require a certain amount of sodium chloride for their normal development.

The series of water and other cultures carried out by Halket showed that species of *Salicornia* grow better in the presence of sodium chloride than in its absence, the most luxuriant growth taking place when 2–3 per cent. of this substance is present. The results obtained for *Salicornia oliveri* are graphically shown in Fig. 1.

On the other hand, *Suæda maritima* is more indifferent, growth being as vigorous with as without the salt, provided that not more than 1 per cent. obtains. If present in greater amounts, the growth of the plants is proportionately diminished.

The permeability or otherwise of membranes, with regard to a definite substance, may differ not only in different plants but also in the different cells of the same plant, so that no general rules can be laid down. With regard to sodium chloride, the plasmatic membranes of the cells of maritime plants generally are permeable, and the same is true for a good many other plants. For instance, De Vries found that the beetroot is permeable to common salt, but this is hardly surprising since this plant in its natural state is maritime.

[1] Strange, *Bot. Zeit.* **50**, 253, 1892.
[2] Halket, *Ann. Bot.* **29**, 143, 1915.

Also the investigations of Boodle,[1] Dandeno,[2] Lewis,[3] and others show that the leaves of several ordinary plants can absorb salt. In such cases of the absorption of sodium chloride, one of the great dangers accruing is that of undue concentration of the salt within the cells, for then various derangements, *e.g.* the salting out of proteins, might take place.

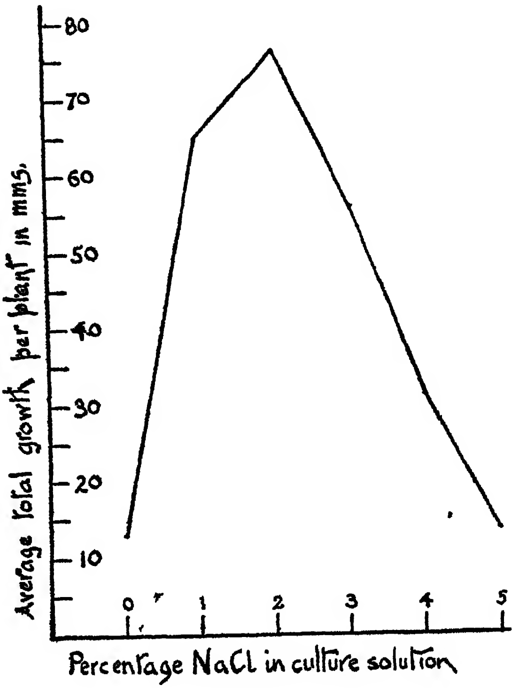

FIG 1.

ADAPTATION TO VARYING SALINITY OF WATER

An important characteristic feature of estuarine salt marshes is the periodic inundation by the tides, and this, necessarily, has a marked effect upon the soil. A region which is covered only by the highest tides tends to become increasingly saline during the inter-tidal cycle; especially is this the case during periods of relative drought. On the other hand, all localities, uncovered by the tides, become less saline after rain.

[1] Boodle, *New Phyt.* 3, 39, 1904.
[2] Dandeno, *Trans. Canad Inst.* 7, 238, 1901.
[3] Lewis, *New Phyt.* 11, 255, 1912.

The following table (I) shows the variation in the salinity—estimated as chlorides—of the soil water taken from definite stations at Erquy. The characteristic features of these localities may be briefly mentioned. Station 1 is situated on a low plateau which is covered by tides of about 37 ft.; station 6 is a pan, the soil of which is argillaceous and does not support vegetation, which is covered by tides of over 39 ft.; and station 18 is a patch of sand inundated by tides higher than 33 ft. It must also be remarked that the salinity of the sea-water at Erquy is about 3·15 per cent., estimated as chlorides.

TABLE I

Date.	Rainfall.	Salinity of soil water.		
September 1905.		Station 1.	Station 6.	Station 18.
	Inch.	Per cent.	Per cent.	Per cent.
5	·015	2·77	—	—
6	·0	3·31	3·21	3·67
7	·075	—	3·02	3·58
8	·0	—	—	—
9	·03	3·35	—	—
10	·42	—	—	—
11	·60	1·47	2·28	·74
12	·0	1·01	·52	—
13	·0	—	·62	—
14	·347	·79	2·12	—

A consideration of these figures shows that the roots of salt-marsh plants may be subjected to considerable variations in the salt-content of the soil.

These variations in the external conditions of growth obtain not only for salt-marsh plants like *Salicornia*, but also for others. Osterhout[1] has described how marine Algæ attached to the sides of coasting steamboats may be subjected to very considerable variations in the salinity of the water, the daily range being from highly concentrated sea-water on the one hand to fresh water on the other. He also found that delicate marine Algæ taken from the sea could grow for months in fresh water; further, that Algæ taken from normal sea-water and placed in concentrated sea-water flourished for one or two months; and finally, that similar plants taken from the sea lived for long periods in diluted sea-water.

[1] Osterhout, *Univ. Calif. Publications*, 2, No. 8, 1906; No. 9, 1906.

It is thus seen that such extremes in the external conditions are anything but rare, and naturally many questions arise regarding the responses made by the plant.

There is no gainsaying the fact that salt-marsh plants grow and flourish in a soil the salinity of which, and therefore also the osmotic pressure, is continually altering. On theoretical grounds it consequently follows that the osmotic properties of the absorbing organs, the root-hairs for instance, must also be continually fluctuating. If this were not so the concentration of the soil-water would eventually withdraw water from the plant, which would therefore die if the conditions were continued for any length of time. On the other hand, if the salinity of the soil-water decreased, the root-hairs would absorb large quantities of water, more possibly than the plant could get rid of by transpiration, so that the internal hydrostatic pressure would become too great for the well-being of the plant.

But conclusions based on theory are not always borne out by the observed facts ; it is, therefore, desirable to examine the plants under these varying conditions of growth.

Before passing on to the observations made at Erquy, brief mention may be made of the work of others in this particular connection.

Janse [1] found that if *Chætomorpha* were placed in water of a higher degree of salinity than that to which it was accustomed, the cells became plasmolysed, but after a longer immersion they recovered their normal state. These facts indicate that there had been a readjustment of the osmotic powers of the cells in response to the increased external salinity.

Nathansohn [2] found, in the case of *Codium tomentosum*, that the cells were permeable to chlorides, and if a plant which had been growing in a saline solution were placed in fresh water free from chlorides, these salts would diffuse out of the cells until their concentration were the same both within and without the plant. Further, the reverse would take place if the plant were removed from one solution and placed in another having a concentration of chlorides greater than the cell-sap. In other words, there was a constant passage of chlorides from the plant to the surrounding liquid, and *vice versa* until the

[1] Janse, *Bot. Centrbl.* 32, 21, 1887.
[2] Nathansohn, *Ber. deut. Bot. Gesells.* 19, 509, 1901.

concentration of this substance in each was equal. He therefore concluded that the permeability of the plasmatic membranes of the cells may change according to the concentration of certain substances inside and outside the cell.

Graves [1] found that the root-hairs of *Ruppia maritima* when placed in slightly concentrated sea-water (110 per cent.) at first showed no reaction, but eventually they became plasmolysed. He also ascertained the rapidity of this reaction exhibited by the cells of the leaves when treated with solutions of sodium chloride of different strengths. It will be observed that the results of Graves are of a nature opposite to those obtained by Janse for *Chætomorpha.*

The Erquy work on root-hairs [2] was carried out on seedlings because it was found to be practically impossible to remove successfully the earth from the roots of mature plants without damaging the region of the active root-hairs.

The osmotic pressure of the root-hairs of *Salicornia* and *Suæda* was first investigated; this was found to vary somewhat according to the place in which the seedlings were growing, and in all cases it was considerably higher than that of an ordinary mesophytic seedling.

Table II sets forth the results:

TABLE II

Locality.	Plant.	Osmotic equivalent.
Tansley's Pan	*Salicornia*	6·73 % sodium chloride.
" "	*Suæda*	6·73 % " "
Station 1	*Salicornia*	5·8 % " "
Station 4	"	6·26 % " "
"	*Suæda*	6·26 % " "
Ploughed field	Mesophyte seedling (a composite?)	1·5 % " "

A number of seedlings were then placed for some time in solutions of sodium chloride in serial decreasing concentrations. On testing the root-hairs, it was found to be very difficult to determine exactly their osmotic strength. Fresh seedlings were therefore selected and were mounted in a molecular solution of sodium chloride (5·85 per cent.); all those whose root-hairs showed no plasmolysis, and whose osmotic equiva-

[1] Graves, *Trans. Connecticut Acad. Arts and Sci.* **14**, 1908.
[2] Hill, *New Phyt.* **7**, 133, 1908.

lents were consequently above that of the test solution, were placed in fresh or slightly saline water for some time, at the conclusion of which the average osmotic equivalents were determined. In each case they were found to be very much lower (Table III).

TABLE III

Locality.	Period of soaking.	Osmotic equivalent.
Station 4	5 hours.	2·4 % sodium chloride.
"	12 "	1·91 % " "
Tansley's Pan . . .	18 "	3·31 % " "

These results were confirmed by various other tests, and clearly show that the root-hairs of *Salicornia*, growing in places where the soil-water is strongly saline, can accommodate their internal osmotic pressure as the salinity of the soil-water falls in concentration.

It remains to be ascertained whether or not they are capable of raising their internal osmotic strength in proportion to the increase of the external salinity.

Sods containing seedlings of *Salicornia* were taken from a spot which had been covered by the tide a few hours before, and placed in a dish of fresh water which was changed at intervals in order to reduce gradually the salinity. Periodically seedlings were tested with regard to the osmotic strength of the cell-sap of the root-hairs. The strength of the sodium chloride solution used for the test was 3·5 per cent.

Eventually the root-hairs of a number of seedlings were found to be strongly plasmolysed by this solution. These were allowed to remain in a saline bath of the test solution all night; the next morning 81 per cent. showed no plasmolysis, and 19 per cent. were slightly plasmolysed, but nothing like to so great an extent as when they were first examined. These results were confirmed by removing a portion of the sod from the bath of fresh water and placing it in a bath of 3·5 per cent. sodium chloride. Natural conditions were thus reproduced, and all the seedlings examined behaved as before.

A fresh batch of seedlings were then taken from the turf soaked in fresh water, and drawings were made of the roots, in order that the root-hairs could be identified when microscopically examined after subsequent treatment. To start

with, all the root-hairs were strongly plasmolysed by a 3·5 per cent. solution of sodium chloride. The seedlings were then rinsed in dilute saline and were gradually brought up through successive concentrations to a 3·5 per cent. solution in which they were allowed to remain for some little time. The root-hairs of all the seedlings but one showed a complete recovery, and exhibited no plasmolysis. The root-hairs of the one seedling which did not show recovery were proved by subsequent investigation to be either dead or in a condition of very low vitality, so that the negative results afforded by this seedling in no way invalidated the positive ones given by the other individuals.

It is, therefore, seen that the root-hairs can raise their osmotic strength as the salinity of the surrounding soil-water increases. The two results show that the osmotic power of the cell-sap of the root-hairs can be accommodated to that of the soil. Incidentally it may be remarked that this adjustment of the internal osmotic pressure in correlation with the external conditions was subsequently found to obtain in desert plants by Fitting.[1]

In addition to this fundamental fact, other observations of less importance were made. It was found that when the roots of seedlings taken from a very salt soil were plunged suddenly into fresh water some root-hairs burst, while others swelled up at their tips in many curious ways. The explanation is obvious: the osmotic strength of the root-hairs was adapted to the surrounding high salinity; when plunged into fresh water there would be a sudden and relatively great absorption of water, so that if the cell-walls were not sufficiently strong to withstand this sudden increase in the internal hydrostatic pressure the cells would either burst or swell up considerably.

This is a well-known phenomenon: thus Noll found that *Bryopsis*, if taken from a rock-pool and suddenly plunged into fresh water, would burst; Lindforss ascertained that the pollen grains of many plants would do likewise when placed in water; Curtis observed that moulds grown in concentrated solutions and then placed in water burst at the tips; also unicellular animals like *Paramœcium* behave in a like manner when subjected to a similar change of conditions.

[1] Fitting, *Zeitschr. Bot.* 3, 209, 1911.

This tendency possibly supplies an explanation of the greater thickness of the cell-walls of the apical region of the root-hairs of *Salicornia.*

Further, it was observed that the osmotic pressure of the root-hairs of the same individual plant varied; generally the younger hairs had a higher equivalent, whilst the cells of the root-cap had a very low co-efficient. Also, a variation in the osmotic strength of the root-hairs of different individuals growing in the same locality was sometimes observed, although this variation was never so great as to cause the internal osmotic pressure to fall below that of the surrounding soil water.

The next question which naturally arises is, How is this alteration in the osmotic pressure of the root-hairs brought about?

Two hypotheses suggest themselves: either the primordial utricle is permeable to salt which passes into or out of the cell according to the differences in pressure on each side of the plasmatic membrane, and so the internal pressure is raised or lowered; or, chemical changes may take place in the cell, as a consequence of which either a fall or a rise in the osmotic pressure would result.

The work of Nathansohn on *Codium*, and the fact that the amount of sodium chloride in the seedlings of *Salicornia* increased in amount as the plumule was reached, indicate that the former hypothesis is probable. But on testing the root-hairs for sodium chloride, by means of silver nitrate, no indication of its presence was given. It is, however, possible to explain this negative result. Before testing for chlorides in the root-hairs it was necessary to wash the seedlings in pure water until all adhering chlorides were removed, for, of course, if this were not done the precipitate of silver chloride on the outside of the root-hairs would make it very difficult, if not impossible, to arrive at a satisfactory conclusion regarding the presence or absence of this substance within the hairs. The washing, however, would lower the external osmotic pressure; consequently, relatively much water would be absorbed and passed on to the more internal tissues, so that the root-hairs would be washed free from salt. Or, from Nathansohn's work, the salt would pass out of the root-hairs into the surrounding water owing to the difference in the concentration of this substance.

The only way of solving the difficulty is to grow the plants in culture media to which chlorides have been added, and to make periodical chemical analyses both of the culture medium and of the plants.

TRANSPIRATION

The facts relating to the absorption of water by means of the roots lead naturally to a consideration of the transpiration of water, for, if this be low, the conditions inseparable from a saline habitat, which is generally considered to mean physiological drought, need not be so spartan as is often supposed. On purely anatomical grounds,[1] the facts that in *Salicornia* and *Suæda* the cuticle is very thin, and the stomates are superficially placed; that in two such typical halophytes as *Salicornia* and *Aster Tripolium* the stomates are essentially mesophytic in character—since they are superficially placed, are capable of movement and are sensitive to changes in the intensity of light and in the humidity of the atmosphere—indicate that transpiration is not abnormally low. But against these must be set the facts that the cell-sap is acid; that the stomates are less numerous per unit area as compared with a typical mesophyte such as *Vicia cracca*; that the stomata in some cases, e.g. *Suæda maritima*, are apparently always closed, or, in other instances, lose their power of movement and remain closed at the flowering period, e.g. *Salicornia*; and that water-storage tissues may be present.

It has been found by experiment that the transpiration of the plants in question is remarkably high. But, before passing on to a consideration of these investigations, brief mention may be made of the relationship between stomates and transpiration in the present connection.

It is a difficult question to decide to what extent the stomates of succulent plants are capable of regulating transpiration. Delf[2] considers that the limiting power of stomates may be greater than is generally supposed, for when the guard cells are apparently closed a regulation of size in the outer respiratory chamber may be demonstrated.

The cut ends of two healthy and similar shoots of *Salicornia*

[1] Delf, *Ann. Bot.* **25**, 485, 1911; de Fraine, E., *Journ. Linn. Soc. Lond. Bot.* **41**, 317, 1913.

[2] Delf, *Ann. Bot.* **26**, 409, 1912.

annua were sealed with wax; one was used to determine the loss of water during transpiration and the other to determine the condition of the stomates during the experiment. Both shoots were under exactly the same conditions, and it was observed that, even before the shoots were cut, the stomates appeared closed; the guard cells, however, showed a wide rift above the closed pore.

It was found that the diminution in the rate of transpiration was 28 per cent., and that this was accompanied by a diminution of 20 per cent. in the size of the rift of the stomates. It is suggested, therefore, that there is a correlation between the two processes.

The investigations upon transpiration may now be considered.

In the first instance, a determination[1] of the transpiration of plants of *Salicornia* under natural conditions was carried out, the plants being selected in order to obtain comparative results.

Sods were cut bearing:

(*a*) Apple-green plants only.

(*b*) Crimson plants only.

(*c*) Mixed growths of apple-green and crimson plants.

In the two former cases the sods were ready for experiment, and in the last case the apple-green or the crimson plants were removed from the different sods, pure cultures thus being obtained.

Gravimetric methods were employed; the sods were cut to fit closely into zinc boxes, and all plants except *Salicornia* were removed. The surface of the soil was then covered with a layer of cacao butter in order that evaporation from the soil surface might be prevented. Weighings of the sods were taken as far as possible twice a day, and the differences, due to loss of water, tabulated. Readings of the wind-gauge, and wet and dry bulb thermometers, also were recorded.

At the conclusion of the experiment the plants were all healthy; they were then cut down, and the surface area in each case was estimated by (1) actual measurements, or (2) weighing a carefully measured moiety of plants of equal diameter and then calculating the area of the whole. The loss

[1] This and the following observation were made by Dr. S. E. Chandler, of the Imperial Institute.

of water from a certain surface-area of *Salicornia,* during regular periods, was thus obtained. The whole of the losses for each individual sod were then reduced to a percentage, *i.e.* to losses per 100 sq. cm. of plant surfaces, thus affording a basis of comparison.

The following chart (Fig. 2) sets forth the result:

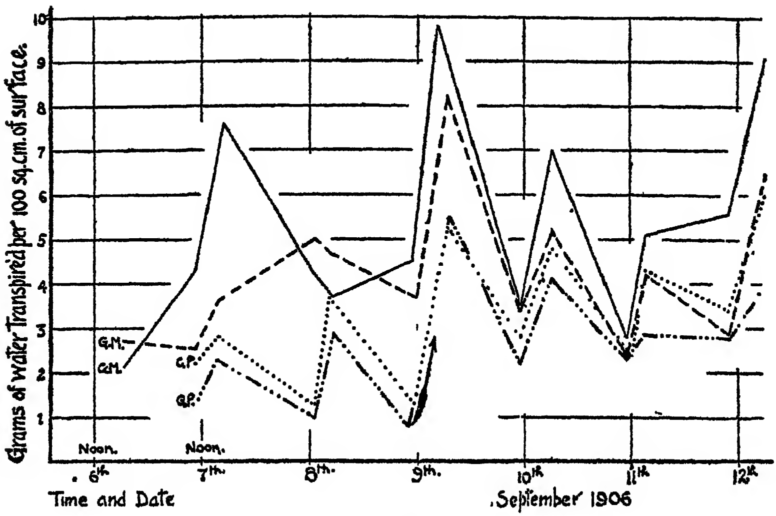

FIG. 2.

Showing the comparative weights, in grams, of water transpired per 100 sq. cm. of surface of (1) crimson *Salicornia* from mixed sod from which the apple-green had been removed (C. M. ————); crimson *Salicornia* from pure sod (C. P. • • • •); apple-green from mixed sod from which the crimson had been removed (G. M. — — —); and apple-green from pure sod (G. P. — • • • • —). All the sods were cut from the Crimson Plain.

The general results were as follows:

1. Crimson plants transpire more rapidly than apple-green.
2. The plants, whether crimson or apple-green, from mixed sods transpire at a greater rate than those from pure sods.
3. The crimson plants from a mixed sod transpire more rapidly than the apple-green plants from a mixed sod.
4. The crimson plants from a pure sod transpire more rapidly than the apple-green plants from a mixed sod.

It will also be observed that the diurnal variation of transpiration is well brought out in the chart, and that the amount of transpiration is surprisingly high.

The next experiments were made in order to compare the rates of transpiration of *Salicornia* plants growing in different

localities which had been shown to be interesting from other points of view. The localities chosen were:

(1) Crimson Plain and Bank.

(2) Ridge and Furrow.

The following chart (Fig. 3) sets forth the results:

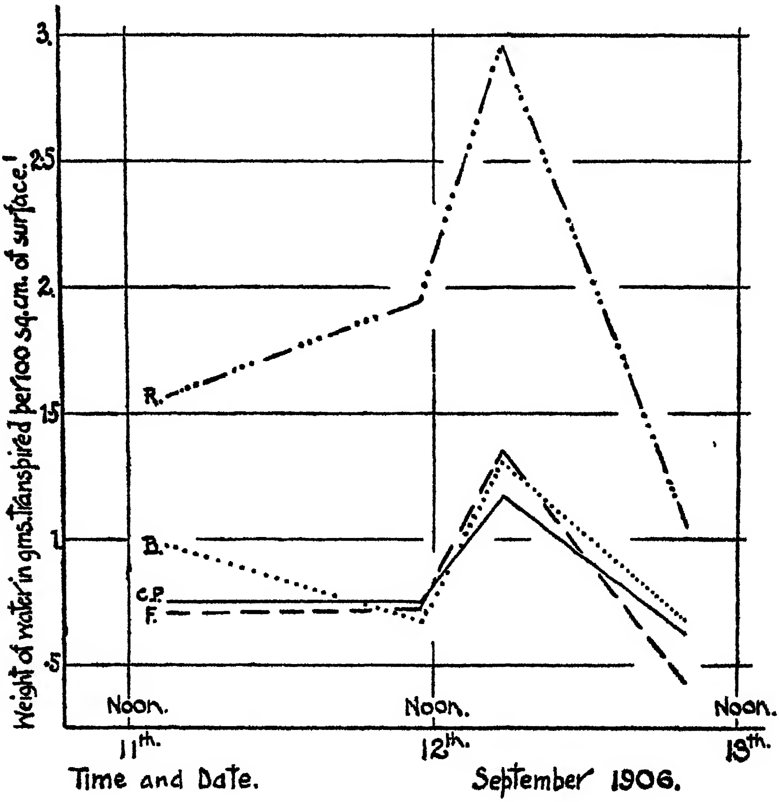

FIG. 3.

Showing the comparative weights, in grams, of water transpired per 100 sq. cm. of surface of *Salicornia* growing on the Crimson Plain (C. P. ————); *Salicornia* from the Bank (B. . . . . .); *Salicornia* from the Furrows (F. — — —); and from the Ridges (— . . . . —).

Summarising the results of this experiment, it would appear that:

(1) Plants from the Bank transpire more rapidly than similar plants from the Crimson Plain.

(2) Plants from the Ridges transpire at a markedly higher rate than those from the Furrows. It should be noted, however, that the Ridge plants were distinctly more crimson than those from the Furrows, though care was

taken to select those that resembled each other in external characters as nearly as possible.

In addition to the surprisingly high transpiration of *Salicornia* indicated by these experiments, the higher transpiration of the crimson plants as compared with the green plants is of particular interest especially in view of the various opinions which have been put forward with regard to the physiological rôle of anthocyan in plants.

Before passing on, it may be remarked that this method of measuring transpiration, the estimation of the loss of water by finding the decrease in weight, is the one which commended itself as being likely to yield the best results ; the use of the various kinds of potometer was precluded owing to the small size and succulence of the plants.

The experiments described above were followed by others of a more exact nature.[1]

Shoots of the red and green varieties of *Salicornia annua*, of *Suæda maritima*, and, for comparison, *Mercurialis annua*, a typical mesophyte, were set upright in shallow tins of melted wax which, on solidifying, supported the plants and also sealed the cut ends, so that any loss of water must have been through the intact aerial parts. The tins and their contents were weighed and placed in the open all under exactly the same conditions.

During the course of the experiment the tins were periodically weighed, and, when the plants had wilted, the areas of the exposed parts were estimated. The following table (IV) sets forth the results :

TABLE IV

	Loss of water per hour per 100 sq. cm. of surface.			
	Mercurialis annua.	*Salicornia* (green).	*Salicornia* (crimson).	*Suæda maritima.*
	gm.	gm.	gm.	gm.
1.	·066	·173	·226	·350
2.	·037	·286	·072	·062
3.	·022	·038	·027	·079
	·125	·497	·325	·491

[1] Delf, *Ann. Bot.* 25, 485, 1911.

It will be observed that the initial loss of water from the crimson *Salicornia* and from *Suæda* is higher than that from *Mercurialis* and the green *Salicornia*; this is due to the fact that the weighings of the two former were begun later in the day than those of the two latter plants.

The experiments show that the amount of water transpired from the halophytes varied pretty considerably, but in all cases it was greater than for a typical mesophyte; that the maximum rate of transpiration per hour was attained by *Suæda*; and that the transpiration of the crimson *Salicornia* was not higher than that of the green variety, a result which is contrary to those of Chandler's observations and which may be due to their being taken from a different locality.

If the succulence of a plant be taken as more or less equivalent to the water content per 100 sq. cm. of surface, then *Suæda* is the most succulent of the plants employed in the above experiment. This is shown by the following figures (Table V):

TABLE V

	Mercurialis.	*Salicornia* (green).	*Salicornia* (crimson).	*Suæda.*
Fresh weight	·560 gm.	1·811 gm.	1·345 gm.	2·240 gm.
Dry weight at 100°C. . .	·129 gm.	·190 gm.	·141 gm.	·219 gm.
Water content per cent. .	76·97	89·5	89·5	90·3
Surface area	45·14 sq. cm.	24·43 sq. cm.	17·44 sq. cm.	40·38 sq. cm.
Water content per 100 sq. cm. . . .	·95 gm.	6·5 gm.	6·1 gm.	10 gm.

It therefore follows that succulence is not necessarily bound up with a reduced transpiration.

Further experiments on these lines were carried out in departmental laboratories where more precautions could be taken than in the laboratory at Erquy.

Shoots of *Salicornia annua* and leaves of *Sedum spurium* and of *Vicia cracca* were selected, and the cut ends sealed with wax. They were suspended, by means of a loop of cotton, in air saturated with water vapour until their turn came to be weighed. After their weights had been noted they were suspended in the relatively still air of the laboratory, all under the same conditions, together with a weighed dish of water. The dish and the plants were periodically weighed. The following table (VI) summarises the results, in which the transpira-

tion values are considered in relation to water, the loss of water per 100 sq. cm. of surface being taken as 100:

TABLE VI

Relative transpiration per hour per 100 sq. cm. of surface

Sedum.	*Salicornia.*	*Vicia.*	Water surface.
36	32	26	100
30	28	14	100
27	36	9	100
17	26	6	100
18	23	4	100

These results corroborate the former and show that the loss of water from succulent plants is considerably in excess of that from certain mesophytes. It should be mentioned that in one respect the results shown in the last table are not strictly comparable, for whereas a shoot of *Salicornia* was employed, in the case of *Sedum* and *Vicia* the leaves were detached. It is not unlikely that these leaves transpired at a rate greater than would have been the case if they had not been removed from their respective shoots. But this does not militate against the sense of the results, for it makes the difference between the two non-halophytes—*Sedum* and *Vicia*—and *Salicornia* still greater.

ABSORPTION OF WATER BY THE AERIAL PARTS

In view of the saline nature of the soil of salt marshes, this high transpiration is surprising, and the observer is naturally led to consider whether the rainfall is sufficient to provide the roots with the requisite amount of water.[1] In fact, in some years the rainfall during the growing season may be so small[2] that sources of supply other than by the absorption of fresh water from the soil must be sought.

For the reason that in *Salicornia*, provided the plants are

[1] It may be remarked, that for the purposes of investigations such as these, the ordinary rainfall records are of but little value; for ecological work on marshes which are inundated by the tides, hourly records are desirable, for it is obvious that rain which falls immediately before or during the period of high tide is valueless in providing the soil with fresh water. At Erquy such hourly observations were impossible. For the present purpose it may be assumed that the whole of the rain which fell during the vegetative season was available for the plants.

[2] The following are records of the rain which fell on the Bouche d'Erquy during the vegetative season (April 1 to September 12) in the specified years: 1906, 3·69 in.; 1907, 5·06 in.; 1908, 4·38 in.; and 1909, 10·48 in.

not shaded and therefore drawn, the height is a general index of luxuriance, yearly observations on the average height of these plants from selected localities were made and compared with the rainfall.

In the majority of cases the relationship between the figures obtained did not warrant any definite conclusion. In some instances, however, there was a definite relationship, wherefore a more detailed examination was made.

The following are the figures for the locality C. 17:

Number of plants on the station in 1907	110
Average length	68 mm.
Average diameter	3 mm.
Total surface	700 sq. cm.
Loss of water per hour per 100 sq. cm. of surface[1]	= ·137 gm.
Loss of water in 3,960 hours[2] per 700 sq. cm. of surface	= 3797·64 gm.
Amount of rain which fell on station during the period	= 128821 gm.

Assuming that all this rain was available for the use of the plants, the amount was sufficient for the needs of, roughly, 35 times as many plants of *Salicornia* as occurred on the station.

If, however, station C. 8 and 9 be taken, the population in the same year was 5,970 plants, *i.e.* more than 50 times as many as on C. 17, which means that the amount of rain was less than the amount of water lost in transpiration.

Although these figures are approximations, the consideration of them points to the probability that sources of water other than rain obtain and possibly means of absorption other than the roots. It is well known that the leaves of ordinary plants can absorb water; it is the obvious thing, then, to ascertain whether the aerial parts of halophytes such as *Salicornia* can do likewise, and thus take advantage of rain, dew, or even the sea-water at the times of high tide.

Shoots of *Salicornia annua*[3] were removed and the cut ends sealed with wax; they were then dipped into water, their surfaces dried as well as possible, and weighed. The shoots were allowed to wither for some time, during which period they were occasionally weighed after being dipped into water and dried as before. At the end of the withering the shoots were immersed in water for a varying number of hours and were again weighed after being superficially dried.

[1] Based on Delf's observations, *Ann. Bot.* **25**, 485, 1911.
[2] From April 1 to September 12.
[3] Delf, *loc. cit.*

The following table (VII) sets forth the results:

TABLE VII

Surface absorption after withering

Withering.						Absorbing (during immersion).		
Plant.		Time.	Fresh weight.	Loss.	Loss.	Time.	Gain.	Loss recovered.
		hrs.	gm.	gm.	Per cent.	hrs.	gm.	Per cent.
Salicornia annua.	*a*	6	·552	·097	17·5	5	·035	35
	b	12	·628	·157	25	12	·094	59
	c	47	·172	·090	52	26	·049	53
	d	2	·232	·010	4	2	·006	60
	e 1	2	·212	·012	6	4	·008	75
	e 2	11	·229	·044	21	15	·054	108
Suæda maritima.	*a*	3	·391	·010	3	2	·029	290
	b	24	·552	·095	17	15	·018	20
	c	63	·297	·105	35	9	·027	26
Atriplex portulacoides	*a*	6	·283	·039	14	27	·052	130
	b	2	1·035	·007	·7	2	·029	400
Arenaria peploides		17	·392	·070	2	21	·175	250

NOTE.—The plant, *e* of *S. annua*, was given two successive periods of withering.

These results have been corroborated by Halket.[1] An even sample of plants was collected from one locality, the succulent parts were removed and weighed, dried at 100° C., and again weighed; from these data the amount of water in the succulent parts was calculated. The succulent parts of other plants were then immersed in water for certain periods of time, and the amount of water taken up ascertained.

In the case of *Suæda maritima* the percentage of water in the leaves of freshly gathered plants was 88·6 per cent., but after immersion in distilled water the amount of water rose to 90·4 per cent. Similarly in the case of *Salicornia* the initial amount of water present in the fleshy parts was 90·2 per cent. before, and 92·0 per cent. after immersion in distilled water.

In other experiments the plants were removed from the soil with their roots, as far as was possible, intact, and the roots, with the adhering soil, were well wrapped around with tinfoil. Then the fleshy parts were dipped into water, and, after as much water as possible had been removed with filter paper, weighed. The weight thus obtained was considered as

[1] Halket, *New Phyt.* 10, 121, 1911.

the initial weight of the plant. The plants were then suspended upside down in the liquid in such a manner that only the succulent parts could absorb water. At the conclusion of an experiment the succulent part of the plant was removed and the non-succulent parts—the base of the stem, the roots, etc.—weighed. Thus was obtained the weight of the succulent parts before and after immersion.

Numerous experiments showed that the plants could absorb water by their aerial parts. The amount thus taken up varied pretty considerably; the smallest increase, calculated as a percentage of the weight of the succulent parts of the plant, was 7·58 per cent., and the greatest increase 30·29 per cent. When the increase was calculated as a percentage of the weight of water in the succulent part, the smallest value obtained was 7·24 per cent. and the greatest 35·78 per cent.

It was also found that the amount of water absorbed was greatly increased if transpiration were allowed to take place after removal from the soil and before being immersed.

The value of this accomplishment to maritime plants, which often are subjected to drought, in enabling them to take advantage of rain and dew, needs no comment.

From these observations it may be expected that the aerial parts of maritime plants when inundated by the tides can also absorb sea-water provided that the osmotic power of the cell-sap of the epidermis and subjacent tissues is greater than that of the surrounding medium. Observations show that although the osmotic pressure of the cell-sap of the aerial parts of maritime plants may vary considerably it is, as in case of the root-hairs, very high.

For example, Halket,[1] who employed Barger's method, found that the osmotic pressure of the cell-sap of the succulent parts of *Salicornia ramosissima*, growing in a water channel, was equivalent to that of a 1·15 gram molecular solution of sodium chloride, which is equivalent to a pressure of 45·6 atmospheres at 18° C. The same species growing on higher and drier ground had an osmotic pressure equal to a 1·45 molecular solution of sodium chloride, which is equivalent to a pressure of 56·53 atmospheres. These plants are, therefore, sufficiently strong osmotically to absorb sea-water, and direct experiment shows that they can.

[1] Halket, *New Phyt.* 12, 104, 1913.

Delf[1] found that plants of *Salicornia* which were allowed to wither for 4½ hours, during which period they lost 8 per cent. of water, recovered 16 per cent. of their loss after 2 hours' immersion in a 3 per cent. salt solution.

Halket[2] found that in plants of *Salicornia* the percentage of water in the succulent parts rose from 90·2 per cent. to 91·6 per cent. after immersion in a 3 per cent. solution of sodium chloride. In subsequent experiments it was found that the increase of water, calculated as a percentage of the weight of the succulent parts, after prolonged immersion (17 to 20 hours) in a 3 per cent. solution of common salt, varied from 5·05 per cent. to 26·7 per cent. in the case of *S. ramosissima* collected at Erquy, whilst for a species of *Salicornia* from Blakeney the variation was from ·45 per cent. to 10·9 per cent. In the latter case the increase calculated as a percentage of the weight of water in the succulent parts varied from ·49 per cent. to 11·8 per cent. Also it was found that if the plants were allowed to lose water before immersion in the saline solution the amount taken up was greatly increased. Thus, for *Salicornia* collected at Blakeney the plants gained in weight from 20·8 per cent. to 53·7 per cent., again calculated as a percentage of water in the succulent parts of the plant at the time of immersion.

Both observers agree that the gain after immersion is less than the loss sustained during wilting, but in Halket's experiments the difference is very much smaller than in Delf's.

These experiments show conclusively that the plants considered can make good some of their losses of water from sea-water, although not to so great an extent as from fresh water. It may be argued that in some of the above experiments the gain may not amount to much since the time of immersion was so much greater than the time the plants would be covered by the tides. It has been found, however, that in cases of prolonged immersion the greatest gain is made in the early period of immersion, *e.g.* three plants of *S. ramosissima* after 5½ hours' immersion gained 20·69 per cent., 5·37 per cent., and 7·92 per cent. in weight respectively, whilst during the following 15¾ hours the gains were respectively 7·90 per cent., 2·78 per cent., and 3·65 per cent. And this obtains also when the

[1] Delf, *Ann. Bot.* **25**, 485, 1911.
[2] Halket, *New Phyt.* **10**, 121, 1911.

total time of immersion approximates more closely to the time the plants would, under natural conditions, be covered by the tide. This is shown by the following figures, which relate to a species of *Salicornia* from Erquy and represent the increase calculated as the percentage of water in the succulent parts at the time of immersion in 3 per cent. sodium chloride:

TABLE VIII

First period of immersion.		Second period of immersion.	
Time.	Increase.	Time.	Increase.
hrs. min.	Per cent.	hrs. min.	Per cent.
2 0	3·8	1 35	·8
2 25	4·6	2 22	3
2 0	3·6	1 30	2·9
2 30	12·8	2 10	1·7

It has been seen that the aerial parts of the plants under consideration can absorb both fresh and sea-water, and thus can take advantage of any rain, dew or sea-water which may wet such regions. The final question arises, Can they also utilise the aqueous vapour of the atmosphere? Experiment shows that they can. Delf [1] found that a shoot of *Salicornia annua*, the cut end of which was sealed, suspended in darkness in an almost saturated atmosphere, gained 2 per cent. in weight in 14½ hours, after which time no visible condensation of water had taken place.

Finally, it may be mentioned that during periods of drought some parts of the plant may be kept alive by the water from other parts. Thus, in *Suæda* it was found that the old leaves acted as reservoirs, yielding up their water to the more vigorous ones at the tip; in *Salicornia* [2] the contrary obtained: the youngest parts were the first to show signs of lack of water and the oldest last; thus in 24 hours, whilst the shrinkage in length during withering of the top internode was 11·9 per cent., the lowest internode showed no change.

[1] Delf, *Ann. Bot.* **25**, 485, 1911; **26**, 409, 1912.
[2] *Loc. cit.*

SOME PALÆOLITHIC PROBLEMS

By HENRY BURY, M.A., F.G.S.

For several years before the war a good deal of attention was paid to the geological position of palæolithic man, and considerable advance was made in the work of correlating our British results with those of continental observers; but some important discrepancies and gaps in our knowledge remained, and now that the end of the war is likely to induce a revival of scientific study, it may not be amiss to offer a review of our knowledge of the succession of industries, and of their relation to the changes that have occurred in river-level.

First of all, however, let us have a clear understanding of the kind of evidence we may expect geology to furnish as to the relative age of implements found in the various formations known as "drift"; for although this is elementary, it is sometimes ignored even by geologists, and being very inadequately treated in the principal text-books and popular works on palæolithic man, is a constant stumbling-block to those collectors who have not had a scientific training.

The gravels in which most of the early palæoliths are found rest on shelves or terraces on the slopes of our river valleys, and were for the most part laid down under water. They often attain a great thickness, and since the lower layers are the older (where there is any stratification at all), we may expect sometimes to learn the chronological sequence of the implements from the vertical succession of the strata. Unfortunately, when a river remains for long periods at approximately the same level, it does not simply pile up strata in vertical series, but swings from side to side of the valley floor, disturbing and rearranging its other deposits; and for this reason this source of information is only rarely available in a satisfactory form.

But another direction from which we may obtain chronological evidence is the succession of the terraces themselves; for since the valleys have been excavated by the rivers which run in them, it follows that the higher terraces are normally

older than the lower. Here, again, however, there are important sources of possible error, for the river does not continuously lower its bed, but sometimes raises or "aggrades" it, by depositing upon it masses of gravel, sand, etc., which it can no longer carry away; and that this process is sometimes very extensive and important may be judged from the fact that the Thames has at some period excavated a channel which extends at least 100 ft. below present sea-level, but is now almost entirely filled with drift. It is evident, therefore, that we must not too readily assume the deposit on a higher terrace to be necessarily older than that on a lower one; and in fact the same terrace might conceivably contain drift of three different periods, belonging to (1) the period of first erosion; (2) a succeeding aggradation; and (3) a final re-excavation; and if the alternation of erosion and aggradation has occurred more than once, as is not improbable, the record may be still further complicated. Seldom, however, will the older deposits remain undisturbed during later movements, and it is therefore possible that extensive fluctuations in level may sometimes have occurred without leaving any definite trace behind.

So far we have dealt only with true river deposits, but there is another series of beds which play an important part in palæolithic history, and which for want of a better term we may class together as "subaerial." Foremost among these come the various forms of "hillwash"—muds, sands, and other material washed down the hillsides by the direct action of rain or by the melting of frozen soils; while materials accumulated under the influence of wind are probably of less frequent occurrence, though they sometimes attain considerable thickness. It is evident that while true fluviatile beds show us the exact level of the river at the time of their formation, these subaerial deposits may extend to any height above it, spreading from top to bottom of the valley slopes, so that their elevation above the river is no indication of their relative age, and it is only when the deposits of one period differ in character from those of another (which is fortunately often the case) that we are able to determine their succession. This distinction between fluviatile and subaerial deposits is of considerable importance in the study of palæoliths, but it must be admitted that it is one that is often difficult to establish in practice; and indeed on theoretical grounds we should expect the two forms of deposit

to merge into one another. Bearing these elementary considerations in mind, we may proceed to examine the different archæological industries, and see what is known of their geological position and distribution.

I. The Lower Palæolithic Period

Strepyan.—At Strépy in Belgium, only a few feet above the present river, are found some rude implements, which are generally regarded as constituting the oldest group of palæoliths. In France, too, "pre-Chellean" implements of much the same character occur in the valley of the Somme, but at a decidedly higher level than at Strépy—namely, in the lowest gravel of the second or third terraces, at 30 and 40 metres respectively above the river-level. Our English evidence so far is more in accordance with the French than with the Belgian. It is true that at Swanscombe only flakes were found by Messrs. Smith and Dewey [1] in the lowest gravel of the so-called 100 ft. terrace (about 90 ft. above O.D.), but exactly as in the Somme valley, Chellean implements (which we shall deal with presently) were collected from another gravel a few feet higher, so that these flakes may reasonably be described as pre-Chellean.

Near Rickmansworth, again, the same observers found flakes of closely similar character,[2] which they regard as also pre-Chellean, but the discovery in the same gravel of an implement of apparently much later date throws a good deal of doubt on this opinion; if accepted, however, it is of considerable interest, since the gravel is here only 40 ft. above the nearest river, and therefore occupies a position about halfway between the Belgian and French levels.

Further research is certainly required into the distribution of this industry, but although implements of Strepyan type are not uncommon in English gravels, their exact horizon has not with any certainty been ascertained; and unfortunately the flakes have no particular character and can only be dated when definite Chellean implements are found above them. As we shall see later on, the discrepancies in level between English and continental deposits are by no means confined to this industry.

Chellean.—The next industry in chronological order is the

[1] *Archæologia*, vol. lxiv. p. 182.
[2] *Ibid.* vol. lxvi. (1915), p. 195.

Chellean. At Chelles itself (on the Seine), and in Belgium, implements of this type are found but little above the level of the river; but in the Somme valley they have a wider range, occurring on the first, second, and third terraces at elevations of 10, 30, and 40 metres respectively above the river, but they are absent from the fourth terrace (50 metres). While the implements of the second and third terraces are identical, some of those on the first terrace are of an apparently more advanced type, called by M. Commont *Chelléen évolué*.[1]

Both in level and in the character of its implements the 100 ft. terrace of the Thames at Swanscombe closely resembles the second terrace of the Somme, and it is not quite clear on what grounds Messrs. Smith and Dewey correlate it with the third terrace.[2] It is true that the *marne blanche* below the Chellean gravel at Abbeville (third terrace), which seems to correspond to the white marl between the Chellean and pre-Chellean gravels at Swanscombe, is absent on the second terrace at St. Acheul; but the same terrace at Menchecourt has a closely similar *marne sableuse légèrement colorée*, also containing shells[3]: on the other hand, no human work at all occurs at Abbeville either in or below the *marne blanche*, so that in this respect the parallel drawn by Messrs. Smith and Dewey is at present incomplete.

Although the gravel of the 100 ft. terrace rises to over 130 ft. O.D. at Dartford (and therefore to the level of the third terrace of the Somme), implements are extremely rare in that locality; but in the Hampshire basin (Bournemouth and the Avon valley) they are often found up to and above this level, though not on a separate terrace. Occasionally, too, they occur at a height of several hundred feet above the present river level, not merely on the surface, but, it seems, actually buried in gravel: but while it is doubtful whether these high-level specimens (to which we shall return later) can be matched in France, it is equally uncertain whether the Chellean and *Chelléen évolué* of the lower terrace of the Somme have their equivalents in England. Unabraded implements not infrequently occur at low levels (30–50 ft. above the rivers) both in the Thames valley and in Hampshire, but they have not

[1] *Ann Soc. Géol. du Nord*, vol. xxxix. (1910), p. 284.
[2] *Archæologia*, vol lxiv. p. 197.
[3] *Ann. Soc. Géol. du Nord*, vol. xxxix. (1910), p. 276.

yet been traced to any definite horizon; and the majority of the specimens from these altitudes are waterworn, and are therefore usually regarded as derived from the 100 ft. terrace. Here, again, further observations are desirable, and any evidence pointing to Chellean implements being contemporary with low-lying gravels should be carefully noted.

The recurrence of a pre-Chellean industry at two different levels, and of a Chellean at three, coupled with the frequently close parallelism between the geological deposits of the different terraces, offers a problem which can be solved in two different ways:

(1) *The Similarity of Industries and Deposits on any two or more Terraces indicates Contemporaneity.*—In this case each of the three deposits (two gravels and an intermediate shelly marl) on the second and third terraces at Menchecourt and Abbeville must in turn have reached a thickness of more than 30 ft., and then have been almost entirely removed before the next one was laid down. That is by no means impossible, but the coincidence of the three deposits behaving in exactly the same way at long intervals of time must be fairly faced.

(2) *The Similarity of Deposits indicates the Recurrence of a Geological Cycle*, probably under similar climatic conditions, but separated by an interval of time during which the river changed the level of its bed by about 30 ft. On this view, when the pre-Chellean industry left the third terrace (assuming that to be the earlier), it did not become extinct, but merely migrated, to be succeeded after an interval by the Chellean culture; and when suitable climatic conditions recurred, each industry in turn came back to the same latitude as before, but owing to the change in river-level, occupied a lower position.

The first hypothesis seems to be the one generally adopted, but in the absence of any conclusive evidence, it is well to bear the second also in mind as at least a possibility; and some further light is thrown on it by a comparison of the first and second terraces at Montières and St. Acheul respectively. In each case we find two distinct layers of gravel, overlaid by a clay or marl called by the workmen *terre-à-pipe*; but the similarity of deposits does not indicate contemporaneity; for the lowest gravel is pre-Chellean in one case and typical Chellean in the other; while the second gravel and the *terre-à-pipe* are

typical Chellean on the second terrace and *Chelléen évolué* on the first.[1]

Acheulean.—With the advent of this period the distinction between subaerial and fluviatile deposits becomes important, since according to M. Commont the series of beds (*limons moyens* or *loss ancien*) in which these implements occur in the Somme valley belong entirely to the former category[2]; and thus we are able to account, without postulating a series of earth-movements, for the fact that the industry is not confined to any one terrace but ranges from the lowest terrace up to the plateau above, 55 metres higher than the river-bed. M. Commont is of opinion[3] that at the close of the Chellean period, after the formation of the first terrace, a depression of the land occurred which gave rise to the raised beaches of Brighton, Sangatte, etc., and caused the deposition of marine shells on the second terrace at Menchecourt; and the *loss ancien*, with Acheulean implements, is assigned to the period immediately following this. The arguments, however, on which this opinion is based are not very convincing, and appear to rely too much on the supposed contemporaneity of beds containing Corbicula fluminalis at Menchecourt and under some ancient dunes; but as that mollusc is found in pre-Chellean gravel at Swanscombe,[4] and again in brick-earth at Crayford (about 40 ft. O.D.) associated with Mousterian flakes,[5] it cannot be regarded as a zone fossil of Chellean times.

Although Acheulean implements are classed by M. Commont as typical products of the first terrace,[6] they are absent from this level at several stations,[7] and appear to be more particularly associated with the second terrace, but not with its fluviatile deposits.

In Belgium the general succession of strata and their relation to Acheulean man accords closely with that of France, but M. Rutot regards the beds covering these implements as the relics of gigantic floods, and not of subaerial origin.

In England the evidence is still somewhat imperfect; but

[1] V. Commont, *Les Hommes Contemporains du Renne*, pp. 39–42.
[2] *Ibid.* pp. 28 and 38.
[3] *Ann. Soc. Géol. du Nord*, vol. xxxix. (1910), p. 207.
[4] *Archæologia*, vol. lxv. (1914), p. 192.
[5] *Proc. Geol. Assoc.* vol. xxv. (1914), p. 64.
[6] *Ann. Soc Géol. du Nord*, vol. xxxix. (1910), p. 292.
[7] *Ibid.* p. 208; *Les Hommes Contemporains*, etc., p. 42.

Acheulean implements were found by Messrs. Smith and Dewey[1] lying on the top of the middle (Chellean) gravel, and overlaid by another gravel which certainly appears to be a river deposit. At Wansunt, too, they are abundant in a clay or brick-earth, which the same authors[2] regard as the last deposit of the Thames at the 100 ft. level, but which Mr. Leach[3] believes to belong to a later period. More perplexing are the specimens alleged to have been obtained from the "shell-bed" at Ingress Vale. The similar bed at Barnfield Pit (Swanscombe), at practically the same level and only a few hundred yards away, is dated by Messrs. Smith and Dewey as pre-Chellean; and they found at Ingress Vale nothing but flakes apparently of that date. If, then, Acheulean implements can really be proved to have come from this horizon, we shall have to reconsider our whole chronology; yet it must be admitted that the evidence, while not eliminating all possible sources of error, is remarkably strong.[4] However this may be, it is certain that Acheulean implements occur at or slightly above the 100 ft. level in the Thames valley, at Ipswich, Farnham, Bournemouth, and elsewhere; and from their position and condition it seems that some at least of them were deposited in true river gravels or sands at that altitude.

But while in this respect English experience is different from French, yet just as in the latter country Acheulean implements are found on the plateaux bounding the Somme valley, so here, though seldom really common at more than 100–130 ft. above present river-levels, they range up to over 500 ft. O.D. on the Kentish plateau and at Limpsfield, while at Hackpen they reach 800 ft. O.D. or more. Many of these high-level implements occur in brick-earth which may be the equivalent of the *limons moyens* and *limons des plateaux* in France; and although we know nothing of its origin (some of it may be an eolian deposit modified by infiltrating water), there is no reason for asserting that it is necessarily older than the gravels of the 100 ft. terrace. But some implements have undoubtedly been found in gravel, and in this case two dif-

[1] *Archæologia*, vol. lxv. (1914), p. 189.

[2] *Ibid.* p. 204.

[3] *Proc. Geol. Assoc.* vol. xxvi. (1915), p. 20.

[4] *Archæologia*, vol. lxv. p. 196; *Proc. Prehist. Soc. E. Anglia*, vol. ii. pt. ii. p. 254.

ferent explanations are offered. One school points to the gravels of the Kentish plateau, in which these implements sometimes occur, as proved by their position and composition (containing as they do Greensand chert from the Wealden area) to belong to the very earliest stages in the excavation of the Thames valley; so that if the implements are really, as alleged, contemporary with the gravel, then they must be far older than the palæoliths of the 100 ft. terrace; and in proof of this they adduce a certain number of these "hill-types," recognisable by their colour, which have been found in the gravels of that terrace in such a condition as to prove that they are derived. If this view is accepted, it seems that both Chellean and Acheulean peoples have visited this country twice at widely separated periods, presumably in connection with cycles of climatic change.

Other authorities, however, while admitting the general antiquity of the high-level gravels, believe that they have been subject to much disturbance at later dates, quite apart from river action, and may in some cases have moved in large masses down the hillsides in a semi-frozen condition, burying implements which originally lay on the surface. The presence of hill-types on the 100 ft. terrace is also admitted, but since the highest gravel (upper gravel) there is shown by Messrs. Smith and Dewey to be a hill-wash of later date than the Acheulean implements, the occurrence in it of some of these specimens, washed from the plateau above, is by no means unintelligible. The same arguments apply to the Hackpen Hill implements, while the Limpsfield site is obviously one which might receive hill-wash of much later date than the old river-plain on which the gravels rest.

Near Farnham two waterworn Chellean implements are alleged to have been found at 600 ft. O.D., and as there is no higher ground from which they can have been washed, it seems hardly possible to account for their abraded condition by mere disturbance of an old gravel by frost or otherwise. If therefore they could be proved to have come from this level, it would be necessary to attribute to them an age far greater than that of the 100 ft. terrace; unfortunately, however, we have nothing but the testimony of the workmen, which can hardly be considered sufficient.

On the whole the evidence at present available seems in

favour of the second of the two hypotheses given above, and a good many facts can be adduced to show that extensive soil movements took place towards the end of the Pleistocene period, some time after the Acheulean industry of Swanscombe, Wansunt, etc. Further attention should, however, be directed to these high-level implements,[1] and among other questions to be settled, the proportion of Chellean to Acheulean types might with advantage be ascertained. On the 100 ft. terrace the former preponderate; but it seems probable that the proportion of the latter may be greater at high levels. If so, does it mean that Acheulean man was more given to wandering away from the rivers (beside which he probably lived) than his predecessor? Or have the implements been swept more than once by violent floods into the terrace gravels,[2] and was there an earlier (Chellean) sweeping more complete than the later (Acheulean)? The evidence as to the river-level in Acheulean times in France and England also merits further attention. At the close of the Chellean period, as we have seen, the river is said to have descended in France down to the first terrace (30 ft.), and it remained at or but little above that level throughout Acheulean times. In England, on the other hand, there is at present no evidence to show that it ever left the 100 ft. terrace throughout Chellean times, or even perhaps before the close of the Acheulean period; for there is certainly no definite connection between the latter industry and the 50 ft. terrace of the Thames. Mention, however, must be made of certain extremely scarce implements obtained at Farnham from a level which should probably be regarded as little if at all above the flood plain of the Thames. Though occurring deep down in, if not below the gravel, they are entirely unabraded, but while they would usually be referred to St. Acheul II., they differ markedly from the specimens of that type found 80 ft. higher up in the same valley, in that they are nearly all made of black or blue-black flint, which, not being a normal constituent of these gravels, suggests special quarrying. They often show also a difference in edge-clipping, and it is quite possible that they should be referred to the Mousterian period; otherwise we must admit a recrud-

[1] See Mr. R. A. Smith's paper, *Proc. Prehist. Soc. E. Anglia*, vol. ii. pt. iii. p. 392.

[2] *Ibid.* vol. i. p. 62.

escence of St. Acheul II. after a considerable interval of time, with but little alteration of form, but a marked change in the material used.

Leaving this case undecided, how are we to account for the association of the 100 ft. level with such much later types in England than in France? Are we to suppose that the industries actually arrived earlier in the former country, or that the emergence of the land was later? Or, again, can we find a geographical explanation? The change in river-level may have been due not to an earth-movement, but to a greater proximity of the sea; and since the North Sea and the English Channel were then distinct, and the one received the Thames and the other the Somme, we might suppose that this encroachment of the sea took place earlier in the Channel than in the North Sea. But against this supposition must be set the fact that all the Lower Palæolithic industries are associated in Belgium with a low river-level; so that it seems wiser to wait for further evidence before arriving at any conclusion.

As to the age of the raised beaches, which, as we have seen, M. Commont places at the end of the Chellean period, geologists are by no means agreed; but Mr. Dewey,[1] in one of the latest attempts to date them, regards them as pre-Chellean, the cemented sands which often overlie them being ascribed to the Chellean period; and since the shelf on which the beach rests is only 10–15 ft. above O.D., it follows that the main valleys must have been excavated almost to their present depth in pre-Palæolithic times. This agrees closely with the conclusion arrived at independently by M. Rutot,[2] who finds several "eolithic" industries underlying the Chellean in the Belgian valleys, only a few feet above the present river-bed; and if we accept it, then the land must have sunk considerably before the Chellean gravel of the 100 ft. terrace was deposited; it rose again, however, during or after the Acheulean period, and it is to disturbance of the old beach material at this second rise that we may attribute the Acheulean implement found in the Brighton raised beach.[3]

But before accepting Mr. Dewey's conclusion we must scrutinise carefully its foundations. Briefly stated, his argu-

[1] *Geol. Mag.* New Series, Dec. V. vol. x. p. 154.
[2] *Bull. Soc. Belg. Geol.* vol. xx. (1906), p. 27.
[3] *Proc. Geol. Assoc.* vol. xxvi. (1915), p. 3.

ment is this: over the shelf of the beach lie four deposits, indicating alternations of cold and warm climates. Beginning at the bottom they are (1) the beach itself (arctic); (2) cemented sand (warm); (3) "Head" or Coombe Rock (cold); (4) Boulder Clay (arctic). Various observers have associated the Chellean industry with a warm period occurring between the last glacial episode (Günzian, in Penck's scheme) and the last but one (Rissian); and since the fauna of the cemented sands agrees on the whole with that of the Chellean deposits of the Thames, and the Coombe Rock is known to be post-Chellean, it is assumed that the sands in question are of early palæolithic age. Mr. Dewey frankly admits that no implements have yet been found in the cemented sands; but he does not meet the other obvious objection that no contemporary Chellean implements have yet been proved to occur in river-drift at such low levels in this country. This is no place to discuss the large and difficult question of how far Penck's four glacial episodes are traceable in England, but a word must be said as to the relation of the Chellean deposits to the Chalky Boulder Clay. Mr. Dewey, in common with most English geologists, believes that the latter is the older; but although this conclusion is very possibly sound, the premises on which it rests have recently been challenged by Mr. Kennard,[1] and it must be admitted that they are of such an unsatisfactory character as to make it surprising that they have been so long and so widely accepted.

Reliance is usually placed on four localities: (1) Biddenham, (2) Hoxne, (3) Warren Hill, and (4) Hornchurch. In the first case the valley of the river Ouse is supposed to have been cut through Boulder Clay before the implementiferous gravels were deposited in it; but as Mr. Kennard points out, the evidence produced by Mr. Boswell and others that our main valleys are older than the Chalky Boulder Clay seriously weakens the argument: and the same may be said of Mr. Dewey's own conclusion that the raised beaches of Brighton, etc., only a few feet above O.D., are pre-Chellean. The low level of the gravels, too (about 40 ft. above the river), makes it doubtful whether they can be correlated with those of the 100 ft. terrace of the Thames.

At Hoxne the implements are mainly, if not wholly, of

[1] *Proc. Prehist. Soc. E. Anglia*, vol. ii. pt. ii. p. 249.

late St. Acheul date, and it is surprising therefore to find it quoted as proving anything about the age of the Chellean period; especially as M. Rutot,[1] and apparently M. Commont[2] too, place the Rissian glaciation between the Chellean and Acheulean industries.

The same may be said of the Warren Hill gravels, in which both Chellean and Acheulean implements occur, often abraded, and giving other signs of derivation. Dr. Allen Sturge himself, on whose paper Mr. Dewey relies, regards them as later than those of Swanscombe, and so far from their overlying the Chalky Boulder Clay, as Mr. Dewey states, proofs are given that that formation has been *pushed against and into the gravels* since their deposition.

Lastly there is the evidence of the Hornchurch section, in which gravel was seen lying on Chalky Boulder Clay at the level of the 100 ft. terrace, but no implements were found in it, and there is no evidence at all to show that it may not be the equivalent of the upper gravel at Swanscombe, which is admittedly post-Acheulean, and not a river deposit at all.

All these sections then are unsatisfactory, but greater weight perhaps attaches to the fact that so acute and painstaking an observer as Mr. Reid Moir has failed to find in the Middle Glacial gravels at Ipswich any industry but a pre-palæolithic one; but more of this negative evidence is required in view of the fact that in some regions (*e.g.* Rickmansworth) no line of demarcation can be drawn between the Middle Glacial and the Palæolithic gravels.

Of the many problems arising out of the geological position of the early palæoliths indicated in the preceding pages, none is of greater importance, or more peculiarly a task for British geologists, than this of determining the relation of the human industries to the glacial deposits; and however right the conclusions already arrived at may be, it is unfortunate that they should have been allowed to rest so long on insufficient premises.

II. The Upper Palæolithic Period

The Upper and Lower Palæolithic are often spoken of as the Cave and the Drift Periods respectively, and this is legiti-

[1] *Bull. Soc. Belge de Géol.* vol. xx. (1910), p. 88.

[2] *Les Hommes Contemporains*, etc., p. 15.

mate so far as it is an expression of the fact that most of our knowledge of the one series is derived from caves, and of the other from the drift; but the view is steadily gaining ground that, while implements of the latter series may be found in caves, most, if not all, of the principal cave industries are represented in deposits which we cannot logically separate from the drift; and so, if we adopt these terms, we reach the rather absurd position that drift continued to be formed and to enclose palæoliths long after the Drift Period had come to an end. The terms Upper and Lower Palæolithic are therefore decidedly preferable, though whether the line between them is rightly drawn at the close of the Acheulean industry is another question altogether.

Mousterian.—Implements of this period are found by M. Commont in the Somme valley at all levels from the plateau almost down to the river, but it is only on the lowest terrace that they are buried in true river deposits. Elsewhere they occur in the subaerial *dernier löss*, and are thus stratigraphically distinct from the Acheulean industry of the *löss ancien*. M. Commont, however, attributes to the Mousterian period a certain number of hand-axes which are not clearly distinguishable in his figures from those of Acheulean age; and one gets the impression, which an examination of the actual specimens might perhaps dispel, that the two industries are not so sharply separable geologically as he represents them.

In England also these implements are found at various levels, but in many cases they are buried in "brick-earth"—an ill-defined term which probably covers a number of deposits of different age and origin, some being certainly fluviatile, while others are probably subaerial; and this makes it very difficult to arrive at a definite decision as to the range of variation of the river-level during this period.

Several flakes attributed by Messrs. Leach and Chandler to this industry were found at Wansunt, in a clay which is certainly fluviatile, and which, as we have seen, is regarded by some writers as the last deposit of the 100 ft. terrace. If this diagnosis is correct, then the Mousterian and Acheulean industries were here co-existent; but great caution is required where only small numbers of specimens are present, since flakes were used also in Acheulean (and earlier) times, and no exact criteria for distinguishing them from those of Mousterian age

can be established. As is well known, the leading feature of this industry is the use of flake implements worked on one face only, in place of hand-axes worked on both faces; and in their most typical development these flakes have the striking platform facetted, and were struck off from a specially trimmed block of flint, often spoken of as a "tortoise core." Where then either facetted butts or tortoise cores are present, we may diagnose Le Moustier with some confidence, but their absence is a purely negative feature, and has but little value as evidence. On the whole, Acheulean flakes are thicker and clumsier, and the secondary edge-clipping is less perfect; but probably no two authorities would agree in drawing the line between them.

Messrs. Smith and Dewey were evidently not convinced of the occurrence of true Le Moustier in the Wansunt clay, and this, coupled with the doubt as to whether that clay really belongs to the 100 ft. terrace, makes it unwise to rely on this locality as proving the presence of the river at so high a level in Mousterian times. Many flakes of Mousterian character have been found at this altitude in other localities; but so far none of them have been traced to a definite fluviatile deposit.

One of the most prolific sources of these implements is Baker's Hole, Northfleet, where a flint factory was evidently established 30–40 ft. above the present river, but was afterwards overwhelmed by a mass of débris, which, probably in a semi-frozen state, swept down on to it from above. It is evident, therefore, that at this stage of the Mousterian period the river-level had already sunk far below the 100 ft. terrace. The Crayford brick-earth takes us a step further, for typical cores and flakes have been found underneath it at about 25–30 ft. above the river.[1] Now the Crayford brick-earth starts almost from the river-level and reaches up almost if not quite to the 100 ft. terrace; so that we have to deal with two long periods, one of erosion, during which the river-level descended from the 100 ft. terrace almost to its present position, and another of deposition, in which (assuming the brick-earth to be wholly fluviatile) it rose again to about 90 ft. O.D.[2] The sheet of gravel under the brick-earth contains no distinctive industry,

[1] *Proc. Prehist. Soc. E. Anglia*, vol. ii. pt. ii. p. 240.

[2] *Proc. Geol. Assoc.* vol. xxv. (1914), p. 69.

and half or more of the erosion period must have elapsed before the factories above mentioned can have been formed; so that so far as the Thames valley is concerned there is a considerable gap in our knowledge. Perhaps, however, this can be partly filled by some implements from Farnham, which are pronounced by Mr. Reginald Smith and Mr. Reid Moir to be of Mousterian age. They were found under a fluviatile gravel about 120 ft. above the river Wey, but there are reasons for regarding this as the equivalent of a 70 or 80 ft. level in the Thames valley. Although there are no cores and the butts are not facetted, the secondary edge chipping is typically Mousterian, and they differ both in sharpness and patina from the late Acheulean hand-axes found at the same level: both archæologically and geologically, therefore, they seem to occupy an intermediate position between the end of the Acheulean period and the fully established Mousterian industry of Northfleet, etc.

Mention has already been made of certain hand-axes, of late Acheulean form, but possibly Mousterian age, found in one of the lowest of the Farnham gravels. They are important as proving that the river reached this low level before the end of the Mousterian period, if not even in Acheulean times. The occurrence of the latter industry in such a position would be of great importance, but unfortunately the specimens are few in number, and until more are found it will be rash to come to a decision as to their age.

Many implements of undoubted Mousterian age have been found at elevations greatly above the 100 ft. terrace (*e.g.* by Mr. Worthington Smith at Caddington and elsewhere), and they are often buried under gravel; but there is good reason to believe that the gravel flowed over them at that subsequent period of intense cold and violent floods to which the Coombe Rock is due. It is commonly assumed that this cold spell drove Mousterian man (at any rate for a time) out of England; but it is not at all clear that he had not already ceased to occupy the Thames valley before the formation of the Crayford brick-earth; for while there is good first-hand evidence of Mousterian flakes having been found *under* that deposit, there is none at all (except that of workmen) that they occur *in* the brick-earth. The specimens obtained by Mr. Brice Higgins,[1] if they really came from above the floor, may quite well have

[1] *Man*, vol. xiv. (1914), Nos. 4 and 31.

been washed off the bank before the period of deposition commenced. This is a point collectors would do well to consider, for the brick-earth, with its bed of Corbicula fluminalis, testifies to tranquil waters and a moderate climate; and if it be true that it does not contain, but only covers, the Mousterian industry, we cannot attribute the disappearance of the latter to the severity of the climate.

It is possible that the Crayford brick-earth is represented in some other English valleys; though this question wants working out; but nothing of the kind, nor any evidence of a corresponding period of deposition, has been recognised in France.

Aurignacean, etc.—It is generally admitted that a great number of implements found on the surface, and therefore hitherto classed as Neolithic, resemble Cave types; but a doubt still exists, and indeed has been strengthened in some quarters by the discoveries at Grimes' Graves, as to how far the age of implements can be determined by form alone, without collateral evidence; and it is therefore highly desirable that every possible case of the occurrence of such implements in geological deposits should be examined and recorded with care.

In France, M. Commont succeeded in locating most, if not all, of the leading types of the Upper Palæolithic series in the highest Pleistocene deposit; but although he proved his specimens to be older than the Neolithic, it would be difficult from his evidence alone to establish their relation to one another.

In England, too, a fair number of specimens have been obtained from under sandy or alluvial deposits; but by far the most important work in this direction is that of Mr. Reid Moir near Ipswich.[1] He found there in definite sandy beds a lower industry of Upper Mousterian age, and an upper one of Aurignacean; above the latter, in a layer of "sludge," closely resembling true Boulder Clay, and like it referred to a cold period, were two early Solutrean implements; while in the uppermost layer of all was a Neolithic arrowhead. Thus in one spot were found no fewer than three of the main divisions of the Upper Palæolithic series, in exactly the order already determined in the French caves. Of the other interesting discoveries of this careful observer—human bones and pottery

[1] *Jour. R. Anthrop. Inst.* vol. xlvii. (1917), p. 367.

of Mousterian age—this is no place to speak; but we may permit ourselves to speculate as to whether the cold period which produced the "sludge" was the same one that gave rise to the Coombe Rock and buried the Mousterian industry of Baker's Hole, or a later one. And if the latter, whether Mousterian man migrated south during the Coombe Rock period, and returned, with pottery, later; for there is no sign at Ipswich of any cold spell during or immediately succeeding the Mousterian occupation.

In spite of all the labour and thought bestowed upon it, the question of how many cold episodes have occurred in south-east England is still far from being solved; but it is a curious fact, and one possibly connected with the climatic problem, that the frequency of occurrence of the various palæolithic industries in this country is practically in inverse proportion to their age.

SOME SCIENTIFIC ASPECTS OF COLD STORAGE

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PART II

In the previous article we dealt with the cold storage of fruit in order to demonstrate the type of cold storage in which no physical alterations of the material take place. We indicated that at present the cold storage of fruit does not allow much variation in method, but it should be realised that the main hindrance at present is the elementary state of plant physiological knowledge. When the physico-chemical aspect of metabolism in fruits has been subjected to an adequate investigation it will no doubt be found that considerable variation will be possible in regard to industrial methods.

In the present article we propose to consider especially the cold storage of meat, a question of the highest industrial importance and scientific interest, which introduces us to the second class of substances, those which can be preserved in the frozen condition. Nevertheless, large quantities of meat are also preserved at temperatures about 0° C., by which no great change in physical state is produced.

The possibility of the preservation of flesh foods by the use of low temperatures has been recognised and employed to some extent for hundreds and thousands of years by the inhabitants of cold countries, where by freezing meat and game in ice under natural conditions these food substances have been preserved for long periods.

The extreme case of this is that of the mammoth [1] and

[1] C. Lyell, *Principles of Geology*, vol. i. p. 182, London, 1872.

other prehistoric animals, which are still found preserved intact in a mummified condition. It is, however, only in recent years that the industrial possibilities of preserving meat by the use of low temperatures were realised, and the industrial development naturally only took place when it became possible to construct refrigerating engines for the production of the low temperatures necessary.

The principles of refrigerating machinery were worked out in the middle decades of the last century. It is outside the scope of these articles to deal with the great amount of work which led to the present development of this machinery.[1] The first vapour compressing machine appears to be due to Jacob Perkins, who patented an apparatus in 1834; but the first machine of this type which was of industrial importance was constructed by James Harrison in 1857, the machine working with ether. According to French writers [2] it was Charles Tellier who first used the refrigerating machine for the preservation of meat during transport. In 1868 he made an attempt to carry meat at a temperature of + 2° to 0° C. from London to the river Plate, but this attempt failed owing to a breakdown in the machinery. Financial difficulties consequent on the war of 1870 prevented an immediate repetition of the experiment, although in 1873 Tellier succeeded in demonstrating to the Académie des Sciences in Paris that the keeping qualities of meat were enhanced by lowering the temperature to 2° C. to 0° C., and that the food value was not lessened. However, in 1876 he showed the possibility of transporting meat in cold storage across the Atlantic by carrying thus ten carcasses of beef, twelve sheep, two calves, one pig and fifty birds from Rouen to Buenos Ayres, it being alleged that the cargo arrived in a perfect state. Unfortunately, the return journey was a failure. The twenty-one tons of meat carried were spoilt; whether this was due to the incapacity of the plant to deal with so much material is not quite clear.

Circumstances led to the withdrawal of Tellier from the enterprise, but the possibility of transporting perishable food products for long distances by means of cold storage had been demonstrated.

[1] See J. A. Ewing, *The Mechanical Production of Cold*, Cambridge, 1908.

[2] *E.g.* E. H. Amagat and L. Décombe, *La Statique des Fluides. La Liquéfaction des Gaz et l'Industrie du Froid*, Paris and Liège, 1917.

The French work on Tellier says:

"La voie était ouverte aux initiatives, pour récolter l'ample moisson semée par le génie français. Qui allait en profiter ?

"Ce furent les Anglais, qui, avec leur ténacité et leur splendide esprit de suite, reprirent la question." [1]

The significance of Tellier's experiment lies in the fact that a clear demonstration was made of the possibility of transporting food substances over long distances and that these distances might be unlimited. It is only natural that such a realisation should cause considerable commotion in the financial and commercial world. It is regrettable that the scientific aspect should have been entirely neglected, because development indicates clearly that the progress has been determined by successful financial ventures, and not on account of advances in science, and this unfortunate lack of balance between commercial and scientific development is probably responsible for the stagnation in the subject.

This may perhaps be better realised if one compares the meat industry with an industry originating at much the same time, the electrical industry. The basis of this industry is entirely the work of a few distinguished scientific men, but the whole development in the electrical industry gives clear evidence of an harmonious interplay between scientific work and financial venture.

The transport of meat in the *frozen* condition as distinguished from the merely *chilled* state, commenced in 1879, when the first shipment of frozen mutton was sent to Britain from New Zealand.

Yet, although the importation of frozen meat characterises the beginning of an entirely new epoch, it was initiated rather as a successful venture, and further development has depended on further successful ventures. Failures led, not to scientific investigations into the causes of failure, but to the banning of the experiment.

In the introduction of freezing as a method of food preservation, a method which involved physical changes of state in the material preserved, a scientific investigation should have been undertaken at once to determine how far the changes taking place on freezing could be made reversible on thawing,

[1] *Compte rendu officiel de la Manifestation Internationale en l'Honneur de Charles Tellier 'Père du Froid,'* Paris, 1913.

as on this would depend the completeness with which the material would regain its original fresh condition. By research on these lines it should have been settled what were the differences between chilled and frozen beef. As an example of how difficult it has been for new ideas to penetrate the indifference of a flourishing trade we may quote an experiment made by the firm of J. & E. Hall, of Dartford, on the application of brine freezing to the preservation of meat, a question which in the last few years has attracted much attention on the continent of Europe. As this British pioneer work has been completely neglected, we may be justified in giving here a brief reference to it. We are indebted to Mr. Hesketh for the following extracts from his diary:

"*January* 26, 1889.—An experiment was made on freezing mutton by direct immersion in brine made by common salt and water and kept at a very low temperature by a CO_2 refrigerating machine.

"*January* 28, 1889.—Mr. Marcet and Mr. Hesketh had an interview with Mr. William Cook, the London partner of Sansinenas, then one of the most important meat freezers at Buenos Ayres, at whose works Mr. Hesketh spent some six months in 1886.

"*January* 29, 1889.—Messrs. Hesketh, Godfrey and Marcet, the then partners in J. & E. Hall, lunched at the works off the mutton which had been frozen by immersion, which proved very good."

This application of brine freezing to meat as an industrial process was patented by Messrs. Hesketh and Marcet[1] in 1889, but the utter lack of interest in this country and elsewhere on the part of those concerned in the preservation of meat debarred any development of the process. So complete, indeed, has been the neglect of this early work that in a recent German paper[2] dealing with the freezing of meat in a salt solution the only reference is to Ottesen of Copenhagen, whose patent on this question[3] was not filed until as recently as 1912.

Even the Committee on Food Standards of the Association of Official Agricultural Chemists, in their definitions of meat, do not distinguish between chilled and frozen meat, since their

[1] British Patent, No. 6,117, 1889.

[2] R. Plank, "Über den Einfluss der Gefriergeschwindigkeit auf die histologischen Veränderungen tierischer Gewebe," *Zeitsch. f. allg. Physiol.* **17**, 221–38, 1918.

[3] British Patent, No. 24,244, 1912.

third definition is, "Cold storage meat is meat from animals recently slaughtered and preserved by refrigeration until delivered to the consumer." [1] In commenting on this definition, W. D. Richardson points out how unsatisfactory it is. He says: "The every-day distinction and scientific distinction also should be made between chilled meat and frozen meat, the former being held at temperatures of a few degrees above the freezing point until delivered to the consumer, and the latter in the solid frozen condition." [2]

It is also interesting to note that there is apparently no other official test for frozen meat than the examination of the blood, the red corpuscles of which have undergone hæmolysis in the case of frozen meat.

Nevertheless, in spite of the fact that the development of the cold storage industry, in so far as it concerns meat, has not been based on scientific research, a certain amount of scientific information having a bearing on the question has been collected. The principal constituents of meat, chemically considered, are water, proteins and fats. The lean of beef and mutton contains about 75 per cent. of water. Of the remaining 25 per cent., by far the greater proportion is protein, this amounting sometimes to 20 per cent. of the total weight in beef, the remaining 5 per cent. being composed of water-soluble organic substances (about 4 per cent.) and inorganic salts about 1 per cent. of the whole carcass. The relative amount of protein varies considerably in different individuals, especially in relation to the amount of fat, which in mutton may amount to three times the weight of nitrogenous substances.[3] Thus it is to be expected that the problem of preserving meat at low temperatures just above the freezing-point presents difficulties different from those in fruits, where the great proportion of the dry matter is carbohydrate.

The lean part of meat is composed of muscle fibres which are narrow and thread-like, the bundles of fibres being penetrated and supported by the connective tissue, which contains the nerves and blood-vessels. After the death of the animal

[1] See W. D. Richardson, "Meat and Meat Products," in *Allen's Commercial Organic Analysis*, vol. viii. p. 261, London, 1913.

[2] *Ibid.*

[3] See *e.g.* A. D. Hall, "The Book of the Rothamsted Experiments." Second Edition, pp. 250–54, London, 1917; also W. D. Richardson in *Allen's Commercial Organic Analysis*, vol. viii. pp. 262–7, London, 1913.

the first change which takes place is the setting in of the condition called *rigor mortis*, during which the muscle fibres lose their contractility. The soluble proteins coagulate, lactic acid is produced and the reaction of the meat changes from alkaline to acid. Later, the condition of *rigor mortis* passes away as the muscle tissue undergoes a fresh change, that known as "ripening," in which the muscle tissue relaxes, the meat at the same time becoming more tender. This change is brought about by proteolytic enzymes in the meat, and is to be regarded as a process of auto-digestion. At the same time, as the process of ripening is proceeding in the lean, other changes are proceeding in the fat. This latter undergoes hydrolysis if water is present, by which fatty acids and glycerol are produced, while, in presence of oxygen as well, lower fatty acids, aldehydes and other volatile substances are formed, producing the unpleasant condition known as rancidity.

These changes take place slowly at ordinary temperatures if the necessary external conditions (water and oxygen) are present, as under ordinary circumstances they always are. At temperatures of from 1° to 3° C. the maximum "ripening" effect in sides and quarters of beef is reached in from fifteen to twenty-one days, when a secondary complication has not affected the meat to any extent. This complication results from the invasion of the meat by micro-organisms (bacteria and moulds). The progressive decomposition of lean meat brought about by micro-organisms is that which is generally responsible for its becoming spoilt, the changes being described as putrefaction or decay according as they are produced in absence or presence of oxygen. The former changes are characterised by the production of unpleasant smelling substances; these are practically absent in the latter, but much carbon dioxide is given off.

Hydrolysis of fat may also be brought about by bacterial action, but is usually negligible.

The penetration of bacteria into meat takes place in two ways. There is a gradual and regular progress inwards of the original surface colonies, and a more rapid and irregular penetration along the connective tissue. In the case of the former, penetration at a temperature of about 2° C. is from 0·2 mm. to 1 cm. in thirty days; naturally the distance of penetration increases with the time stored.

By the second method the bacteria may reach the interior of the meat comparatively rapidly, and set up fresh centres of infection within the meat.[1]

Fungi produce a somewhat characteristic degeneration of the surface of the meat described as mouldiness.

The changes bringing about the deterioration of meat at ordinary temperatures are, then, chemical in nature. They are either brought about by the meat itself in conjunction with certain external conditions in regard to moisture and oxygen supply, or are the result of the action of micro-organisms (bacteria and moulds), living saprophytically on the meat. In the preservation of meat in cold storage the object is, of course, to prevent these changes as far as possible.

In considering how low temperatures influence these processes we must treat of chilled meat and frozen meat separately, as the problems involved are quite different in the two cases. Chilled meat is usually stored at temperatures from 0° to 3° C. At these temperatures all the changes which take place at higher temperatures also take place, but at a reduced rate. As in the case of fruit, scope for variation in the method is not great. The external factors which can be influenced are again light and humidity. As regards light it is generally held that meat should be kept stored in the dark, as the presence of light accelerates the actions resulting in rancidity of the fat. In regard to humidity, a moist atmosphere favours the growth of bacteria, while later, moulds appear on the surface of the meat in abundance. On the other hand, if the atmosphere is dry, water is lost from the meat by evaporation, but it is regarded as an advantage if the surface of the meat is harder and drier owing to water loss, as it renders the penetration of micro-organisms slower and more difficult.

Thus the principle involved in the preservation of meat at low temperatures above the freezing-point is the reduction in the rate of progress of a number of chemical actions. It is obvious that the changes leading to deterioration of the substance are not stopped, but are only slowed down, and consequently there is a limit to the time in which meat can be kept in this way without substantial deterioration. For sides and quarters of beef this limit is about thirty days, although sometimes the produce is kept for twice this time, or even longer.

[1] Richardson and Scherubel, *J. Ind. Eng. Chem.* 1, 95, 1909.

When meat is preserved by freezing the case is quite different. It is stated that if the meat is frozen to $-10°$ C. the growth of bacteria is absolutely prevented, since this cannot take place in a solid medium, while the chemical actions proceed at a negligible rate. Hence, frozen meat can be kept without deterioration for a much longer time than in the chilled condition; frozen beef can be kept for three years, or even more, without reduction in its nutritive value.

The physical changes taking place in freezing are those connected with the water relations of the tissue. As is well known from the appearance of frozen meat, water which was formerly bound in some way becomes free. This is particularly the case with beef, in which the amount of water lost under certain circumstances can be very considerable. For instance, Ascoli and Silvestri [1] mention a loss of 15 to 20 per cent. of the fresh weight of the meat. The juice which runs out contains valuable nutritive substances [2] and the quantities which can be lost in this way may be very considerable. The keeping qualities after thawing may be much reduced by this behaviour. Nevertheless, Gautier [3] found the composition of frozen meat the same as that of fresh meat, or that it even had a higher nutritive value on account of the water loss. As regards beef this process is to some extent reversible if careful thawing be applied.[4]

It seems very desirable to have an official gradation of frozen meat based on such an essential property as this binding of water.

In the concluding article we shall deal with the question as to the conditions under which these irreversible changes in regard to water relations can be minimised or avoided.

[1] See summary in *Le Froid*, Paris, Jan. 1914.

[2] W. Storp, *Über Gefrierfleisch*, *Veröffentlichen aus d. Gebiete d. Militär-Sanitätswesens*, Heft 55; *Arbeiten aus d. hyg.-chem. Untersuchungsstellen*, VI Teil, 51–73, Berlin, 1913.

[3] A. Gautier, *L'Alimentation et les régimes chez l'homme sain et chez les malades*, Deuxième Edition, Paris, 1904. See also Richardson and Scherubel, "The Deterioration and Commercial Preservation of Flesh Foods," *Journ. Amer. Chem. Soc.* **30**, 1515–64, 1908.

[4] W. D. Richardson, "The Cold Storage of Beef and Poultry," *Premier Congrès International du Froid, Rapports et Communications*, vol. ii. pp. 261–316, Paris, 1908.

(*To be concluded*)

CORRESPONDENCE

To the Editor of "Science Progress"

THE INTERNAL EAR

From Sir Thomas Wrightson, J.P., D.L.

Dear Sir,—The criticism of Professor Barton upon the theory advanced by me and supported on anatomical grounds by Dr. A. Keith, is one which we welcome as putting forward the contrast between the Helmholtz theory and our own in a definite form, and which calls for serious reply.

Professor Barton at the outset questions whether, in regarding the internal ear as a mechanism, we have "sufficiently borne in mind the high frequency at which it works." The answer to this is that our sensations are sufficient proof that some work has been passed through the cochlea, and that these sensations correspond to the main periods of the wave forms in the air as delineated by mathematicians. The high frequency with which we deal does not therefore appear to interfere with the perception of sound within the limits of audibility—which audibility incontestably proves that moving pressures have been transferred through the mechanism.

Professor Barton in paragraph *a* states that, "The resonance theory regards some particular part of a graduated mechanism in the internal ear as capable of sympathetically responding to the air-waves of correspondingly suitable frequency falling upon the external ear." This hardly exhausts the case, as a scientist wants to know—What is this capability? and how does it work? Some of our greatest intellects have demonstrated how the action of elastic vibrating bodies is carried through the air to the drum of the ear, but surely it is equally important to follow those actions into the liquids of the ear, which lie in the only route by which pressure and motion can reach the nerve terminations to be passed forward to the brain.

Professor Barton in paragraph *b* proceeds to describe his

understanding of our views with regard to impulses and analysis. With regard to the former he does not allude to the effect of the initial and varying elastic resistances caused in the basilar membrane, the ossicular muscles, the bending hairlets, the stalk of the tectorius, etc., all of which oppose the pressures of the air-wave in accordance with Hooke's law. In our book all this is investigated. It is also pointed out that by the mechanism of the cochlea, the sine curve in air at its position of highest amplitude where the change of pressure is most gradual, is by these resistances entirely altered in form.

In the sense of so altering the wave-form when it arrives in the liquid that the four impulses become equally definite, the cochlea may be regarded as having an analytic power and creates or makes accurately definite, certain impulses which are subjective and formed in the cochlea and cannot therefore be detected by resonators acting in the air.

Professor Barton next draws attention to our conclusions, " That because liquids are incompressible the liquids present in the cochlea must move in a piece and affect all the graduated mechanism equally and simultaneously," and asks what is there to prevent the various parts of this mechanism, and those only, responding which are in tune with the pressure wave arriving." This may appear simple when one tone alone is sounding, but the ear can detect many such tones sounding together.

Under such conditions where does the Professor's hypothesis for improving our theory land him ?

With a number of notes sounding together, the strings of the basilar membrane would move with different vibration rates in the different positions of the membrane, and therefore in constantly differing directions. What becomes of the displacement ? and how are we to explain that, while some of the strings are moving in one direction, due to the pressure of the liquid, other strings should move in the opposite direction to the pressure, which, according to the hypothesis, causes their movement ?

Further, what happens when the anatomists have demonstrated that all these so-called resonators are not free to move independently, but are cemented laterally together ?

Again, Resonance requires time to produce its effect. Mr. Sedley Taylor found by experiment that, with two tuning-forks

of 256 vibrations per second (about the pitch of middle C in the piano) about one second is required before resonance of the other fork becomes audible, that is, 256 air waves have to impinge on this resonating fork before its resonance accumulates sufficiently to become audible.

With a pair of forks making 1920 vibrations per second (about three octaves higher than in the first experiment) he found that he was unable to damp the first fork sufficiently soon after striking it before the resonance was audible in the second fork.

If the basilar membrane is a series of resonators it is clear that the lower pitched tones will, according to Mr. Sedley Taylor's experiment, take longer to receive audible resonance than the higher tones. How is it possible, under these conditions, to imagine that rapid arpeggio musical passages could be distinctly heard if so much more time were required for the lower notes to resonate than the higher?

These considerations have driven us to the conclusion that a dead beat transference of pressure and motion is the only solution which can satisfy this problem, and that we have to look for a series of actual impulses to be set up in the cochlea as the varied wave pressures produced in the liquid are conveyed through the corti arches direct to the hairlets.

These periodic impulses in many series are shown to exist in the compound wave-forms in the liquid, their maxima of pressure, whether positive or negative, with the crossing points at the axis of the pressure curve, furnish evidence of not only the periodic impulses of the original tones, but of the Differential and Summational Tones, Octaves and Harmonics.

Yours faithfully,

THOMAS WRIGHTSON.

II

THE ICE-AGE QUESTION

FROM H. SPENCER JONES, B.SC.

DEAR SIR,—In the last issue of SCIENCE PROGRESS appeared an article entitled "The Ice-Age Question Solved," by Major R. A. Marriott, D.S.O. In his article, certain conclusions of a geological nature were held to be in accordance with and

to be supported by Major-General Drayson's so-called "discovery," viz., that the pole of the heavens moves in a circle whose centre is 6° removed from the pole of the ecliptic, and makes a complete revolution in a period of 31,756 years.

With the geological facts I am not concerned. But for the benefit of those of your readers who do not claim to be able to express an authoritative opinion on astronomical matters, I think it is desirable to point out that astronomers generally do not accept Drayson's theory. Into the reasons for its unanimous rejection it is not necessary to enter here. Any geological conclusions, however, which are based upon it should be received with caution. Even if such conclusions were in accordance with purely geological evidence, it would not necessarily follow that Drayson's theory was correct. Astronomical and geological reasoning sometimes lead apparently to widely different conclusions, as in the question of the age of the earth. In such cases, it is reasonable to attribute the divergence of view to an incomplete knowledge on one or both sides of the underlying data.

Yours faithfully,

H. Spencer Jones,

Royal Observatory, Greenwich.

NOTES

Professor G. Carey Foster, F.R.S. (A. W. P.)

By the death of Professor G. Carey Foster, on February 9, 1919, science lost one who had taken a great part in connection with education and with physical measurements. He was appointed Professor of Physics at University College, London, at a time when physical laboratories in the modern sense did not exist. In 1866 he started the first in which students might repeat the standard methods of measurement which were then being rapidly developed (particularly on the Continent, where he had himself received the final part of his training). Other places very rapidly followed suit, for the spirit of change was in the air. Carey Foster (in his own words) looked upon his college not only as an important place of education, but also as an important expression of a most remarkable intellectual movement—a movement which stood for free inquiry and effort towards improvement, intellectual and social. He identified himself with all the movements towards making his university a great teaching university, as distinct from a mere examining body, and it was under the stimulation of two or three, of whom he was one, that his college ultimately, in 1907, allowed itself to be swallowed up in the university in order that it might, if possible, work the necessary reforms from inside. He was also a leader in the movement for giving women the same educational facilities and rewards as men. Although to many, at the time, it seemed a most hazardous step to take, yet it was clearly inevitable, and time has justified it. Those who knew him as a man will recall a singularly kindly personality, almost too shy and diffident to be genial, but living at peace with all: too conscious of the changes in scientific theories with the lapse of time to feel definitively bound to any, and with a strong sense that the revelations of experiments were the important part of scientific progress. In his writings he had the mastery in a remarkable degree over a lucid and logical prose.

The Mittag-Leffler Institute (Philip E. B. Jourdain, M.A.)

An account of the Mittag-Leffler Institute for pure mathematics has already appeared in SCIENCE PROGRESS (12, 647, 1918). The Institute was founded by money to be bequeathed after the death of Prof. G. Mittag-Leffler and his wife, and the original idea was that the working of the Institute should only begin after the decease of Prof. Mittag-Leffler. But now Prof. Mittag-Leffler, as I hear from him, wishes to see the beginning of this working, and has, therefore, handed over a capital sum to the Academy of Sciences of Sweden so that the activity of the Institute can begin at the present time on a modest scale. "I already have," writes Prof. Mittag-Leffler, "two scholars endowed with travelling scholarships, and will send them to England as soon as circumstances permit."

The Present Position of Wireless Telephony (Philip R. Coursey, B.Sc. (Eng.), A.M.I.E.E.)

ONE of the greatest obstacles in the way of successful wireless speech transmission, as distinct from mere telegraphic signalling, has been that of modulating the high-frequency power output of the transmitting station in accordance with the speech form, and of accomplishing this modulation by means of the minute energy associated with ordinary sound-waves in air.

The employment of the three-electrode valve—first invented in its earlier two-electrode form by Dr. J. A. Fleming, and subsequently developed by many workers—has greatly simplified the modulation problem. It has also at the same time provided a simple and extremely reliable form of high frequency continuous oscillation generator, suitable at present for moderate power work.

Under the stimulating influence of war-time requirements, the developments in the design and construction of these valves have progressed rapidly, so that their manufacture in quantity is now no very difficult matter. The first serious attempts at radiotelephony with the use of valves were made about 1914, and developments in America in that and the following year led, in October 1915, to the successful wireless transmission of human speech over a range of 5,000 miles from Arlington (U.S.A.) to Honolulu. The Atlantic was also bridged between Arlington and the Eiffel Tower at the same time. These tests were essentially of an experimental nature to test the possibilities of the valve for such purposes.

This, then, was the position of long-distance radiotelephony at the close of 1915. During the war no specially important attempts have been made relative to the use of wireless telephony over great distances, since the naval and military requirements have been of an essentially different nature. In general only very short-range work has been required. For these purposes the small valve transmitters have proved of very great value.

Wireless telephone sets have been developed for ship-to-ship work between the various units of a squadron, to secure rapid co-ordination of action and centralisation of control. The apparatus has also been used by the American Navy to enable the ship's captains to report direct to the Base Headquarters on shore, but of necessity the bulk of such traffic has been in the form of coded telegraphic messages. Undoubtedly the most spectacular application has been to aircraft, and for the control of such craft direct from the ground. The commanding officer on the ground can now issue his spoken orders direct to the pilots of the squadron some miles away, thus immensely simplifying the planning and carrying out of aerial operations.

The application of radiotelephony to aircraft is likely to be an increasingly important feature of peace-time flying, especially for maintaining touch with land during long-distance flights, and in conjunction with direction-finding apparatus to aid navigation. The tendency of recent developments of the telephone apparatus has been in the direction of reducing the power of the transmitter, and increasing the range by using more sensitive receivers with valve amplifiers. A recent achievement on these lines has been the respanning of the Atlantic by the spoken word from Ireland to Canada, by the Marconi Company, the object being to investigate the feasibility of commercial working.

The other important applications of radiotelephony must not, however, be lost sight of in the natural interest in new developments. As examples we may mention its uses for ordinary ship-to-ship, and ship-to-shore business traffic, for

communication with long-distance railway expresses, and in lighthouses for assisting ship navigation during fog. It therefore appears highly probable that we are on the threshold of great developments in these directions that should prove of immense advantage to the nations of the world in furthering friendly and beneficial intercourse.

Medico-political Union

This is a proposed Union to further the interests of British practitioners, and this *Quarterly* is in entire sympathy with it. Broadly speaking, the policy is so shaped as best to safeguard the interests of the general practitioner, and, as three-fourths of such in this country are on the Panel, and, therefore, have common interests, and likely to be aiming at a common end, the activities of the Union have been, and will continue to be, mainly directed by considerations of their welfare. . . . The following clauses constitute the principal objects of the Union :—

(*a*) To impose restrictive conditions on the conduct of the trade or business of medical practitioners.

(*b*) To regulate the relations between medical practitioners and their employers and between medical practitioners and medical practitioners.

(*c*) To regulate and control the conditions of contract practice by medical practitioners.

(*d*) To promote or oppose legislation which may affect the interests of members of the Union.

(*e*) To take active part in all lawful political propaganda in the interests of the medical profession.

(*f*) To further the interests of the medical profession in all legitimate ways, and to employ the funds of the Union for such purposes.

All communications should be sent to The General Secretary, A. Welply, Esq., M.D., 14, Gray's Inn Square, W.C.1.

The League of Red Cross Societies

A very important project in connection with the sanitation of the world has recently been suggested. A League of the Red Cross Societies or organisations of Great Britain, the United States, France, Italy and Japan has been formed and those of twenty-four other countries (including India, Canada, South Africa, Australia and New Zealand) have been invited to join. A part of the scheme of work of the League is the formation of a bureau for the purpose of co-ordinating sanitary knowledge throughout the world, of giving information on sanitary matters to those who desire it, and possibly of helping with funds where necessary or feasible. We hope to be able to give more definite information later, but at present the scheme is still in its infancy. Early in April a Conference of leading medical experts was summoned at Cannes to consider how best the League could help with reference to each particular disease, and the Articles of Association of the League of Nations contain a clause enabling the nations to use this new hygienic weapon if they wish. We are glad to see that Lieut.-General Sir David Henderson, K.C.B., has been appointed Director-General of the League. The supreme

Economical Council in Paris has already requested the League to suggest such measures as are possible to deal with anticipated epidemics that threaten Europe from the East, such as typhus, plague and cholera. We can foresee that the League may be able to do great work in the future.

The Royal Horticultural Society

This Society is appealing to the general public for support, as its administration expenses have greatly increased owing to the war. During these years of war it has been conspicuous for the help it rendered to the country. From the first day of the commencement of hostilities in 1914, long before the Government recognised the need of increased production of home-grown food, it took steps to organise, direct and encourage this movement. All communications should be sent to the Secretary, the Rev. W. Wilks, M.A., V.M.H., Vincent Square, Westminster, S.W.1.

To Research Workers in Physics

The Royal Academy of Science and Literature of Denmark announces that a Classen prize of 800 crowns will be awarded for the best account of an experimental investigation of the mode of action and properties of microphonic contacts received by October 31, 1920. It is stated that hitherto nearly all the work on this problem has been carried out with carbon-carbon "electrodes" in air at atmospheric pressure, and it is suggested that by using other gases, other pressures and "electrodes" of metal and carbon, or even metal and metal, a true explanation of the phenomenon may be obtained.

Notes and News (D. O. W.)

As is usually the case, the names of scientific men were not very conspicuous in the "New Year's" Honours list, published last April. Knighthoods were conferred on Prof. R. A. Gregory; Mr. J. T. Hall, organiser of the section dealing with the production of fertilisers in the Ministry of Munitions; Prof. W. Ridgeway, Professor of Archæology, University of Cambridge; Dr. C. S. Tomes, F.R.S.; and Dr. J. T. Verrall, Chairman of the Central Medical War Committee.

Sir Joseph J. Thomson, O.M., President of the Royal Society and Master of Trinity College, Cambridge, has been appointed by an Order of Council, dated May 8, 1919, to be a member of the Advisory Council to the Committee of the Privy Council for Scientific and Industrial Research.

Sir Ronald Ross has been elected a member of the Royal Society of Science, Upsala, Sweden, in succession to Sir William Gowers.

Prof. J. W. Nicholson has been awarded the Adams Prize of the University of Cambridge.

The Iron and Steel Institute has awarded the Bessemer Medal this year to Prof. Cav. Federico Gioletti, of Turin.

Sir Arthur Newsholme has been offered the Chair of Public Health at the Johns Hopkins University, Baltimore.

Among those whose death has been announced during the last quarter we have noted with deep regret the following: Sir W. Crookes; Prof. C. L. Doolittle, Emeritus Professor of Astronomy at the University of Pennsylvania; M. Jacques Danne, editor of *Le Radium*; Dr. F. Ducane Godman, F.R.S.; Horace Fletcher, the expert on dietetics; Prof. A. M. Liapounoff, Professor of Applied Mathematics in the Petrograd Academy; Lieut.-Col. A. M. Paterson, Prof. of Anatomy in the University of Liverpool; Mr. D. Rintoul, Head of the Physics Department, Clifton College; Prof. J. J. Schloesing, of the Conservatoire des Arts et Métiers; Sir E. C. Stirling, C.M.G., F.R.S., Professor of Physiology at the University of Adelaide; Lieut.-Col. W. Watson, C.M.G., F.R.S., Director of the Central Laboratory, B.E.F., France, and Professor of Physics at the Imperial College of Science, London; Dr. H. Wilde, F.R.S.

It is announced that Cambridge has received the promise of a sum of 200,000 guineas from a few firms engaged in the oil industry to enable her to build a new School of Chemistry. The University has also received £20,000 from M. Émile Mond for the establishment of a Francis Mond professorship of aeronautical engineering, in memory of Lieut. Mond, who was killed in action in 1918.

The Government has expressed the intention of spending £2,000,000 during the next five years on agricultural research and education. It is hoped to increase very largely the number of workers researching on agricultural problems, to encourage higher agricultural education in colleges, and to instruct the farmer as to the benefits which result from the application of scientific knowledge and methods to his work.

At a special general meeting of the Geological Society, held on March 26, it was decided by 55 votes against 12, to admit women as Fellows of the Society.

The British Scientific Products Exhibition, organised by the British Science Guild, will be held in the Central Hall, Westminster, from July 3 to Aug. 5.

It is announced that Mr. J. L. Cope, who accompanied the Ross Sea party of the Imperial Antarctic Expedition, 1914–17, as surgeon and naturalist, will lead another expedition in the ship *Terra Nova* to the same regions in June 1920. It is anticipated that the expedition will last from four to six years, and a comprehensive programme of work has been drawn up, including (1) an investigation of the mineral deposits in Antarctica, (2) observations on the distribution and migration of whales with a view to the assistance of the British whaling industry, (3) the mapping of the unknown coastline between Wilkes Land and Coats Land by aeroplane and sledge journeys, (4) meteorological and magnetic observations during the winter on the middle of the Ross Barrier. The expedition will be furnished with a wireless outfit to enable it to keep in touch with the rest of the world, and should result in a valuable increase in our knowledge of a little-known part of the Antarctic regions.

A strong appeal is being made for a sum of £50,000 for the foundation of a Geophysical Institute at Cambridge. There is at present no provision for geodetic research anywhere within the Empire, and, in the past, it has been necessary for the British Surveys, when confronted with geodetic problems, to obtain aid from the Geodetic Institute at Potsdam. Included in the estimate is the sum of £20,000 necessary for the endowment of a Chair of Geodynamics to be held by the Director of the Institute. The appeal is made by a committee, which includes Sir Horace Darwin, Sir F. W. Dyson, Sir Joseph Larmor, Sir Charles Parsons, Sir Napier Shaw, and Sir J. J. Thomson, and is being supported by the Conjoint Board of Scientific Societies.

The National Council of the U.S.A. has had placed at its disposal by the Rockefeller Foundation a sum of $500,000 to be spread over five years for the promotion of fundamental research in physics and chemistry in educational institutions in the United States. The first intention of the donors is the initiation and maintenance of a system of National Research Fellowships to be awarded to those who have shown themselves to possess a high order of ability in research. They will, for the most part, be awarded to persons who have had training at an American University or Scientific School equivalent to that represented by a doctor's degree. The research fellows will, in the first instance, be appointed for one year at a salary of $1,500, but will be eligible for successive reappointments with increases of salary. It is hoped to award from fifteen to twenty fellowships this year.

In a letter to *Science* (Feb. 14, 1919) Prof. George Sarton announces the reappearance, next autumn, of the *Isis* (an international quarterly devoted to the history and philosophy of science), which has not been issued since 1914, owing to the fact that the sixth part was in the press in Brussels when war broke out. After the parts already prepared have been issued it is proposed to use only two languages, French and English, mostly the latter, and Dr. Charles Singer will then share with Prof. Sarton the responsibilities of editorship. In future, also, *Isis* will contain only the shorter articles; the longer ones are to be included in Dr. Singer's *Studies in the History and Method of Science*, of which the second volume is now in the press.

The Year Book of the Scientific and Learned Societies of Great Britain and Ireland for 1918 (Griffin & Co., 9*s.* net) is even more complete than usual, as it gives information concerning twenty-six societies not hitherto mentioned, and has been extended to include musical societies. This extension, however, has led to some rather incongruous inclusions; for example, the precise reasons for classing the Glasgow Ballad Club (with its headquarters in a restaurant) with the "Learned and Scientific" are not exactly obvious! On the other hand, the National Union of Scientific Workers has been overlooked, though not so the recently formed British Association of Chemists. The Röntgen Society is classified under the heading Chemistry and Photography, and not under Physics, or even Medicine, where one might naturally expect to find it. Incidentally the publishers might consider the possibility of separating the first of these sections into its two parts, the disparity between the two, from a scientific point of view, being so very considerable. This issue of the Year Book contains, of course, the usual features, *i.e.* the prospectus of each society, and a list of the papers read during the year.

The report of the General Committee of Chemical and Allied Societies dealing with the question of the publication of chemical bibliographies in the English language, recommends that steps should be taken forthwith to render English-speaking chemists altogether independent of the German compendia. In support of their suggestions the Committee urge (1) that though the German publications have been continued during the war period their export is prohibited, and this prohibition may be maintained; (2) that their compendia are not impartial, the survey of original work performed outside Germany being very incomplete; (3) that in other countries, *e.g.* Italy, technical students learn German, rather than English, owing to the superiority of the technical literature published in that language, the result being that their sentiments lean rather towards Germany than to ourselves; finally (4) that the study of German in the schools in this country has declined so much during the war that comparatively

few of the younger generation will be able to read it. The Committee has drawn up a scheme for an English compendium in 53 volumes, each containing 1,000 quarto pages and costing £2 2*s.*—an amount estimated to cover expenses if an edition of 2,000 copies be subscribed for. The sum involved is, of course, rather large, but it becomes considerably less imposing when it is remembered that German chemists have recently (before Nov. 11 last) subscribed £125,000 for the further extension of their literature. It is proposed that the Organic section, in 18 volumes, should be arranged on the lines of Stelzner's *Literatur-Register*: the Inorganic section, which would require 16 volumes, would similarly be based on Abegg; while the Physico-chemical volume would be of the form adopted by Landolt and Bernstein in their *Tabellen*. The 18 volumes which remain would be needed for an abstract of the English Organic patents. These would appeal to a much smaller public than the first three sections, and the Committee are of the opinion that the question of its publication should be deferred for the present. The Committee further suggest that chemists in France and the United States should be invited to co-operate in the scheme, but it is not impossible that these will consider that they should have been approached before its format had been decided on, especially as the Physico-chemical tables which the Committee desire to have published are even now rendered unnecessary by the Annual Tables published by a Committee appointed by the seventh Congress of Applied Chemistry held in London in June 1909, and which receive financial aid from the Governments of England, France, and the United States. It is not, indeed, at all clear why these tables should have been ignored in the report, for at least one of the signatories is also a member of the Annual Tables Committee. Surely it would have been much more fitting if the work and organisation of this latter Committee had been made the basis of the larger scheme. A larger circulation would thus have been ensured, and the standard of the work would be raised owing to the wider choice then possible in the selection of the compilers. Indeed, it is by no means certain that a sufficient number of these, with the necessary qualifications, could be found in England alone. It is not impossible that the pre-eminence of Germany in this class of work in the past was largely due to its suitability for the national temperament.

The report of the General Education Board of the U.S.A. for 1917–18 contains an account of an investigation of the technique of handwriting carried out by means of the cinematograph. The analysis of movement by this method has been used for several years for speeding up manufacturing processes and minimising fatigue with remarkable results, and it would seem that it may be equally successful in its application to penmanship. An examination of the motion pictures of the movements of good writers, poor writers, and writers who had received different kinds of training, showed that certain features of position and movement were of considerable importance, and others much less so than is commonly supposed. A teaching method has been organised on the basis of the results obtained, and a year's trial in a public school in Chicago has shown the new method to be full of promise.

From experiments on the action of mustard gas (Dichloroethyl sulphide) on marine organisms, Messrs. Lillie, Clowes and Chambers have obtained substantial support for the theory, advanced by Marshall and Smith, that the gas penetrates the walls of living cells (in virtue of its high lipoid-solubility), and inside liberates hydrochloric acid, which, in free state, is relatively incapable of penetrating the cell. They suggest, therefore, that to obtain a remedy, search should be made for a substance whose physical properties, solubilities, and rate

of hydrolysis resemble those of mustard gas, but which yields on hydrolysis inside the cell a base, *e.g.* ammonia, instead of an acid.

We have received *Bulletins* Nos. 8, 9, 10 and 11 from the Advisory Council of Science and Industry, Australia. No. 8 contains the results of further investigations of the geology of the Bendigo Gold Field; No. 9 deals with the possibilities of the manufacture of ferro-alloys in the Dominion; No. 10 gives a most interesting account of the cardboard substitutes for tin-plate containers, with which we have become familiar during the last two or three years. The most important problem in their manufacture is the selection of suitable proofing materials for different classes of containers. Of the various substances available for this purpose (*e.g.* waxes, drying oils and resins, casein and gelatine, viscose and synthetic resins) the Committee of the Research Council recommend a Bakelite varnish which they call Magramite.[1] The cardboard container has first to be immersed in a 20 per cent. solution of carpenter's glue, to make the absorbent cardboard surface impervious to the magramite. It is then dried and varnished by dipping, painting or spraying, and finally it is stoved to harden the Bakelite surface and to make it insoluble and free from odour. Containers so prepared are not only suitable for foodstuffs, but also for such searching liquids as kerosene, turpentine, and methylated spirits. The last *Bulletin* contains a review of the possibilities of paper pulp manufacture in Australia. It appears that the prospects for materials native to the Continent are not too promising, and it is anticipated that for some years to come the principal material which would have to be used is straw. Apart from this, satisfactory results have been obtained by pulping young karri-trees, and a paper made from a blended pulp containing 80 per cent. karri and 20 per cent. *Gahnia decomposita* (a sedge which grows abundantly near karri-trees) is reported on very favourably. It is hoped that this may prove a partial solution of the problem of utilising the enormous quantities of waste karri at the saw-mills in Western Australia.

The second Report of the British Association Committee on Colloidal Chemistry and its general and Industrial Applications has been published by the Department of Scientific and Industrial Research (H.M. Stationery Office, pp. 172, 1*s.* 6*d.* net), as the British Association found itself unable to meet the greatly enhanced cost of production. On the industrial side the Report deals with the part played by colloids in the textile industry, in agriculture, in sewage purification, in the dairy, in physiology, and in medicine; while from its purely scientific aspect it discusses peptisation and precipitation, emulsions, the Liesegang phenomena and electric endosmose. Each page is so crammed with interesting information that it is quite hopeless to attempt a detailed review, but any one desirous of obtaining up-to-date information on this subject, which has become so amazingly important in modern industry, would be well advised to procure a copy forthwith; albeit, the Report is primarily intended for those engaged in research work in colloidal chemistry.

On March 14 Prof. R. W. Wood of the Johns Hopkins University, Baltimore, gave, before the Physical Society, a most interesting demonstration of two signalling-lamps he has devised for use in warfare. The first consisted of an electric lamp with a small filament placed at the focus of the object-glass of a telescope. This produces a narrow beam, visible only at points covered by the image of the filament when the telescope is sighted on them in the usual way. If the beam so directed crosses a line of enemy trenches the bulb can be

[1] Bakelite is a synthetic resin obtained by the interaction of carbolic acid and formalin.

covered by a dark red screen, the beam then obtained being visible, in daylight, only to an observer provided with a similar screen. For use at night a screen transmitting only ultra-violet rays could be used in place of the dark-red one, the observer having a screen of fluorescent material in the focal plane of his telescope. The second lamp was devised for naval purposes—to render the ships of a convoy visible to each other at night when no ordinary lights could be shown. The source was a Cooper-Hewitt mercury arc provided with a chimney of a special glass which transmits only one of the mercury lines (λ = 3660 Å. U.) and that beyond the limits of the visible spectrum. The light from the lamp could be picked up by means of a special telescope fitted with a barium platino-cyanide screen. In the course of his demonstration Prof. Wood showed some of the very remarkable fluorescent effects which could be obtained by means of this lamp; it is to be hoped that he will shortly put it on the market in a form suitable for lecture demonstration purposes.

Bulletin No. 3 of the Department of Scientific and Industrial Research is a paper by L. C. Martin, entitled "A Study of the Performance of Night Glasses." It describes experiments performed to determine the extent to which, under conditions of feeble illumination, the magnifying power of a field-glass or telescope may be increased at the expense of the reduction of the effective diameter of the exit pupil with the consequent reduction of the brightness of the image formed on the retina. The author concludes that for hand-glasses a magnifying power of 5 or 6, with the largest possible field of view, and an exit pupil of 7 or 8 mm. is the best. For stand instruments, provided the object-glass is not less than 3 inches in diameter, the magnification can be increased until the angle subtended by the image at the eye is 1°; but the magnification required to do this must *not be less than* 10. It is extremely important, however, if the best results are to be obtained, to screen the eyes from all brighter illumination. Thus, at dusk, when the sky is fairly bright, stray light entering the object-glass and reflected from the walls of the instrument may be fatal to the successful use of the telescope. This difficulty makes it undesirable to increase the magnification above 30 with a 3-in. glass, and, of course, such a large magnification is not suitable for a general survey of the landscape, but merely for the observation of particular small objects. Among the numerous facts brought to light in this investigation is that the sensitiveness of the eye to feeble illumination increases for *at least* one hour after it is first placed in darkness.

A joint meeting of the Röntgen and Faraday Societies was held on Tuesday, April 29, for the purpose of discussing the problem of the application of X-rays to the examination of materials opaque to ordinary light. While it is comparatively easy to obtain photographs through considerable thicknesses of substances of low atomic weight (and therefore of small absorptive power) such as aluminium and timber, very considerable difficulties have to be overcome when it is desired to obtain, in this way, information as to the interior character of iron or steel Nevertheless, encouraging results have already been obtained, and M. H. Pilon (the licensee for the Coolidge tube in France) announced that he had succeeded in dealing with thicknesses as great as 55 mm. by giving an exposure of 6,500 sec. with an X-ray tube, taking 6 milliampères at 120,000 volts. Limits of 40-45 mm. have been reached by several other workers.[1] The industrial importance of this

[1] In an article by F. Janu and M. Reppelen in *Stahl und Eisen* (which was translated for use at the meeting by Sir Robert Hadfield) it is claimed that flaws have been detected in steel of from 10–12 cm. in thickness; further, Sir Robert Hadfield stated that with new apparatus, now at work in this country, it is hoped

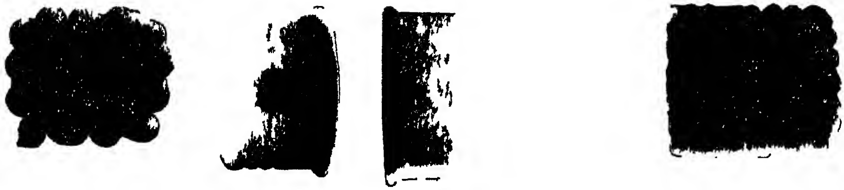


FIG 1 —X Ray photograph (Coolidge) of two loaded cartridge cases

In the first the outer tube of cardboard is quite invisible in the second the case and powder show faintly In both the detonator in the base of the cartridge will be noticed

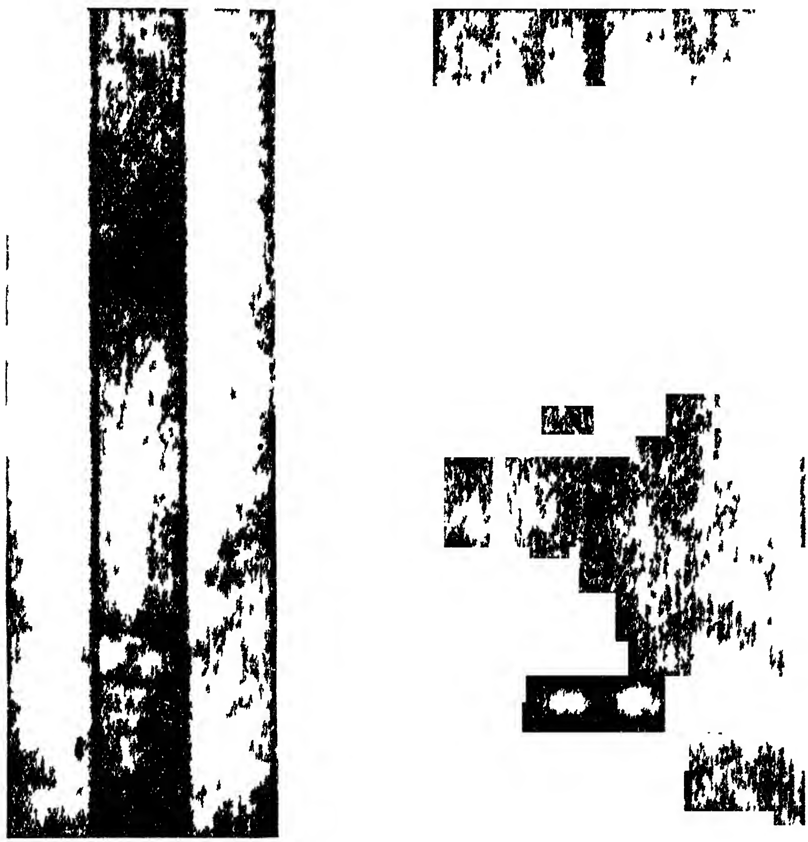


FIG 2 —X-Ray photograph (Knox and Kaye) showing front and side views of a laminated wooden spar of an aeroplane.

The spar is made up of three laminæ glued together The external appearance was excellent, but the radiographs show that the middle layer contains two knots and a grub hole and should not have been used

application of X-rays is sufficiently obvious. It has, for example, already been used for the examination of welds, for investigating the effect of aluminium in preventing the formation of blow-holes in cast steel, for the detection of weak points in castings whereby the method of casting may be altered until no such points are visible, and for the examination of iron embedded in concrete. The difficulties to be overcome fall under two heads—firstly, the production of rays of sufficient penetrating power, and secondly the prevention of fogging due to secondary radiations from the material and from the photographic plate itself. In the first place, the penetrating power of the rays (*i.e.* their wave length) should be adjusted to the thickness of the material to be traversed in order to obtain the maximum effect on the photographic plate, for it is desirable that the rays which penetrate the metal should not succeed in passing through the plate but be absorbed by it. The rays which get through the plate can, however, still be utilised by means of intensifying screens of lead or of calcium tungstate placed in contact with the back of the plate. With lead the electrons liberated by the X-rays intensify the action of the rays on the sensitive film, while the calcium tungstate gives out a blue fluorescence, and this visible light produces its usua effect on the silver bromide. This screen reduces the time of exposure to at least one-sixth, and under particularly favourable circumstances to one-thirtieth of the period otherwise required. To minimise the effects of the secondary radiations which arise when the X-rays are absorbed by ordinary matter it is necessary, when dealing with thick specimens of iron, to surround or embed the specimen in lead (*e.g.* by piling small shot round it), which must overlap the edges of the plate, to prevent "under-radiation" from its back or from surrounding objects.

A most important fact in connection with this work is that the *relative* thickness of the specimen examined and of the fault in it does not matter. Provided only that the rays penetrate the specimen and that the flaw is not thinner than a certain minimum, it will produce its proper effect on the plate, and sufficient contrast for visibility can be obtained by suitable exposure and development. Thus, Dr. Slade estimates that by using the K radiation of nickel with iron it should be possible to detect differences in the thickness of the iron of the order of 4×10^{-5} cm. In practice the tool-marks on the surface are easily visible. At the present stage of its development the process reaches its most striking application in the detection of faults in timber or of faulty workmanship in articles made thereof. At the meeting Dr. (Major) G. W. C. Kaye showed numerous examples of faults in aeroplane construction discovered by means of it. The absorption in this case is so small that fluorescent screens can be used for direct visual examination instead of photographic plates—a possibility which makes the process one of ordinary commercial utility. Two of the many photographs shown at the meeting are reproduced on the adjoining plate. Fig. 1 shows an X-ray photograph of two brass cartridges taken by Dr. Coolidge and lent to the writer by Prof. Bragg; the great absorption produced by the fulminate of mercury in the detonator will be noted. Fig. 2 shows one of the photographs taken by Major Kaye and Dr. Knox, and represents front and side views of a laminated wooden aeroplane spar made of three laminæ glued together. The external appearance

to penetrate through 9 inches of that metal. In Germany the Lilienfeld tube is used instead of the Coolidge tube. It requires three separate currents and permits of the independent variation of the heating current and of the driving potential which imparts to the cathode rays their necessary high velocity. No information as to its capabilities appears to be available in this country.

was excellent, but these radiographs show that the middle layer contains two knots and a grub hole. It should not have been used.

Notes and News (J. Brontë Gatenby, D.Sc.)

Among the many facts which the Great War has brought home to us, not the least disquieting is that which shows that the majority of the foreign element which has passed through our "ever open door," is not only of an inferior physical type, but also much diseased. This is one of the menaces to be dealt with, and we note that the "Eugenics Education Society," which numbers among its members men such as Havelock Ellis, Mott Schuster, and Leonard Darwin, is of the opinion that the time is at hand to begin to introduce reforms and enquiries. This Society publishes a journal known as the *Eugenics Review*, in which matters of interest to eugenists will be found. Communications may be sent to the Secretary, Eugenic Education Society, 11, Lincoln's Inn Fields, W.C.2.

We note the formation of an "American Society of Mammalogists," which will publish a new journal and devote its attention to the study of mammals in a broad way, including life-history, evolution, palæontology, anatomy and other phases. The application for membership may be sent to H. H. J. Jackson, U.S. Dept. of Agriculture, Washington, D.C., U.S.A.

ESSAYS

THE USES OF WEEDS AND WILD PLANTS (Winifred E. Brenchley, D.Sc., F.L.S., Rothamsted Experimental Station)

FROM very early times in history people have recognised that many of the plants native to any district can be turned to some useful account. In olden times the chief use of wild plants was in connection with medicine, and, as in those days superstition was rife, the majority of plants had some medicinal value attributed to them, provided they were gathered at the right time and under the proper auspices. Most of this herb lore is now lost, though traces of it still survive in out-of-the-way places. Various other local uses were made of wild plants, but, as methods of agriculture improved, and as cultivated plants were introduced from one country to another, the useful native plants became more and more neglected as their exploitation became less profitable in comparison with cultivated crops.

The war, however, has caused a considerable revival, and old and new uses have been found for native plants in the belligerent countries. Many of these are confessedly makeshift, and will be given up as soon as possible, but in a few cases valuable improvements have been effected, and it is probable that further advances will be made in the future.

Many of the uses suggested in this paper have been published in the Foreign Press during the war. As these apply to countries affected by the blockade, some amount of discrimination is necessary in estimating the actual value of the wild plants in these cases for the purposes quoted, as it would often prove most unprofitable to utilise them if ordinary supplies were available.

The uses that can be made of weeds and wild plants fall into several classes :

1. Human food, chiefly as substitutes for ordinary articles of consumption, and also as oil.
2. Fodder.
3. Fibres, which can be used to replace or supplement the ordinary fibres of commerce.
4. Fuel.
5. Manures.
6. Drugs.
7. Dyes.
8. Miscellaneous uses.

1. *Human Food.*—Wild plants are used for food in various ways, but in most cases they are only adopted as a temporary expedient, and when the emergency is over their use is discontinued, though there are a few outstanding exceptions to this rule.

The aromatic or appetising properties of many wild plants make them more or less efficient substitutes for tea, coffee and even cocoa when they are properly prepared. The root of chicory (*Cichorium intybus*) has long been used as coffee, either alone or as a mixture. In Belgium[1] pure chicory has been substituted for

[1] Hogg, R., and Johnson, G. W., *Wild Flowers of Great Britain*, 1863.

coffee, but in this country it is used as an addition, partly because some people prefer the mixture, partly because it makes a cheaper beverage. The roots are carefully dried, then roasted and ground into powder. During the war the value of chicory as a coffee substitute was so well recognised that in Germany and Austria the whole crop was reserved for this purpose, its use for feeding animals being prohibited. Another good substitute is prepared from hawthorn berries, and the coffee thus obtained has been reserved for German workmen engaged on heavy work, while in order to increase the supply people have been recommended not to clip their hawthorn hedges. In parts of the same country the fruit of dogwood (*Cornus sanguinea*) was used for a similar purpose long before the war. Other substitutes have been made from the rhizomes of couch-grass (*Agropyrum repens*) and bracken, the roots of salsify (*Tragopogon pratensis*), sowthistle, dandelion, and reeds, from acorns, beechnuts, horse-chestnuts, and broom seeds, a specially recommended mixture being one part of ground couch-grass rhizomes to three parts of dandelion root, which is said to have a pleasant aroma.[1] In Sweden the horny fruits of goose-grass (*Galium aparine*) have been dried, slightly roasted, and ground.[2]

An almost bewildering variety of plants have been laid under contribution as tea substitutes, leaves and flowers being the parts usually used. For many years the Russians have mixed the leaves of the narrow-leaved willow-herb (*Epilobium angustifolium*) with ordinary tea, and local use has often been made of aromatic parts of plants, as flowers of cowslip and primrose, leaves of sweet vernal grass (*Anthoxanthum odoratum*), agrimony (*Agrimonia eupatoria*), meadow-sweet (*Spiræa ulmaria*), and ground-ivy (*Glechoma hederacea*). German war substitutes have included the leaves of willow, birch, rowan-tree, willow-herb, bilberry, and cranberry, and the fruit of hips, the aroma being given by peppermint, walnut-leaves and lime-flowers. Tea mixtures sold in Germany also include leaves of lime-tree and hemp-nettle, fruits of agrimony, flowering heads of yarrow, hawkweed, and daisy, leaves of walnut, blackberry, borage, and plantain, flowers of primrose and marjoram, roots of mallow and red sandalwood.[3] Heather blossoms have also been strongly recommended as a tea substitute. They are gathered when the blooms can be separated from the stems by drawing through the fingers, and are dried by the sun or artificial heat, and are then sieved to separate them from stalks and dust. The flowers so prepared can be kept for years without losing their agreeable narcotic smell.

In Austria a substitute for chocolate has been prepared from dogwood berries, though this may possibly be the same thing as the coffee substitute mentioned above. The preparation is nutritive, and is said to have a vanilla-like taste and smell resembling that of chocolate.

Ground-ivy or alehoof (*Glechoma hederacea*) was much used in the manufacture of beer and other liquors until the introduction of hops, as the leaves clarify the liquor and give it a characteristic flavour.[4] About the time of Henry VIII hops were substituted for this purpose and ground-ivy fell into disuse, though it is sometimes dried by village herbalists and used instead of tea.

[1] Other coffee substitutes, not derived from wild plants, however, have been prepared from hyacinth bulbs, grape pips, asparagus seeds, and lupine seeds, the last named being called "Peasant's Coffee" in the Tyrol.

[2] Johnson, C. P., *Useful Plants of Great Britain*, 1861–2.

[3] The most satisfactory tea substitutes, however, are said to have been prepared from the leaves of strawberries, raspberries and blackberries.

[4] Hogg, R., and Johnson, G. W., *ibid.*

In almost all conditions of life the provision of flour for bread-making is of vital necessity. In normal circumstances the cereal crops meet this need, but, under special conditions brought about by failure of crops or imports, native substitutes have to be pressed into service. As a general rule, these substitutes are used to supplement the ordinary flour, but on occasion they have beenused by themselves. The rhizomes of couch-grass have been used in this way; in Italy a kind of arrowroot has been prepared from them,[1] and during the Thirty Years War and the Great War the rhizomes were dried and ground into flour for bread-making. Flour substitutes have also been prepared from the roots of sowthistle (*Sonchus oleraceus*) and silverweed (*Potentilla anserina*), and bread made from common sorrel (*Rumex acetosa*) is said to be used by the Swedish poor when the barley or rye crops are deficient.[2] The Swedish also claim to have been successful during the war in preparing flour from timothy (*Phleum pratense*), which was ground and mixed with rye meal in the proportion of 1:4. In the same country a company has recently been formed for the manufacture of "artificial flour" from such natural products as water-lilies, wild leguminous plants, and the roots or rhizomes of such plants as reeds and couch-grass. Heather is also utilised, a certain amount of albumen, carbohydrate and fat being added, giving a product that has proved very satisfactory for bread-making. Yet another Swedish substitute is said to be prepared from the leaves (needles) of coniferous trees, which, when mixed with equal quantities of rye-flour, yields a bread of softer and more porous texture than pure rye-bread, while it is free from any objectionable taste. Horse-chestnuts have been exported from Sweden to Germany for the preparation of flour by special methods of treatment.

Serviceable flour substitutes can be prepared from the seeds of certain weeds, especially from spurrey (*Spergula arvensis*) and species of *Chenopodium*. In northern countries, as Finland and Norway, spurrey grows freely and well, and the seeds have often been ground and mixed with wheat or rye-flour.[3] In South America a species of goose-grass (*Chenopodium quinoa*), has been cultivated for centuries as a bread plant, as the seeds can be ground into a good flour.[4] Of recent years the plant has been introduced into Germany for the same purpose, with considerable success. The common weed fathen (*Chenopodium album*) is very closely related to the quinoa, and occasionally its seeds have been used in a similar way when emergency has arisen, as in the Russian famine of 1891–2.[5] Lichens have also been exploited abroad for bread-making purposes, the most useful being those of the *Cetraria* family, including Iceland Moss and varieties that the Scandinavians call "Snölav" and "Trälov." The colouring matter and bitter substances are removed from the lichens, and the soluble substances, amounting to about 75 per cent., are dissolved out. It is claimed that these form a kind of jelly which can be used for bread-making, as much as three parts of jelly to two parts of flour being employed, yielding a bread which is said to be more nutritious than ordinary rye-bread. The jelly serves other useful purposes, as it can be used for preserving fruit, thus effecting a saving of more than half the amount of sugar usually required.

A few other wild plants are used for food without being made into bread. Salep, a preparation of the root of the early orchis (*Orchis mascula*), is probably

[1] Wilson, *Rural Cyclopædia*, 1847.
[2] Pratt, A., *Flowering Plants, Grasses, and Ferns of Great Britain.*
[3] Wilson, *ibid.*
[4] Hanansek, T. F., *Zeitsch. für untersuch. der Nahrungs una Genüssmittel.* 29, pp. 17–25, 1915.
[5] *Ibid.*

still part of the staple diet of some of the eastern countries, particularly Persia, Syria and Turkey.[1] The plant is cultivated for the purpose, though it is a common wild plant, being very abundant in the moist meadows of some parts of England. Salep is said to be very nutritious, and is a tough, horny substance prepared by heating the orchis root and drying it in the air. Another wild food that might be worth exploiting is the "pignut" or "earthnut," the swollen underground part of *Conopodium denudatum*, which is exceedingly abundant in many places.[2] It has long been used in the Low Countries, though less now than formerly, being treated like parsnips and carrots. Their flavour is said to resemble chestnuts, and at times they have been a regular market article in Sweden.[3]

Some wild plants are more or less palatable if used as vegetables, but they are almost entirely neglected in favour of the cultivated types. Local use is made of the young flowering stems of the spiked Star of Bethlehem (*Ornithogalum pyrenaicum*), which is plentiful in parts of the West Country, and used to be marketed under the names of French or Bath Asparagus.[4] Nettle-tops are often used by country people in the early spring, as they are credited with some medicinal value if cooked like asparagus. The roots of chicory, dandelion and silverweed (*Potentilla anserina*), and the leaves of sorrel and dandelion are the most common parts of wild plants that are used as vegetables, either cooked or uncooked. An interesting list of wild plants was recommended during the war by the Austrian authorities in Vienna for use as vegetables:

(*a*) For use as *spinach or cabbage*: nettle, plantain, giant fennel, wild garlic and plants of the primrose family. These can all be used alone or mixed with other vegetables.
(*b*) *Salad substitutes*: chicory, dandelion, sorrel and wild hops.
(*c*) *For flavouring soup*; violets, giant fennel, ground-ivy.

The universal shortage of oils and fats has stimulated the collection and use of various wild products that can be used as sources of oil, especially in Central Europe. Beech-nuts are credited with yielding an excellent food-oil, and Germany made efforts to secure the beech-nut harvests from neutral countries in order to augment her oil supplies. Hazel-nuts, walnuts and pine kernels also contain much oil. The German hazel harvest in 1916 was the largest since 1882, and high prices were paid for the nuts for the purpose of oil extraction. Pine-cones have been broken up and the kernels separated by a light form of threshing, but where facilities exist it is possible to release the kernels by drying the cones in kilns. Long before the war it was the custom for the children of the Black Forest (Germany) to collect elder-berries and take them to the little village mills for the oil to be expressed. Edible oil can also be obtained from horse-chestnuts, but, as the oil is very firmly held in the tissues of the seeds, the extraction is a matter of some difficulty, and even then only about 2–2·5 per cent. of oil is obtained. One of the best oils is obtained from the common red poppy (*Papaver rhœas*); it is particularly sweet and wholesome, and has been used as a substitute for olive-oil.[5] The cultivation of poppies is encouraged in Austria, as they yield a greater harvest than any other variety of oil-seed. Oil is expressed from various

[1] Wilson, *ibid.*
[2] Crichton Browne, J., "A Neglected Source of Food Supply," *Selborne Magazine*, vol. xxviii. No. 332, 85–7, 1917.
[3] Hogg and Johnson, *ibid.*
[4] Lindley, J., and Moore, T., *Treasury of Botany*, 1889.
[5] Hogg and Johnson, *ibid.*

other wild plants, but as this is for other purposes than food it will be dealt with elsewhere.

2. *Fodder*.—Valuable as wild plants have been in providing additional food for human beings, yet they have proved still more valuable with regard to supplementary fodder for animals, so much so that it is quite likely that some of the fodder substitutes will be retained after the food substitutes have fallen into disuse. Heather, gorse and bracken cover large areas, and provide an abundant source of supply. Quite early in the war attempts were made to feed heather, and at first it was simply ground roughly before use. In East Friesland a mixture of meal and heather was made that was stated to be 40 per cent. cheaper than any fodder previously used, and to be readily eaten by horses, cattle and pigs. Special heather fodders were prepared by the Germans for different classes of animals. The woody stalks were ground, mixed with molasses and an albuminous foodstuff, and used for fattening pigs. The green stems, on the other hand, when ground and mixed with molasses, were utilised for feeding horses and cattle. "Heather meal," containing 7 per cent. protein and 9 per cent. fat, is said to be equivalent in feeding value to medium quality meadow hay. The Austrians used heather as a substitute for hay for both military and civilian horses.

Although gorse (*Ulex europæus*) looks such an unpromising subject for conversion into fodder, it can be utilised when properly treated, and at one time it was much used as forage in this country, sometimes being cultivated for the purpose. Before use gorse needs to be thoroughly crushed to break the spines, rollers or a heavy mallet being suitable implements. Only young growth is fit for feeding, and a constant supply may be maintained by cutting areas of old established woody gorse down to the roots, and utilising the new growth after one or two years' interval. Gorse is much appreciated by stock, especially horses and cows. Old writers state that when it forms part of the diet milch cows give an abundance of milk which produces excellent butter, and cattle fatten as well as though fed on turnips.[1] More recent work indicates that the gorse should be well crushed and freshly prepared, in order to avoid fermentation setting in.[2]

Bracken is less usually employed, but it is reported that the young fronds make a good fodder for pigs if they are mixed with potatoes, steamed and pounded, and also that the rhizomes are rich in food material, especially before the end of April, and may be fed to pigs.[3]

The rhizomes of couch-grass make a valuable fodder when it is possible to overcome the mechanical difficulties of cleaning. Animals are very fond of it when fresh, and it also dries into a good hay of a nutritious quality, which if properly chopped up is said to be equal in value to good meadow-hay or even clover-hay.

One analysis of couch-grass gave[4]:

15·30 per cent. water.
7·15 „ „ albuminous compounds.
34·80 „ „ soluble feeding constituents (as mucilage, sugar, glucose).

[1] Wilson, *ibid.*

[2] Fau, E., *L'Industrie laitière*, No. 42, pp. 677–9, 1910. *Journ. Board Agric.* vol. xxv. p. 894.

[3] Weiner, *Landw. Ztg.*, April 22, 1916. Summarised in *Journ. Board Agric.* vol. xxiii. p. 499.

[4] Hughes, *Journ. Bath and West of England and Southern Counties Assoc.* vol. viii. pp. 44–52, 1897–8.

While a German analysis showed[1]:

10·37	per cent.	crude protein.
4·93	„ „	digestible protein.
1·36	„ „	fat.

The collection of couch-grass rhizomes has been organised in Austria during the war, a fair price being paid for the fodder when washed, air-dried, and made up into bundles.

Chicory is also valuable, as the dried roots contain a good deal of sugar, and have been used instead of oats.[2] As, however, the root is so valuable as a coffee substitute, its use as fodder has recently been prohibited abroad, especially in Holland. The plant has often been cultivated in Italy, France, Switzerland and elsewhere, the harvested crops usually being made into hay.

A considerable use has been made of reeds as fodder in Central Europe during the last four years. The reeds have been reaped green in the summer time and fed to oxen, calves and milch cows in order to save the ordinary rough fodder for winter time. They are supposed to be very nutritious, and in Germany have been manufactured into "reed grass meal."

Wild nuts have always been widely used for feeding purposes, especially for pigs, which can easily be driven into the woods to forage for themselves. Beech-nuts have recently been collected to some extent for oil, but it is recognised that it is more profitable to use them for feeding pigs, as less labour is required and a larger return is obtained. Reports have been made that sheep will not touch beech-nuts, and that care must be taken never to feed them to horses, as the "fagin" in the nuts is an active poison to them. Poultry and cattle, however, do not seem to be affected by the poisonous constituent.[3]

Horse-chestnuts have generally been used in their natural state or roughly crushed, but the Germans have been converting them into a concentrated food, containing albumen, a large proportion of starch, and rather more than 8 per cent. of fat. The horse-chestnuts are dried, peeled and powdered, the bitter flavour being removed during the preparation, the process yielding about 8 per cent. of saponin, which may be used as a soap substitute.[4] Horse-chestnuts are very digestible, and do not appear to be poisonous, and practically all kinds of animals can be fed on them.[5]

It has long been customary to collect acorns in England for pig feeding, and to turn out cattle and pigs to pick them up for themselves. This is less done nowadays, however, as many cattle have been lost by "acorn poisoning," which does not seem to affect pigs or sheep. The chief feeding value of acorns lies in the large amount of carbohydrates present, and the dried nuts are better than the fresh in this respect. With care and proper precautions there seems to be no reason why acorns should not be used for fattening livestock, especially pigs, provided they are only used in moderate amounts.[6]

The green parts of various herbaceous wild plants have considerable value as fodder, either in the fresh state or as hay, some of the commonest weeds being among the number. Spurrey (*Spergula arvensis*) is rarely or never utilised

[1] Strecker, 1917. Summarised in *Journ. Board Agric.* vol. xxv. pp. 330–31.
[2] See *Journ. Board Agric.* vol. xxiii. pp. 499–500.
[3] *Journ. Board Agric.* vol. xxi. pp. 527–8, 1914.
[4] See also Goris, A., *C. R. de l'Académie des Sciences*, vol. clxv. No. 10, pp. 345–8, 1917.
[5] *Journ. Board Agric.* vol. xxi. pp. 522–6, 1914.
[6] *Journ. Board Agric.* vol. xxi. 1914, pp. 511–21.

nowadays in this country, but years ago it was recognised as good for animals, and was specially cultivated abroad, largely in the Low Countries. The plant grows rapidly, and two crops a year can be obtained, which are fed either green or as hay, or are eaten off the field by the animals.[1] If the seeds are allowed to ripen it is said that they can be harvested and used separately for feeding purposes. Nettles, again, are used abroad to some extent when other forage is scarce. The animals will not touch the growing plants because of the stinging hairs, but shortly after cutting these hairs collapse from loss of water and are then innocuous. Nettles have long been cultivated in Sweden, as they can be grown on waste or poor land and they are very resistant to extreme temperatures.[2] One great advantage is their rapid growth, as with judicious cutting three crops a year can be obtained. Even where regular cutting is not practised the leaves and seeds are often collected and dried as winter food for fowls. The leaves are boiled in water to the consistency of gruel, and are then mixed with cooked potato peelings, while the seeds are added to other food and are found to stimulate egg production. Analyses made by Professor Hendrick[3] show that nettles may be very useful as feeding-stuff. Young nettles from 12–18 in. high sampled in May contain much fat and ash, and also as much albuminoid material as linseed cake. Older nettles cut in July are more fibrous and contain more soluble carbohydrates, but the percentage of nitrogen is higher than that in grasses. These figures indicate that dried nettles cut at the flowering stage are similar in analysis to hay made from leguminous crops.

Various other weeds can be used as green fodder in localities where they happen to grow abundantly. Knot-grass (*Polygonum aviculare* and *P. lapathifolium*)[4] is relished by pigs, which also eat hog-weed (*Heracleum sphondylium*) with avidity, the knot-grass has been made into hay in Alsace.[5] German experiments[6] show that many of the most common weeds are distinctly valuable, chickweed (*Stellaria media*), bindweed (*Convolvulus arvensis*), creeping thistle (*Cirsium arvense*), fathen (*Chenopodium album*), and sowthistle (*Sonchus oleraceus*) being suitable for feeding in quantity. They are all more nutritious in a dry state than when fresh, bindweed containing the largest proportion of food constituents. Bent grass (*Agrostis stolonifera*) was at one time sold in France and Italy for horses,[7] and used to be cultivated in this country for fodder, though the practice has now almost been discontinued.

The necessity created by the war has caused experiments to be made with unusual food substances; attempts have even been made to utilise "flour" made from wood. This has proved of some value for feeding cattle, but is useless for carnivorous beasts, as dogs. In Austria fallen leaves have been collected from the woods and used instead of chaff in feeding horses and cattle. Another substitute has been made in Sweden from lichens, reindeer and Iceland moss being said to form valuable fodder when they are soaked and prepared. In Germany American waterweed (*Elodea canadensis*) has been collected from lakes and water-courses and dried for forage, especially for pigs.

[1] Spurrey as a forage crop, *Journ. Board Agric.* vol. xviii. pp. 1020–24.

[2] Garcia Santos, *A Vinha Portugueza*, vol. xxxi. No. 9, pp. 276–80, 1916. See *Rome Bulletin*, vol. viii. p. 77, 1917.

[3] *National Food Journ.* Sept. 11, 1918. See also *Journ. Board Agric.* vol. xxv. p. 992, 1918.

[4] Störme und Klein, *Deutsche Landwirtsch. Presse*, 41. No. 80, p. 890, 1914. See also *Rome Bull.* p. 1626, 1914.

[5] Hogg and Johnson, *ibid.*

[6] Kling, M., *Landw. Versuch-stat.* vol. lxxxv. pp. 433–70.

[7] Wilson, *ibid.*

Weed seeds form a useful food for animals when they can be obtained in bulk, and they are used either in their natural state or as cake after the oil has been expressed from them. The seeds of poppy, charlock, and gold-of-pleasure (*Camelina sativa*) all yield valuable supplies of oil, and a cake that is of much feeding value. Black bindweed (*Polygonum convolvulus*) is closely related to buckwheat, and the composition of the fruits is very similar, so that the weed fruits have been utilised to replace buckwheat for feeding purposes.[1] The fruits of cleavers (*Galium aparine*) are exceedingly hard, but have been used as food for cattle when thoroughly ground.[2]

Mulberry-leaves are not always obtainable for rearing silkworms, but they can be replaced by comfrey (*Symphytum officinale*), dandelion and knot-grass (*Polygonum aviculare*), which are readily eaten by the caterpillars. Great claims are made for comfrey, as German growers state that if the leaves are used for food four or five crops of cocoons can be secured during one year, instead of the single crop that is obtained when mulberry-leaves are fed.

3. *Fibres*.—The utilisation of wild native plants for the provision of fibre suitable for textile manufactures has not been considered very seriously until the last few years, except in a few cases where local use has been made of occasional species. Nettles (*Urtica dioica* and *U. urens*) are the one exception to this, as they have been exploited from very ancient times, though with the great modern developments of cotton and linen manufactures they have fallen into disregard. The ancient Egyptians held the secret of preparing fine white thread from nettles, and this was largely used for weaving into textile fabrics. In some parts of Italy nettle cloth has been woven till comparatively recent times, and in Siberia the weed has provided the fibre needed for the manufacture of twine and rope, chiefly for fishing-lines and cordage.[3] Some advance had been made during the last ten years, as in 1911 an Austrian firm devised a method of removing the gum from the rough fibre, the resulting finished product being a pure bleached fibre which was glossy, supple and flexible. The war, however, has given a considerable impetus to the exploitation of this source of fibre, and so many improvements and inventions have been carried out that it seems likely that, with judicious methods of cultivation, nettles will prove a profitable crop. In 1916 the Central Powers had developed the industry so far that nettle fibres were reported to cost 60 per cent. less than imported cotton, and the War Ministries were using it for manufacturing wagon-covers, tents, sackings, clothing materials, and even cloth for military underwear. In Denmark the nettle yarn has been worked up with wool, with satisfactory results. At first the warp was of wool, the woof of yarn spun from nettle fibres, the resulting fabric being suitable for men's suits, as it was not so soft as pure woollen cloth, though it possessed considerable strength. Later on attempts were made to blend the wool and nettle fibres in the yarn, as better results were expected from such a combination.

Steps have been taken to organise the collection and cultivation of nettles, with the result that by 1918 it was estimated that 23,000 hectares (= 9,292 acres) would be under nettle cultivation in Germany, with an estimated yield of 175,000 metric cwt. of fibre. The cultivation of nettles has also been encouraged in Hungary, a special company being formed to further the work. The plants will grow well on ground that is of little agricultural value, and in some plantations

[1] Hogg and Johnson, *ibid. Landw. Versuchs-stat.* 78. See *Journ. Board Agric.* vol. xix. pp. 676–7.
[2] Bernatsky, J., 1915. See *Rome Bull.* vol. vii. pp. 245-6, 1916.
[3] Hogg and Johnson, *ibid.*

they have developed into shrubs as high as a man. In other cases plantations have been formed on moorland which is unsuitable for growing food crops, and it is claimed that by doing this the land is subjected to a process of slow, natural cultivation that in time will increase the agricultural value of the land.

Other fibre substitutes are still of comparatively little importance, though they have proved useful in emergencies. The long hairs attached to the flowering heads of cotton grass (*Eriophorum* sp.) have been used for spinning purposes, either alone or mixed with wool or cotton. When mixed with ordinary cotton in the proportion of 3:1 a fine yarn is obtained, and when combined with about equal proportions of wool it yields a coarse yarn useful for making into thick stockings, or for weaving into a serviceable cloth. Cotton grass is very abundant on the German peat moors, though the exact species used is not indicated.

The hairs from the seed of willow-herb (*Epilobium* sp.) were used at one time for stuffing pillows, bed-quilts, etc., and were also spun with cotton to give a yarn for glove-making. The cost of production is high, the yield is low, and the fibre has little strength, so the outlook for this is not promising. The hairs from the heads of reedmace (*Typha* sp.) have been used for stuffing feather beds, and it was an old Swedish fashion to mix them with cotton for the manufacture of hats.

Other wild plants yield a class of much rougher fibres. The bast fibres from willow-bark (obtained from twigs peeled for the manufacture of baskets) make a good substitute for oakum, and have been used instead of tow and jute in Germany, the carded willow-bark being very useful instead of tow for cleaning guns. Successful attempts have also been made to spin the fibres into cloth, either with or without the admixture of cotton. The willow fibres have been used as a bast for tying up planks. Air-dried willow-bark (or rind) will yield from 10–20 per cent. (or even up to 30 per cent.) of fibre, but care is needed in treatment, as, if the freshly gathered material is piled in heaps, it is apt to rot, which renders it practically worthless.

Hop tendrils have also been recommended as a good substitute for jute, especially in the production of sacks, but, as the proportion of fibre is only about 8 per cent. and the cost of production is rather high, it is more profitable to utilise the tendrils as fodder.

In Sweden cloth has been woven from peat fibres, proving cheaper than shoddy at the prices ruling in 1916. Broom fibre is being used for rope-makers' wares, and may be regarded as a prospective substitute for hemp.

In connection with the textile industry, seaweed has been turned to account in the preparation of a dressing substance which is said to be valuable in binding the other dressing stuffs and to make the colours faster and more brilliant. Various species of seaweed are used for the purpose, including *Laminaria*.

Sedge, rushes, and reeds used to be widely employed for coarse work, though they are of less importance nowadays. Rushes were woven into mats and baskets of all kinds; chair bottoms were made from them, and useful cordage for rustic purposes was prepared by twisting the rushes together.[1] Sedge has been used by the Italians for the covers of oil-flasks and for chair bottoms, and also for stuffing crevices in casks, while the Laplanders are said to make a kind of flaxy fibre from it to protect their feet and hands in winter. In Kent the larger varieties of sedge have been used as ties to fasten the hop-vines to the poles.[2]

The great shortage of paper-making materials has led to a diligent search for substitutes, and much work in this direction has been carried out by Professor

[1] Pratt, A., *ibid.*
[2] Wilson, *ibid.*

F. W. Oliver. Large quantities of raw material are essential if the schemes are to be of practical value, and amongst others attempts have been made to utilise the large areas of vegetation on the sea-coasts. Cord-grass (*Spartina* sp.) is one of the most promising plants, and with judicious cutting two tons of dry grass per acre can be obtained, which can be worked up into a satisfactory paper, care being needed to avoid disfigurement with specks of mud. Maram-grass (*Psamma arenaria*) is also being investigated for this purpose. The common reed (*Arundo phragmites*) yields a good typewriting paper, which bleaches well. Couch-grass (*Agropyrum repens*) rhizomes can also be worked up into paper, and, although the quality is not very good, it is useful for rough work. As experience is gained and improved methods of treatment are developed it is hoped that a larger measure of success will be obtained.

4. *Fuel.*—Gorse is a very old-fashioned type of country fuel, and is still used for heating the large brick bread-ovens, as it throws out a great heat. It is also customary for country people to collect the gorse from commons after it has been charred by fires which are sometimes accidental and sometimes started purposely to clear areas for regeneration. In districts where pines and firs grow freely the cones are useful for fuel, and latterly they have been used for firing on the Swedish railways, where it is found that two tons of cones give out as much heat as one ton of the German coal usually used.

Large quantities of weed-seeds are obtained when cereals are screened in bulk, and as these often contain an abundance of charlock (an oily seed) and fat-hen, it has been proposed to mix them with small coal for burning purposes.[1]

In Denmark seaweed has been used a source of gas, and as one ton of seaweed produces more than half the amount of gas obtained from one ton of coal, it is a profitable undertaking where great accumulations of seaweed occur.

Oil for heating and lighting purposes can be obtained from the seeds of gold-of-pleasure (*Camelina sativa*). This oil was famous in the days of the old herbalists, as Gerarde tells us that the rich people of his day burnt it in their lamps, while the poorer people used it for culinary purposes at festivals. The oil has hardly any smell, and is supposed to give a brighter flame and less smoke than oil of rape or mustard, but it is probably more useful to soapmakers than as fuel.[2]

5. *Manures.*—The residue, after gas has been abstracted from seaweed, contains considerable quantities of potash salts, thus making it a valuable manure. Factories have also been set up in Sweden to extract the potash from seaweed for agricultural purposes. This, of course, is only a revival and modification of the old use of kelp as manure. Where cartage is cheap it has long been the custom to manure fields near the coast of our own country with seaweed, but with increased labour costs it is now less frequently possible. Various attempts have been made, with more or less success, to set up factories to burn the kelp on the spot in order to obtain the potash and other valuable ingredients in a more portable form. Waste hedge-trimmings supply a fair amount of potash, but to obtain the best results the material should be burned as soon as possible after cutting, and the ashes either spread on the ground at once or stored in a dry place to avoid loss of soluble salts by leaching. A similar use can be made of bracken, the most profitable time to cut it for this purpose being July.[3]

[1] Dymond, J. R., Archibald, E. S., Eldorf, F. C., "Grain Screenings and Results of Feeding Experiments in Canada," *Dominion of Canada, Dept. of Agric.* p. 44, 1915.

[2] Wilson, *ibid.*

[3] *Journ. Board Agric.* vol. xxv. No. 1, pp. 1–11.

The ordinary farm weeds can be turned to account in various ways, either by using them as green manures or by burning them on the ground and scattering the ashes. On sandy land bent grass (*Agrostis* sp.) is most abundant, and it is the usual custom to scuffle it up into heaps and burn it there and then. Couch-grass (*Agropyrum repens*) is sometimes treated in the same way, when it grows in soil that is not too heavy to allow the rhizomes to be removed cleanly from the ground. When large quantities of miscellaneous weeds are collected by the harrow and it is not possible to burn them, they are often carted away and mixed with lime, which rots them down into a useful compost. The carting needs to be carefully done, in order that the seeds shall not be scattered about more than is necessary, but under no circumstances should heaps of weeds be left lying about without some attempt being made to destroy them, as it is in that way that bad attacks of weeds are encouraged.

If foul land is ploughed up and the weeds are thoroughly well buried they soon decay and return their plant constituents to the soil. The texture of light land can be considerably improved in this way, and it has been suggested that spurrey may profitably be sown on sandy soil for this purpose, provided it is turned in before the seeds are ripe.[1] Analyses have been made in Germany of some of the commonest weeds, showing that they have considerable manurial value, as chickweed (*Stellaria media*) and fathen (*Chenopodium album*) each contain about 10·9 per cent. of potash, while bindweed, sowthistle, and others are also useful.[2]

6. *Drugs.*—Comparatively few of our native plants are of much commercial value as drugs, but meadow saffron (*Colchicum autumnale*), hemlock (*Conium maculatum*), poppy (*Papaver* sp.), deadly nightshade (*Atropa belladonna*), foxglove (*Digitalis purpurea*), henbane (*Hyoscyamus niger*), and dandelion (*Taraxacum vulgare*) are widely used, and are cultivated to supplement the supplies obtained from the wild plants. The seeds of meadow saffron have been prescribed for gout and rheumatism,[3] and the sedative alkaloid conine is obtained from hemlock, the green, unripe fruits containing the largest proportion of the drug. Poppies have been valued for their narcotic properties from very early days, as they were used in medicine even in the time of Theophrastus (*circa* 322 B.C.). More recently the field poppy has been chiefly valued for a fine red substance obtained from the petals, which is used as a colouring matter in pharmacy.[4] During the war it has been difficult or impossible to obtain adequate supplies of many drugs that are usually imported, and a considerable impetus has been given both to herb-growing and to the collection of medicinal wild plants. For the latter work the school children have been organised in many parts of the country, and considerable supplies have been thus obtained.

The bark of alder buckthorn (*Rhamnus frangula*) has recently been used in Austria to provide substitutes for a whole series of drugs that were hitherto imported, and in various places iodine, bromine and other medicinal preparations have been extracted from seaweed. The root of broad dock (*Rumex obtusifolius*) is very astringent and can be used in the same way as the powder or tincture of Turkey Rhubarb,[5] while if powdered it is said to make an excellent dentifrice.[6]

[1] See *Journ. Board Agric.* vol. xviii. pp. 1020-24.
[2] Kling, M., *Landw. Versuchs.* vol. lxxxv. pp. 433-70. See *Journ. Bo Agric.* xxii. pp. 362-3.
[3] Fluckiger and Banbury, *Pharmacographia: A History of Drugs*, 1874.
[4] Fluckiger and Hanbury, *ibid.*
[5] Wilson, *ibid.* [6] Pratt, A., *ibid.*

The juice of the tall buttercup (*Ranunculus acris*) is very acid when fresh, and is occasionally applied to cause local irritation in cases of rheumatism and other ailments. If carelessly used it may cause ulcers, but the plant loses its virulence on drying. Old writers say that beggars use this juice to raise ulcers on their feet, which they then exhibit to excite compassion.[1]

Coltsfoot (*Tussilago farfara*) is an ancient remedy for chest and lung troubles, as the leaves are demulcent, and give great relief when they are smoked. British herb tobacco largely consisted of coltsfoot leaves, and was formerly much used in pulmonary complaints.[2]

7. *Dyes.*—Several wild plants contain colouring matters that have been used for dyes, but they are seldom so employed since chemical dyeing has come into its own. A blue dye, woad, is obtained from *Isatis tinctoria*, and when this is combined with the bright yellow dye from woodwax (*Genista tinctoria*) the celebrated "Kendal Green" is the result."[3] Ladies' Bedstraw (*Galium verum*) has been used by the Highlanders to dye red, the roots of the plant being boiled with the yarn, the colour being fixed by the addition of alum. In some quarters this dye is considered to be superior to madder, and at one time attempts were made to cultivate it, but the yield per acre is too low to make it a paying proposition.[4] If the tops of the plants are boiled with alum a bright yellow dye is obtained. Agrimony (*Agrimonia eupatoria*) yields a yellow dye which varies in shade according to the season at which the plant is gathered, being bright nankeen if gathered when the flowers are beginning to open, and a deeper colour if left until September.[5] Yellow colouring matters have also been extracted from the roots of nettles (*Urtica dioica*) and from dyer's rocket (*Reseda luteola*). A fine red dye has been produced by boiling the dried roots of sorrel (*Rumex acetosa*), and the outer parts of the root of corn gromwell (*Lithospermum arvense*) give a red colour to oily substances, skin, paper, and linen, a substitute for rouge being prepared from the plant in Sweden.[6]

8. *Miscellaneous Uses.*—Individual species of weeds and wild plants have many specialised uses to which they are adapted by some peculiarity of habit or property. Many of these uses are very local, but nevertheless the plants fill certain gaps in the general economy of life. Some industries take advantage of wild products and utilise waste substances to great advantage. The small round fruits of fat-hen (*Chenopodium album*) are pressed into moist hides in the preparation of Shagreen leather, giving the characteristic pitted appearance. Tannin substitutes can be derived from agrimony (*Agrimonia eupatoria*), which has been used in Germany, cinquefoil (*Potentilla reptans*), tormentil (*Potentilla tormentilla*) (sometimes used in the Western Isles of Scotland and in the Orkneys[7]), and bracken (*Pteris aquilina*). The seeds of plantains (*Plantago* sp.) are coated with a considerable quantity of mucilage which stiffens muslins well, as it gives a substance that is not easily obtained with other preparations.

Carragheen moss has recently been used in Germany for the impregnation of balloon envelopes and aeroplane wings, carragheen, salt and magnesia powder being boiled together and filtered, the filtrate being applied as the dressing.

[1] Woodville, W., *Medical Botany*, 1790–92.
[2] Woodville, W., *ibid.*
[3] Lindley, J., and Moore T., *Treasury of Botany*, 1889.
[4] Wilson, *ibid.*
[5] Hogg and Johnson, *ibid.*
[6] Wilson, *ibid.* Pratt, A., *ibid.*
[7] Hogg and Johnson, *ibid.*

The various species of horsetail (*Equisetum* sp.) abound in small particles of mineral matter which make the plants useful for scouring purposes, especially for cleaning tinware and for polishing hardwood, ivory and brass.[1] A large species (*E. hyemale*) has been imported from Holland under the name of *Dutch rushes*, and used in Northumberland for cleaning milk-pails. Goose-grass (*Galium aparine*) is also appreciated in some dairies, as it makes an efficient substitute for a sieve for straining milk. This is a very ancient use of the plant that was known to the Greeks, and has lived on in various parts of Europe, to the present day.[2] Milk is curdled in Picardy by the addition of fumitory (*Fumaria officinalis*), and nettles and goosegrass (*Galium aparine*) can be used in a similar way.[3] Nettles can be made into a rennet by adding a quart of salt to three pints of a strong concoction of nettles, and a tablespoonful of this rapidly coagulates a large bowl of milk, making a pleasant beverage free from any flavour of nettles.[4]

Other domestic uses are occasionally served by wild plants. Heather is commonly used for beds by cottagers in remote places, and mattresses are stuffed with it. Cushions and pillows are sometimes filled with the cottony down from the leaves of coltsfoot (*Tussilago farfara*), and in the days when flint and steel were used instead of matches this same down made an excellent tinder when it was wrapped in rag, dipped in a solution of saltpetre and dried in the sun.[5]

During the war cork substitutes have been prepared from elder pith, but better results were obtained by drying and pressing certain fungi that are plentiful in the forests of Hungary, the collecting and drying presenting little difficulty. In Norway boots and shoes have been soled with birch bark, several layers of smooth bark being glued together with a special preparation. The soles are warm, watertight and durable, and are fairly flexible. Seaweed is being exploited as a source of glue and similar products, especially in Norway, where great accumulations occur in places along the coast.

From the above account some idea may be gained of the part played by wild plants in common every-day life, and especially of the revival in their use brought about by the Great War. Doubtless many of the uses, both old and new, will again fall into neglect, but it is possible that in some instances sufficient advance has been made to assure that certain weeds will continue to be exploited for the benefit of the community. The field of possibility is large, and it would be a great pity if the knowledge gained during the time of stress were lost for lack of enterprise in following the matter up and extending the bounds of usefulness in cases justified by experience.

WAR—A PLEA FOR SCIENTIFIC RESEARCH (Major-General Charles Ross, C.B., D.S.O. (ret.))

PART II

How control the Spirit of Illegitimate Ambition?

It is illegitimate ambition which, in ordinary social intercourse and business competition, gives rise not only to robbery by violence, but to cheating and chicanery. The two last, indeed, are infinitely more prevalent in civilised communities than the first, for they can often be practised with impunity in spite,

[1] Mac-Millan, C., *Minnesota Plant Life*, 1899.
[2] Pratt, A., *ibid.*
[3] Hogg and Johnson, *ibid.*
[4] Wilson, *ibid.*
[5] Hogg and Johnson, *ibid.*

or even under the *ægis*, of the law and the police force. Yet they are as apt as violent robbery to give rise to the fighting spirit. It is possible to devise social laws under which robbery and open cheating can be punished and more or less controlled; but no law can be devised which leaves no loopholes for chicanery. International chicanery is certainly calculated to rouse passion and the fighting spirit in a community, as witness in the Japanese when the Russians obtained possession of Port Arthur by trickery.

Society attempts to control chicanery to some slight extent by means of exposure in the Press. Such a method might operate with success in international relations; though a nation which would be guilty of chicanery would hardly hesitate to open a press campaign in support of its pretensions.

It is, evidently, only along educational lines that any real lasting success can be gained. In a word, the whole of humanity must be taught to be gentlemen. The German leaders, by means of their educational system, successfully taught the whole of their people to be cads; so that the thing might be possible provided the necessary educational machinery could be set up. One need only glance at the present condition of Europe to grasp what a superhuman task such an attempt will be.

In this connection it is as well to bear in mind that youth loves competition, conflict, and pursuits which involve hardship and danger. In our unregenerate condition youth particularly loves to be taught how to fight. To eradicate or control such a sentiment will not be easy, and will require not only a most perfect system of education, but guarantees that the instruction is everywhere carried out on the required lines. It is only under the strictest discipline that such guarantees can be established. Certainly, the educational system and curriculum must not be left to the "self-determination" of the various nationalities; and it is, consequently, essential that some controlling power be established with right and power to lay down and enforce both system and curriculum. It is probable that the best, if not the only practicable, system would be that which existed before the war and was known as "militarism," with, however, military training eliminated and replaced by physical or hygienic training and education—in a word, vast disciplined national schools instead of armies. Such national schools might, however, at short notice, be transformed into armies, and would, therefore, in themselves be a source of danger to the peace of the world. Neither would "Labour" listen to the suggestion: for discipline is apparently opposed to their principles, inasmuch as it interferes with "liberty." It is an unfortunate fact that any serious attempt to suppress war must inevitably result in the suppression of liberty, so far, at least, as education and freedom of thought are concerned. And if such a system could be established, should we not come back to the days of Papal supremacy, finally of the Inquisition?

There can, however, be no objection to an international university which, could it but draw to itself all the best thought of the world, might, in the end, exercise an important influence on education throughout the world. Such a university has already been suggested for medical science; and there seems no reason why it should not embrace all arts and sciences, not even excluding this vast science called war.

Other Considerations which cannot be Disregarded

We see war all around us throughout the animal and vegetable worlds. The pine tree allows but few forms of life, either animal or vegetable, to flourish beneath its branches. It overtops and destroys flowers and shrubs, depriving

them of sunlight and air, choking them with its poisonous needles. But few trees permit vegetation to thrive; and all, if at all crowded, seek to over-top or thrust their neighbours aside in the struggle to reach sunlight and air.

A similar struggle exists in the animal world. Most types of animals prey on others. The exceptions, those which are in no way carnivorous, are themselves invariably preyed upon in the wild state by the better armed and more ferocious. It is noticeable that all animals are armed in one fashion or another for defence, and those armed for offence are the most savage.

Of all animals man is the most ferocious. With the exception of the leopard, the weasel, and one or two other types, he is the only one that kills for sport and for the very love of it. He also conducts a ruthless war of extermination against all animals or vegetables which so much as incommode him. He destroys even their very breeding-grounds.

It is to be noted that carnivorous beasts of the most savage type seldom attack or prey upon each other except when driven to extremes by starvation. This absention appears, however, to be due to fear; for a wounded or sick wolf, for instance, is generally killed and devoured by his comrades. Some domesticated animals, on the other hand, have been cured of their propensity for the destruction of their own kind by man, by the expedients of punishment and the supply of ample food. Man has constituted himself the master, and exercises absolute control over the actions of the animals he has domesticated. He permits only such liberty as suits him or as is essential to enable them to execute the tasks he sets them. They are his slaves. If, then, it is desired to cure man, himself, of his propensity for the destruction of his own kind, it is, presumably, necessary to domesticate him and to establish a ruling authority, if none such already exists, which shall provide him with a sufficiency of food, with such comforts and luxuries as he desires, with a fair portion of liberty, and certain and severe punishment if he attacks his neighbours.

Is there such a Ruling Authority?

Any attempt at a scientific study of war must inevitably bring us to this question, and the solution of the problem is quite impossible unless it be honestly tackled.

There is no evidence, worthy of the name, that the writer has ever been able to discover in his studies of history to show that the Almighty concerns Himself in the slightest degree with the struggles of men. This, in spite of the fact that combatant armies and nations are accustomed to offer up prayers for victory, asseverating loudly that they have justice on their side, and that the opponent is unworthy of consideration. How can we account for the fact that the Almighty, being both omniscient and omnipotent, pays no regard to the piteous supplications of humanity, and permits the horrible turmoil to continue as if we were but communities of ants?

Neither does He apparently exercise, in a direct manner at least, penal authority, the powers of a judge, over mankind in ordinary social intercourse; for if He did there would be no necessity for our own judges, juries, law courts, or police force. It has been said that "the mills of God grind slowly, but they grind exceeding small." That may be so; but punishment inflicted long after the offence has been forgotten is no deterrent to crime. Hence it is that society is obliged to adopt measures to safeguard itself.

It is clear then that similar measures, which have been proved by long

experience to be efficacious, must be adopted by man if international or civil wars are to be terminated. That is, a ruling authority with power to make laws and inflict punishment must be established, with its whole paraphernalia of law, judges to interpret it, and a police force of overwhelming power to maintain it. And, as we have already seen, this ruling authority must also possess the power to lay down the system and curriculum of education and, if necessary, enforce it.

It is evident that if a termination is to be put to war nations must abdicate their sovereignty and rest content to be governed by an international parliament.

It is a noteworthy and undoubted fact that, with the introduction of party government to South Africa, the idea of revenge against Great Britain quickly died out. The party system is certainly calculated to withdraw public attention from national problems and concentrate it on purely social problems.

Is an internatioual parliament, conducted on the party system, a feasible proposition?

War, Civilisation, and Progress

It has been commonly asserted that another such war as the last will "destroy civilisation."

Certainly, it might seriously damage the Christian ideal, the moral aspect of civilisation, in that it may prove might to be right and unscrupulous methods to be the surest road to success. It should be noted, however, that this last war has considerably strengthened the Christian ideal; and, for the first time in history, all the great nations have set themselves seriously to consider how bickering and treachery are to be suspended and war averted.

On the other hand—and this self-evident fact can no longer be disregarded—war gives a tremendous impulse to material progress, not only in invention and scientific research, but also in the arts. It awakens the best in humanity as well as the worst, and it certainly destroys lethargy. It is probably the case that a greater stride forward is taken by civilisation in four years of war than in fifty years of peace. And such progress by no means terminates with the conclusion of the war—as is proved throughout history. The "Golden Age" of a nation usually follows victory.

If, therefore, the great object of a Higher Power is progress, can a better means be found than a great war? Is war a drastic pruning at the hands of the Great Gardener?

The type of war which is calculated to destroy civilisation is the violent upheaval of the "masses," of the uneducated labour classes, or the irruption of hordes of barbarians over the whole civilised world.

Thoroughly disciplined and organised military forces are the surest safeguards against such forms of war. It is to be noted, for instance, that Bolshevism—which is certainly calculated to destroy civilisation—finds better soil on which to feed in the unorganised British nation than in the thoroughly organised French, Italian, and German nations—and that in spite of the fact that the Germans have been defeated, while the British have been victorious. Yet, throughout history, revolution has usually followed on defeat and seldom, if ever, on victory.

There is another catch-phrase to the effect that armaments are "unproductive services." No system which lends itself to education can by any possibility be termed unproductive. As well might one term universities and schools unproductive services and suppress them because the curriculum of education is faulty. The value of armaments as educational systems has long since been proved by the Germans and the French, and is now being proved by the British.

The Widening of the Basis of War

In the early Middle Ages, each feudal baron was at war with his neighbour. The power of the barons was ultimately suppressed by the king. We next find war engaged between small principalities, then kingdoms; and it was exterior menace which induced these to combine in self-defence. Great Britain and France have been hereditary enemies for centuries. Their enmity slowly died out as the German menace gradually took shape. Finally, these hereditary enemies combine in self-defence.

Thus the basis of war is slowly but surely widening; and, in the last great war, instead of one petty baron or kingdom against another, we see two leagues of nations engaged in a life-and-death struggle. Will the next great conflict occur between hemispheres, or between white and black, or white and brown? Or will Bolshevism develop into a universal civil war?

If all mankind could but find some common object of hostility! Will international party politics serve the purpose of distracting men's minds from war?

Overcrowding of the Earth

Medical science, day by day, gains triumph over disease. If war also disappears, overcrowding must result. In that case famine or new diseases will, presumably, appear. The only suggestion so far made to cope with this probability is the compulsory limitation of the birth-rate. Here is, however, one would think, a certain cause for civil war.

War one of the Forces of Nature

Can there be any doubt of it? Can it be suppressed? As well might one try to suppress the sun or the sea. The utmost we can attempt is to control, harness and utilise it in some slight degree as other forces of nature have been utilised. And the first step is certainly to institute a scientific and exhaustive study of the whole subject and of those others which are so closely allied to it.

There are many great thinkers, each engaged in exploration of his own little river or stream of science, mere tributaries of the vast river of war. These explorers in a dark continent are slowly progressing towards the unknown, where the true causes of war and perhaps the remedy for it lie awaiting discovery. Has not the time arrived when an attempt, at least, should be made to combine and organise these various activities?

A PLEA FOR THE TEACHING OF GENERAL HISTORY (Ernest Short)

THERE is a striking passage in *Amiel's Journal* to this effect: "Under the pretence that we want to study it more in detail, we pulverise the statue." How true this is of the teaching of history in many schools! In the effort to impress facts upon the memory of the student for examination purposes, we lose sight of the all-important duty of making the boy or girl feel the real thrill of history. So the student fails to realise the essential harmony of all human happenings, which is the source of the real joy and satisfaction of history.

And how, after all, can it be otherwise while history is regarded as meaning

English history? while geography, science, literature, and art are treated as separate subjects? Surely "pulverising the statue" is not an unfair description of such a method?

The purpose of these pages is to suggest a method of teaching history which will not only afford a background for our own island story, but will utilise the hours devoted to geography, literature and Bible teaching. Above all, the aid of art is evoked—art which, in later years, is often the outstanding reason for the study of the past.

A school year is generally made up of three terms of twelve weeks each, thirty-six weeks in all. I suggest that these thirty-six weeks should be divided into eighteen fortnights, and each devoted to a period of general history. Thus all history would be covered in the school year. In the following year the process would recommence. Here is a provisional list of the suggested periods:

1. The Creation and Prehistoric Man.
2. Egypt, Babylonia and the Far East.
3. The Greek City States.
4. The Art and Thought of Greece.
5. Roman Republic to Julius Cæsar.
6. Rise of Christianity.
7. The Roman Empire.
8. The Middle Ages.
9. Italian City States.
10. Reformation and Counter Reformation.
11. Spanish and Dutch Empires.
12. Supremacy of France.
13. England in the Age of Shakespeare.
14. Rise of British Imperialism.
15. The Napoleonic Age.
16. The Reaction and the Revolutions of '48.
17. The Coal and Factory Age.
18. The World War.

There is probably no better way of introducing each epoch than a general lecture, given by the head master or a visiting lecturer, to the whole school. In an hour it would be possible to outline the main facts and principles underlying any one of these periods. The facts of the nineteenth century can be grouped around such ideas as the coal age and the invention of the railway, the steamship and industrial machinery. These brought about the growth of large towns and the increase of national wealth. Finally, with the increase of knowledge came the demand of the mass of the people for political power. Ideas which can be summarised in a sentence can be vitalised in an hour.

Let me deal in rather more detail with another typical fortnight, that covering the Story of Creation and Prehistoric Man. The first of all historical facts is the vastness of geological time and space, compared with the physical littleness of man. A description of the creation of the sun, the planets and the earth from the chaos of undifferentiated matter will bring home to a child's mind the immensity of space. As for time, the throwing off of the moon from the glowing earth may have been a hundred million, or even a thousand million, years ago. The physicists and geologists differ. The minimum suggested is 57 million years ago. Yet the whole recorded history of man is about 8,000 years. It is, perhaps, 26 million years since life became possible on our planet through "the gathering together of the waters" when the once-glowing globe had cooled to boiling point,

Perhaps 20 million years ago the first living thing appeared. Later came the deposition of the chalk cliffs, the uplifting of the mountain chains, and the coming of the man-like apes, say a million years ago. The first lecture would end with a simple sketch of palæolithic and neolithic man, showing the place "tools" had in their development, aud the growth of pastoral and agricultural life on the steppes of Central Asia, Africa and Europe.

If this lecture were all, the effect upon the child mind would, doubtless, be fleeting. But the first impressions can be deepened by a visit to the Stone Age Gallery at the British Museum, and, above all, by the other English lessons dealing with cognate subjects. Thus the Bible lessons in the first fortnight might be the first chapter of Genesis and the thirty-eighth chapter of Job. The literature lesson might be Raphael's "Story of Creation" in the seventh book of *Paradise Lost*, or such poems as Kipling's *The River's Tale*, and *The Story of Ung*. The English history lesson would deal with such subjects as "London Before the Houses," or England in the Stone Age." Instead of a map selected at random from the atlas, why should not each student construct a chart of the solar system and a chart of the geological epochs? Finally, the weekly essays would also be suggested by the theme for the fortnight's study—let us say "The Poetry of Creation," and "England in the Stone Age."

Every essay should be carefully filed and preserved. It should also be illustrated by pictures. Thus "The Poetry of Creation" might include the Sistine Ceiling designs of Michelangelo, the "Creation Series" of Burne-Jones, while "England in the Stone Age" could be illustrated by postcards of museum objects which can readily be secured at the British Museum. If the essays are preserved for a year, each child will possess a series recalling the main course of universal history, and illustrated by some hundreds of pictures and thirty-six maps. He will also be familiar with a number of illustrative poems and passages from general literature, which will surely not have lost in value by association with a fitting historical setting.

Let us test the method by applying it to another epoch—say "The Golden Age of Greece." The previous fortnight closed with the story of Marathon, which placed Athens at the headship of the Greek city-states, and saved Europe from the domination of the Eastern conquerors. Surely a child can be taught to visualise the building of the Parthenon, and see the difference between a Greek temple, the shrine of a tribal god or goddess, and a great Gothic cathedral where a Christian community meets for prayer and praise. And, from the greatness of the Greek sculptors, it is an easy transition to the greatness of the Athenian dramatists, philosophers and historians. Declaim a passage from Pericles' address, as set out by Thucydides, and then pass on to the tragedy of Melos. Recall that the very qualities which made for the wonderful art and literature of Greece did not make for political wisdom and sobriety. After a short century the passion for liberty was lost. At last, in spite of the call of a Demosthenes, Athens fell before Alexander of Macedon.

Here, again, the first sketch of Athenian history may seem so general as to be almost useless. But remember the effect of the collection and possession of such photographs as the "Delphi Charioteer," Myron's "Discobolus," the "Theseus" and the "Three Fates," from the Parthenon; the "Demeter," the Praxiteles' "Hermes," the Trentham "Mourner," and the "Venus" of Melos. Moreover, the Bible lesson would deal with St. Paul's visit to Athens, or even consist of a reading of the death of Socrates as told in the *Phædo*. The maps of the fortnight would be the "Siege of Syracuse" and Alexander's conquests,

while suitable essays would be "Describe a Greek Theatre," or "Alexander's Boyhood."

Each fortnight's work should aim at providing a framework into which future knowledge may be fitted, rather than a final structure. Intellectual curiosity should be encouraged rather than the mere accumulation of facts. When once the spirit of enquiry is aroused in a boy or girl, the facts will take care of themselves. The last thing to be desired is that the first glimmering of knowledge should be fixed or final. But the boy or girl cannot learn too early the necessity for relating the facts of history to life, literature, art and science.

The carrying out of such a scheme would occupy six or seven hours of a school week, including an ordinary English history lesson. It is also presupposes two "home lessons." I do not know what the opinion of schoolmasters and mistresses will be about such a demand; but I cannot think that parents or guardians will grudge the time taken from Latin grammar and translation, even supposing it is necessary to take any time. If one or two hours are taken it may be pointed out that classical history has a considerable part in the suggested scheme of teaching, and this, in a form by which many a boy and girl will profit who are never likely to read Greek or Latin with fluency.

It is impossible to over-emphasise the importance of preserving the essays, crude and unhistorical as they may seem in the junior forms. Efforts of this kind gain greatly in significance when twenty or thirty are bound together. This will be seen when the scheme of general history is treated a second and a third time in following years, and the student's task is to rewrite the essay or pen another supplementing the effort of an earlier year. We do not sufficiently foster "the sweet pride of authorship" latent in most children.

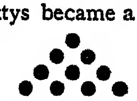
Lastly, the illustration of the essays is no less important. The provision of suitable pictures will be troublesome to many teachers at first, but a very little experience will prove the value of the method. Visualisation is, perhaps, the first requirement in the study of history. A name should always bring to mind a lively impression of a person or place. An historical happening must never be regarded as an abstraction. The study of history is a spiritual adventure. Scholar and teacher alike must embark upon it in the mood of the sea-dogs, launching out into unknown seas, but confident in their power to reach the golden harbour at the last.

ESSAY-REVIEWS

THE NATURE OF NUMBER, by JOSHUA C. GREGORY, B.Sc., F.I.C.: on an **Introduction to Mathematical Philosophy**, by BERTRAND RUSSELL. [Pp. viii + 208.] (London: George Allen & Unwin; New York: The Macmillan Co., 1919. Price 10*s.* 6*d.* net.)

THOMAS HOBBES reminded us that, in the opinion of Pythagoras, men are distinguished from other animals by their ability to number and use numbers. Since a writer can select from many distinctions between humans and animals, from wearing collars to puzzling over metaphysics, he intimates his own interests by the distinction he chooses. Pythagoras was desperately interested in numbers, and he made many other philosophers desperately interested in them also. The Pythagorean enthusiasm for numbers must have been very great and very catching for Xenocrates to define the soul as a "number which moves itself.' Xenocrates did not mean that the soul is X-fold, or divisible by other numbers, or in any way specially numerical: he called the soul a number as a lover calls his beloved a rose. The beloved is as fragrant and as beautiful as a rose; for Xenocrates numbers were so mysterious and wonderful that the word "number" meant for him "the most exalted objects of knowledge," and so appropriately denoted the soul. Aristotle perceived in this apparently absurd definition a legacy to Xenocrates from the ecstasy that led Mr. Bertrand Russell to remark, in his *Introduction to Mathematical Philosophy*, that Pythagoras "believed that not only mathematics, but everything else could be deduced from numbers."

This mysteriousness and significance of numbers, amounting to a sense of the ineffable in many mystics, has always haunted the human mind. The Pythagoreans developed the mystic qualities attached to numbers by primitive men, and systematised them into a philosophy and a religion. They discovered also a new source of astonishment and further mystic qualities in the more purely mathematical qualities of numbers. They observed, for example, that 10 is the sum of the first four integers, $1 + 2 + 3 + 4 = 10$, and the tetraktys became a celebrated figure because it represented a pyramid with four balls or dots as its base, three balls or dots as its second tier, two as its third tier, and one at its apex. This arrangement of dots long remained famous as a "figurate number," representing a pattern after which many natural objects were made. The Pythagorean discovery that the note of a plucked wire alters with its length, so that different notes correspond to distances expressible by numbers, associated numbers with the magic of music and confirmed their status as the most abstract and most exalted objects of knowledge. Number became a name to denote the inmost essences of things, and we can understand, staggering though the idea may be to the modern mind, why a fundamental Pythagorean doctrine declared that all things were numbers.



No renascence is only a renascence, just as believers in reincarnation regard the soul as in some sense remade at each birth: the early Greek preoccupation with numbers was no mere repetition of primitive thought. Primitives, says

Lévy-Bruhl, do not distinguish clearly between the number and the numbered object. Numbers excited the Pythagorean world partly because they were distinguished as independent objects of thought and observed to enter into striking mathematical relations. But the old feeling that numbers are remarkable because they belong to, or are confounded with, remarkable things continued to pervade the Greek mind. Almost every small number has been regarded as mystical and peculiarly sacred by some people. Different numbers, remarks Lévy-Bruhl, have supreme mystical quality among different peoples. The number seven was charged with significance for the Babylonian and the Hebrew, probably becauses the phases of the moon divide time into periods of seven days. Seven has not yet lost its mystical import, for modern Theosophy divided the universe into seven planes and each plane into seven grades.

Numbers probably, in part at any rate, first excited the human mind because they were one of its mental conquests. Man was not born with a sense of number. The Pythagorean definition of man as the numbering animal would probably exclude from the class of men all those "missing links" that modern anthropology has discovered. The ability to count is exceedingly defective among most primitive peoples: the savages who can count at all are often unable to proceed beyond five, and frequently stop at two or three. Very often they reckon by comparing objects with their fingers, and this method may be devoid of any real ability to number in the real sense of the word. Primitive feebleness in reckoning by numbers was known long before the modern science of anthropology. "The Tououpinambos," writes Locke, "had no names for numbers beyond five; any number beyond that they made out by showing their fingers, and the fingers of those who were present." The art of numbering, when it was attained, probably brought a sense of mastery that was one of the first sources of the excitement produced by numbers. Numbering or counting is one mode of mental mastery. The most primitive men probably knew whether all the members of their group were present by realising whether particular individuals were missing or not, as a dog probably knows that the tale of sheep is complete. When men learned to count their fish or their head of game or their cattle or the members of their tribe they simplified enormously their control over their collections. This sense of control is perhaps reflected in the primitive fear of being counted. The Bakonga believe, according to Weeks, that evil spirits will snatch away a woman's children if she deliberately counts them; the people dislike being counted, and fear a census. Census superstitions are constantly encountered in the life and records of peoples. The familiar story of the pestilence sent by Jehovah upon the Hebrews when they were numbered by King David doubtless originated in the belief that it was dangerous to be counted. Numbering was a source of power for man over his collections, enabling him to check their items without the labour of identifying each separately; demons obtained similar powers if numbers became known to them. There was probably a sense of identity between a number and the individuals or objects it denoted—a notion that because a number denoted a certain group of individuals it gave a grip over them. The number does give a mental grip which seems to have been confounded, in true primitive fashion, with power of a more concrete kind. Through this sense of identity numbers participated in the qualities of the objects they denoted. The four cardinal points made the number four significant. A mystic quality attached to the number six when the zenith and the nadir were collected into a group with the four primary points of the compass. The seven days of the week made seven a magic number. Thus the way was prepared for

Pythagoreanism to combine the more purely mathematical properties of numbers with their mystical inheritance from the past and declare that all things are numbers.

Locke, in his chapter on number, drops a hint at another reason why number was so prominent in early speculation. "Amongst all the ideas we have, as there is none suggested to the mind by more ways, so there is none more simple than that of unity, or one. It has no shadow of variety of composition in it; every object our senses are employed about, every idea in our understandings, every thought of our minds, brings this idea along with it: and therefore it is the most intimate to our thoughts, as well as it is, in its agreement to all other things, the most universal idea we have. For number applies itself to men, angels, actions, thoughts—everything that either doth exist or can be imagined."

The subsidence of the first mental excitement roused by the discovery of numbers and their properties has left a problem for solution. The most familiar and universal concepts are the most mysterious. What *is* time or space or causation? St. Augustine said that he knew what time was so long as he was not asked. We know what number is so long as no one asks us or we do not ask ourselves. Is number a property of the objects to which it is attached? Locke apparently thought that it is, for he includes it among the "original or primary qualities . . . such as are utterly inseparable from the body, in what state soever it be. . . ." The particular number "of the parts of fire or snow are in them," he adds, "whether any one's senses perceive them or no." If number is really a property of objects it seems to be a peculiarly sliding property. A group of six stones may be thought of as six individuals or as one heap. Number is a collective property if it be one at all. Each member of the class of red objects is red, but each member of a group of six is not six. A body may be in the classes of round objects, of wooden objects, of blue objects, and of manufactured objects, because it has the characteristic property of each class. Any object can be included in any collection denoted by any number, and apparently it can be so included because it has none of these numbers itself. Since any collection can be regarded as one or many, it is tempting to suppose that number is a purely mental thing. Some deference certainly seems due to the objects, for a group of five stones must be regarded as either one or five, but number appears to be something to which the objects dispose the mind rather than a quality possessed by them. Number has been described as strokes of attention—a definition that transfers it from external to mental existence. Is number, then, a mental entity?

"The question, 'What is number?' is one which has often been asked," writes Bertrand Russell in his recent *Introduction to Mathematical Philosophy*, "but has only been correctly answered in our own time. The answer was given by Frege in 1884, in his *Grundlagen der Arithmetik*. Although this book is quite short, not difficult, and of the very highest importance, it attracted almost no attention, and the definition of number which it contains remained practically unknown until it was rediscovered by the present author in 1901." Towards the end of the chapter he condenses this discovery into definitions running as follows: "The number of a class is the class of all those classes that are similar to it," and "A number is anything which is the number of some class." Many may feel inclined to leave Mr. Russell in peace with his definitions, to continue to count their money without peering into the inward mystery of numbers, and to wait for something simpler to remove their metaphysical itch. A little patience will, however, dispel the apparent portentousness of this revelation of the nature of number, even if it be decided that the whole mystery has not been disclosed in it,

There is also an interest in combining with the discussion of these definitions an attempt to understand how the notion of number arose. Mr. Russell does not deal directly with the origin of numbers; but, though his immediate concern is with logical definition, his discussion has obvious points of contact with the mental route that led men to conceive and employ numbers. It will, therefore, be convenient and profitable to begin with Francis Galton's description of the Damara who was bartering sticks of tobacco for sheep.

The rate of exchange was two sticks of tobacco for each sheep. A sheep was driven out and two sticks of tobacco laid down; then a second sheep was sent to join the first, a third to join them, and so on. Two sticks were tendered in payment as each sheep joined the group. In the phraseology of Bertrand Russell and his confrères, the Damara established a one-one relation between a collection of sheep and a collection of sticks of tobacco in which two sticks composed one item: to each sheep two sticks corresponded. Counting would have been quicker, but would have arrived at the same result that the bargain had been rightly concluded. "In actual fact," writes Mr. Russell, "it is simpler logically to find out whether two collections have the same number of terms than it is to define what that number is. An illustration will make this clear. If there were no polygamy or polyandry in the world, it is clear that the number of husbands living at any moment would be exactly the same as the number of wives. We do not need a census to assure us of this. . . . The relation of husband to wife is what is called one-one." The Damara's method of bargaining, with its plentiful parallels in primitive trading, shows that it is not only "simpler logically," but more obvious in practice to realise that two collections have the same number of terms than to assign a number to the collection, or realise that there are such things as numbers. The Damara takes us one step into the tail of Mr. Russell's first definition by introducing to us the conception of similar classes. There was a one-one relation between the sheep and the tobacco-stick couples: every sheep was correlated with one pair and every pair with one sheep. "Two classes are said to be 'similar' when there is a one-one relation which correlates the terms of one class each with one term of the other class." Under perfect monogamy husbands and wives are similar classes, and so were the sheep bought by the Damara and the pairs of sticks of tobacco he paid for them. This similarity of classes is a wider conception than number, for it can be used without numbering or without the ability to number. It is also more fundamental than number, for it is easily perceived that, in counting a collection, a one-one relation is established between the items and the numbers 1, 2, 3, 4 . . . . . n. If the Damara had counted his sheep and his sticks of tobacco he would still have been busy with one-one relations and similar classes. For simplification of study, suppose him to halve his collection of tobacco sticks by making each pair into one: one stick is now the price of one sheep. By counting ten sheep he establishes similarity between the collection and the numbers 1 to 10; by counting ten sticks of tobacco he establishes similarity between the same set of numbers and the tobacco collection. Since the sheep collection and the tobacco collection are similar to the same collection of numbers they are similar to one another, and the Damara knows, or would know if he could count, that his bargain has been rightly concluded. Thus, numbering appears to be essentially an extension of the more fundamental similarity of classes, and numbers to be a vast collection from which standard classes can be selected to place in relations of similarity with other classes.

There was, it is reasonable to suppose, a process of transition between the method of the Damara and the use of numbers proper. Digital enumeration, if

the phrase be allowed, probably contained the real germ of number. The notion and use of numbers so short-circuits mental method that their actual function and nature are obscured. This concealment of the nature and function of numbers beneath their effectiveness is one reason why Bertrand Russell's definition seems, at first sight, so cumbersome and so elaborate. An attempt to follow the genesis of the notion and use of numbers enables us to appreciate the definition by a better understanding of what number really is. Digital enumeration is not necessarily counting, and was not counting, in our sense, when it was first used. A class or collection of fingers was observed to be similar to two other classes which were then known to be similar to one another. If three fingers corresponded, one by one, to three turnips, and also to three apples, there were as many apples as turnips. We incline to consider this process to be one of numbering because our understanding of the nature of number is confused by our tendency, natural but erroneous, to think of numbers as properties of the collections or classes they denote: Locke fell headlong into this error when he described numbers as qualities inseparable from bodies. The natural numbers 1, 2, 3, ... , compose a vast collection from which classes can be selected related by similarity to classes that are numbered; a collection of five apples is similar to the collection 1, 2, 3, 4, 5, since there is a one-one relation between the members of the two classes. The number five, used to designate the group of apples, expresses this relation. All classes of five are denoted by this number, be they pennies or cauliflowers or miscellaneous objects, because their items stand in a one-one relation to the numbers from one to five. Five is not the class of turnips, or of whatever items it contains, nor is it a property of that class; it is a class, the class containing the numbers 1 to 5, to which the class of turnips, or whatever items it contains, is similar, or to which it stands in a one-one relation. All collections of five objects are in a common class because they are all similar to the class 1, 2, 3, 4, 5, a common class that may be defined by the common property of similarity to the first five natural numbers. Similarly, all classes of N objects form a common class because they are similar to the class 1, 2, 3, ... N. A number, then, is a standard class, whatever its inmost metaphysical nature may be, and it is used to denote classes that are similar to it. Also numbers are permanent classes, permanent standards: this is one reason why the three fingers used to compare the three apples with the three turnips do not, in the first instance, constitute a number.

Digital groupings—three fingers, or six fingers, and so on—were at first temporary numbers, so to speak; they gave rise to real or permanent numbers when they were themselves recognised as examples of those puzzling entities. Familiarity gradually detached particular groupings of fingers, those of three and four for example, from the classes of threes and fours, as standards of reference. The ideas of three and four proceeded from the continuation of this process as the finger groupings became signs of what we call numbers—collections to which the finger groupings were themselves similar. Stones, sticks, etc., might be used as supplemental fingers, or instead of them. By the use of written signs and by the use of names, numbers were firmly established as concepts. One by one the lower numbers settled into the human conceptual system and provided the basis for the formation of the arithmetical series that contains an unlimited supply of numbers.

The vertical strokes still retained in the Roman Numerals are a remnant of the earliest form of numeration—one stroke for one, two strokes for two, etc. The stroke notation may represent the primary digital enumeration—strokes repre-

senting fingers. These original signs display the relation of similarity, the one-one correspondence between members of groups, that underlies all numbers and numbering. The appearance of signs, including written symbols and words, that contained no explicit reference to one-one relations, marked the establishment of numbers as mental concepts when, as an abstraction, five, six, etc., could be thought of as numbers only, without reference to sheep or other objects. All particular numbers were finally conceived under the general concept of number. This rough outline will be recognised as a logicised version of the development of the notion of number; but, though schematic, it may possibly be accepted as a reasonable account.

Number still remains a mystery, as, in the last resort, all things remain mysterious. Mr. Russell speaks of any number, such as the number 2, as "a metaphysical entity about which we can never feel sure that we have tracked it down." The preceding enquiry seems to lead to a definition of the number of a class as a standard class to which the numbered class is similar. A cluster of cherries is numbered six if the number six is the standard class to which the cluster is similar. Peano's definition of number as the common property of similar classes seems to adopt a cognate point of view. The common property of collections of six cherries, six apples, etc., is their similarity, their one-one relation to the group of the first six natural numbers. It seems to be in conformity with our mental habit, and with the order of development of this habit, to regard six as a single entity to which all classes numbered six are similar, seven as an entity to which all classes of seven are similar, and so on. Mr. Russell's definition of the number of a class as "the class of all those classes that are similar to it," appears to obscure the standard of reference contained in number. It seems to reduce any number to the simple status of one of the collections similar to the numbered collection. Thus, six cherries, six elephants, and the number six would be co-equal members of the class of sixes. If this be so it is difficult to understand why we can speak of six elephants and not of elephant sixes or of cherry elephants. The adjectival function of the number six that permits its predication of the cherries, of the elephants, and of all other collections of six seems to entitle it to the more prominent position accorded to it when it is defined as a standard class.

The general definition of number as "anything which is the number of some class" is independent of the definition preferred for particular numbers.

AGRICULTURAL ECONOMICS IN ENGLAND AND DENMARK, by WALTER STILES, M.A.: on

(1) **Agriculture in Oxfordshire**, a Survey made on Behalf of the Institute for Research in Agricultural Economics, by JOHN ORR, with a Chapter on Soils by C. G. T. MORISON. [Pp. xii + 239.] (Oxford: at the Clarendon Press, 1916. Price 8*s*. 6*d*. net.)

(2) **Co-operation in Danish Agriculture**, by HAROLD FABER, an English Adaptation of *Andelsbevægelsen i Danmark*, by H. HERTEL, with a Foreword by E. J. RUSSELL, D.Sc., F.R.S. [Pp. xxii + 176.] (London: Longmans, Green & Co., 1918. Price 8*s*. 6*d*. net.)

Agriculture in Oxfordshire is the first of a series of monographs on Agriculture in England which the Institute for Research in Agricultural Economics in Oxford proposes to issue. This intention to make a formal economic survey of English

agriculture could scarcely have come at a more opportune time, and this is even more the case than it was when the scheme was initiated some little time before the outbreak of war in 1914. Even then agriculture, after half a century of indifference and neglect at the hands of most Englishmen, had become a matter of interest to others besides those engaged in it; but now, after the experiences of war, the realisation of the extremely vital importance of the agricultural industry has been brought home to every one. The first results of the proposed survey of English Agriculture are therefore of far more than specialised or local interest. Mr. Faber's work, as its title implies, deals especially with the history and development of the co-operative movement in Danish Agriculture. It is thus written from a different point of view from Mr. Orr's *Agriculture in Oxfordshire*, yet both books give clear pictures of the present position of agriculture in the regions with which they deal.

In his first five chapters Mr. Orr surveys the agriculture of Oxfordshire in five sections, these being based as nearly as possible on the natural divisions. By visiting as many landowners, agents and farmers as possible the material was collected which enabled the author to describe the various styles of farming, the crops and stock, and any special developments in different departments of agriculture, such as market gardening, potato-growing, and especially dairying. For each section of the county questions of transport, housing, and other matters are discussed.

The most interesting part of Mr. Orr's work is that which follows the descriptive survey, and deals with the more general questions of land-tenure and the relationship of landowner to farmer, and of farmer to labourer, and with the administration and management of land. It is also this part of the work which affords a most interesting and important comparison with the Danish conditions described in Mr. Faber's book.

To appreciate the position, it is necessary to recall in outline the general trend of agricultural history in the last century.

After the system of joint tenure of land was brought to an end by the wholesale enclosures at the end of the eighteenth century, the principal feature of agriculture, both in England and Denmark, was increasing production of corn brought about by increasing the corn-growing area and improving the yield on the same area. Corn-growing was combined with the keeping of cattle, but the latter was quite subsidiary to the growing of crops, the cattle being badly fed, especially during the winter. However, to get the best results from the system, it was found necessary to keep more livestock and feed it more liberally. "A considerable difference in the corn crops," says Mr. Faber, "was observed between the ordinary farms and the better managed estates where the impoverishment of the soil was compensated by the greater quantities of more valuable farmyard manure resulting from keeping more stock and feeding them more liberally." Hence, it came about that both in England and Denmark a system of corn-growing combined with the keeping of livestock was evolved. But there was this important difference between the English and Danish systems: in England cattle were kept chiefly for meat production, in Denmark for dairy produce.

This change in agricultural practice came about in the decade 1860–70—that is, just before the period of agricultural depression which set in during the seventies. Owing to development in means of transport corn was brought from America, India, and Australia, and flooded the European markets, and corn was soon followed by animal produce. The resulting competition was felt in all

European countries, and particularly in England. A table given by Mr. Orr on p. 97 of *Agriculture in Oxfordshire* indicates the resulting depression very clearly, and shows how the rent of the same estate fell gradually from £8,583 in 1876–7 to £4,892 in 1902, in spite of a slight addition in area during the interval. During the succeding ten years the rent rose slightly to £5,348 in 1912. The depression was not nearly so great in Denmark as in England. "English agriculture," says Dr. Russell, "suffered a terrible set-back, and did not begin to recover until about 1896. Danish agriculture, on the other hand, was able not only to weather the storm, but even to make headway all the time."

The wisdom of the Danish choice in developing dairying is now obvious, but it seems certain that the great difference to-day between the position of English and Danish agriculture is not to be attributed wholly to the choice made in the sixties. There are two other factors which must have played a great part, in one way or another, in determining the course of events. These factors are no doubt not independent of the first nor of one another. One of them is the system of land tenure, the other is co-operation.

Although in 1910 it seemed that a start had been made at a break-up in this country of the large estates, yet even now, as all through the last century, the system of land tenure is essentially that of the landlord-tenant system, in which the farmer who works the land very generally holds the land on a yearly tenancy. Mr. Orr apparently likes to regard this system as a partnership in which the two partners, landlord and tenant, both have well-defined duties and responsibilities, and in which they each invest a certain amount of capital. Our author states that Oxfordshire farmers approve of the system, the advantage of it being that fluctuations which affect the farmer's position are not so wide and violent as if he owned the land. On the other hand, the system has many disadvantages, which Mr. Orr sets out, such as the failure of landlords to co-operate closely enough in particular schemes, and the inconstancy of the landlord's desire to make money out of the concern, resulting sometimes in very erratic policy, such as an alternation of benevolence and harshness in the matter of rent, which is very unsound economically. Most people will probably agree with the remarks of Mr. Price, quoted on p. 148 of *Agriculture in Oxfordshire*, "I am not sure that the conception of the partnership between landlord and tenant is not carried beyond the limitations of the actual work-a-day world."

That the system, as it is worked at present, is not at all ideal is sufficiently indicated by the following quotations from Mr. Orr: "What are regarded as defects in soil and climate frequently cause loss and disappointment, but if the men engaged in developing the different branches of the industry were to fill their parts more fully, and were to stand by each other, these physical difficulties could be made less formidable." "Thus landlords are not and cannot be present on their estates as they once were. It is, perhaps, not too much to say that they will never be present again with the same intensity of interest which they once displayed." "It [agriculture] has suffered by the loss of the landlords' interest." "This process [*i.e.* removal of the landlord's interest] has gone so far that agriculture has been left relatively, at least, in a looser and less organised state than before."

The system of tenure in Denmark is very different. From tables given by Mr. Faber it appears that even in 1873 no less than 81·1 per cent. of agricultural holdings were held freehold, while in 1905 the number had increased to 89·9 per cent., covering about 7·8 million acres, the remaining leasehold and tenancy holdings occupying less than a quarter million acres.

Relatively very few of these holdings exceed 147 acres, and under such circumstances the wonderful co-operation which is such a feature of Danish agriculture was almost certainly necessary for success. The story of this co-operation is admirably told by Mr. Faber from the formation of the first co-operative dairies in 1875 and 1882, in which the principles of co-operation introduced by the Rochdale weavers were developed with such success that larger farmers and estate owners have now mostly joined in the co-operative systems initiated by the small proprietors. From dairies co-operation has spread to egg export and butter export, cattle-breeding, banking, and other activities. Mr. Faber's account shows clearly how this high development of co-operation has played a vital part in the attainment by agriculture in Denmark of its present degree of prosperity.

Whether co-operation can play so leading a part in improving English agriculture is a question which neither of our authors attempts to answer.

The review of agriculture in Oxfordshire given by Mr. Orr is distinctly hopeful, though nothing to excite national pride. Mr. Orr concludes that inferior farm management is largely due to insufficient capital on the part of the farmer, lack of co-operation, inadequate education, neglect of economics. However this may be, it is certain that much more can be done in English agriculture. For instance, the two books before us give some interesting statistics in regard to yield of different crops. Thus, in Denmark in the decade 1875–84, the average annual yield of wheat was 30·9 bushels per acre; in 1909–13 it had risen to 43·1 bushels per acre. In Oxfordshire for the decade 1904–13 the annual yield was 31·15 bushels per acre, and for the whole of England and Wales 31·54. Such figures speak for themselves.

It is impossible to speak too highly of the production of *Agriculture in Oxfordshire*, with its maps and excellent illustrations. By way of criticism, one cannot help feeling, rightly or wrongly, that the author is too cautious in expressing opinions which he must have formed on many matters in the course of his survey. The index might be considerably improved. Perhaps more statistical matter could have been introduced with advantage. In *Co-operation in Danish Agriculture* the misspelling in nearly every case of all words containing the Danish letter "æ" for which the diphthong "œ" is substituted, is irritating, as is also the misspelling of both "by-law" and its Danish equivalent "Bylov." In the opinion of the reviewer the citations used as mottoes on pages ii, 1, and 23 are decidedly commonplace for the purpose for which they are used, and the book would be better without them.

REVIEWS

MATHEMATICS

A Treatise on Gyrostatics and Rotational Motion: Theory and Applications. By ANDREW GRAY, F.R.S., Professor of Natural Philosophy in the University of Glasgow. [Pp. xx + 530.] (London: Macmillan & Co., Ltd., 1918. Price 42*s.* net.)

ESPECIALLY in recent years, the theory and applications of gyrostatic action have occupied the minds of many people. This is due, to a large extent, to the importance of inventions depending on gyrostatic principles, such as torpedoes, which is even greater in the time of war than in times of peace. This very thorough and systematic treatise, which is very finely produced by the publishers, is of great value both to students and to would-be inventors. There is much about this book that will be very attractive to those who have tendencies opposed to the traditional "academic" ideals. The aim of the present work is "to refer, as far as possible, each gyrostatic problem directly to first principles, and to derive the solutions by steps which could be interpreted at every stage of progress" (p. v).

After an introductory account of many striking gyrostatic phenomena, the chapters (II–XXIII) deal in succession with dynamical principles; elementary discussion of gyrostatic action; systems of co-ordinates and their relations, and "space-cone" and "body-cone"; the simpler theory of tops and gyrostats; further discussion of the rise and fall of a top when the precession is not zero; gyrostats and various physical applications of gyrostats; vibrating systems of gyrostats, and suggestions of gyrostatic explanation of properties of matter; the motion of chains of gyrostatic links, and magneto-optic rotation; the earth as a top, precession and rotation, and gyrostatic theory of the motion of the nodes of the moon's orbit; the free precession of the earth: further discussion; calculation of the path of the axis of a top by elliptic integrals; liquid gyrostats, and miscellaneous investigations (such as the motion of a projectile); effects of air-friction and pressure, and boomerangs; the spherical pendulum, and motion of a particle on a surface of revolution; dynamics of a moving frame containing a fly-wheel; motion of an unsymmetrical top; the rising of a symmetrical top supported on a horizontal surface; general dynamics of gyrostatic and cyclic systems; theory of gyrostatic domination; geometrical representation of the motion of a top; analogy between a bent rod and the motion of a top, and the whirling of shafts, chains, etc.; and examples of gyrostatic action and rotational motion.

Of the book we may say, in general, that, like very many other books published in this country, vector *ideas* and traditional co-ordinate *methods* are used (cf., for example, p. 408). "As a rule the method employed has been one of calculating rates of growth of angular momentum for different axes, which amounts to a reduction to practice of vector ideas, and which I devised nearly twenty years ago for use in my own teaching and dynamical work" (p. vi). It seems rather a pity that the extraordinary simplification introduced by Grassmann's methods should be ignored. Also, when discussing non-holonomous systems in Chapter XIV, it is unfortunate that no notice should have been taken (as there might have been on

p. 414) of Appell's equations or the fact that Gauss's principle of least constraint leads to these equations, which are more general than Lagrange's equations in that they apply to non-holonomous systems. Ferrers's generalisation of Lagrange's equations is given (p. 412), but Ferrers's name is not mentioned.

PHILIP E. B. JOURDAIN.

An Introduction to the Algebra of Quantics. By EDWIN BAILEY ELLIOTT, M.A., F.R.S., Waynflete Professor of Pure Mathematics, and Fellow of Magdalen College, Oxford. Second Edition. [Pp. xvi + 416.] (Oxford: Clarendon Press, 1913. Price 15*s*. net.]

THIS well-known, lucid, and well-written book has two objects: the first is didactic, and the second is to help the investigator in his researches. The second object is intended to be fulfilled especially by the chapters near the middle of the book (p. v). The first edition was published in 1895, and both the first and this edition are characterised by the use of methods which are distinctively English. Very little attention has been given to the symbolical methods of Aronhold, Clebsch, Gordan, and others; and, now that an introduction to these methods has been published in English in Grace and Young's *Algebra of Invariants*, this omission is certainly excusable (pp. vi, viii). Thus Gordan's own proof that the number of irreducible invariants of a binary p-ic is finite for any value of p is not given. The proofs given are those due to Hilbert (p. 182).

The sixteen chapters deal respectively with principles and direct methods; essential qualities of invariants; essential of covariants; cogredient and contragredient quantities; binary quantics; invariants and covariants as functions of differences; annihilators and seminvariants; further theory of the annihilators and reciprocity; generating functions; Hilbert's proofs of Gordan's theorem; protomorphs, and so on; further theory of seminvariants and the binary quantic of infinite order; canonical forms, etc.; invariants and covariants of the binary quantic and sextic; several binary quantics; binary quantics in Cartesian geometry and restricted substitutions; and ternary quantics and the quadratic and cubic.

The theory discussed in this book cannot, in spite of Sylvester's extravagant claims for the almost universal character of the theory of algebraic invariants, be regarded as more than a very small part of algebra. It seems that writers should point this out, for students are little apt for independence of thought. The apparent importance of what is treated in such books as the present one and a well-known work of Salmon is largely due to historical circumstances. It is, therefore, particularly unfortunate that history is usually suppressed in them. Thus Boole very early discovered (cf. p. 344) an interesting invariant for restricted substitutions. In textbook order such invariants are treated at a fairly late stage, and yet they contain the germ of rational views on the scope of invariant algebra as a whole.

PHILIP E. B. JOURDAIN.

Integral Calculus. By H. B. PHILLIPS, Ph.D., Assistant Professor of Mathematics in the Massachusetts Institute of Technology. [Pp. vi + 194.] (New York: John Wiley & Sons; London: Chapman & Hall, 1917. Price $1.25, or 6*s*. net.)

DR. PHILLIPS'S preliminary volume on the *Differential Calculus* has already been reviewed in SCIENCE PROGRESS (1917, **12**, 343), and both that volume and the present one can be had bound up together from the same publishers ($2, or 9*s*. 6*d*. net). "Throughout this course," says the author (p. iii), "I have endeavoured to

encourage individual work, and to this end have presented the detailed methods and formulas rather as suggestions than as rules necessarily to be followed." This book is very simply written, and is, on the whole, a far better book than the same author's *Differential Calculus*. The examples and both geometrical and physical applications are very good and numerous. Beginning in the traditional way by defining integration as the inverse of differentiation, and hence developing the usual formulæ and methods, definite integrals are then introduced as limits of sums. Then come various geometrical and physical applications, a treatment of approximate methods, and chapters on double and triple integration. The last chapter is on differential equations, and it is both pleasant and interesting to see differential equations treated in a textbook on the integral calculus—where they used to be treated and where it seems that they still should be treated. The rest of the book is made up by supplementary exercises, answers, tables of integrals and natural logarithms, and an index. A very useful book, especially for those who are going to devote their attention to applied mathematics.

PHILIP E. B. JOURDAIN.

Solid Geometry: Including the Mensuration of Surfaces and Solids. By R. S. HEATH, M.A., D.Sc., Professor of Mathematics and Vice-Principal of the University of Birmingham. [Fourth Edition. Pp. iv + 123.] (London: Rivingtons, 1919. Price 4*s.*)

SOME of the propositions in that part of this useful little work which is devoted to lines and planes in space and polyhedra are some of the propositions of the eleventh and twelfth Books of Euclid which are rearranged and given abbreviated proofs. Besides other propositions, Dr. Heath has added a large number of exercises and a chapter on spherical triangles and mensuration of the sphere, cylinder, cone, and polyhedra—"particularly the very general formula known as Simpson's Rule" (pp. iii, 64*a*, 104*b*). Also there is (p. 71) Euler's relation between the number of faces, vertices, and edges of any polyhedron. It may be remarked that the author refers back by name to propositions of Euclid even when they are reproduced in a shortened form in this book; thus, on p.22 he refers to Euclid XI. 10, whereas it would be more convenient for a reader to refer to the version of this proposition on p. 15.

In the preface to the fourth edition the author refers to the great importance of solid geometry in the training of surveyors as well as of students of mathematics. It may perhaps be pointed out that an introduction to the study of the regular polyhedra, such as is given on pp. 30, 65–70, seems to be of the greatest importance in teaching the elements of geometry. It was, perhaps, chiefly the study of polyhedra that brought about the birth of geometry, and that subject shows at once the power that geometry has of adding to a student's knowledge of space. It may be mentioned that Desargues's name is not "Desargue" (p. 107).

PHILIP E. B. JOURDAIN.

A First Course in the Calculus. Part I: "Powers of x." By WILLIAM P. MILNE, M.A., D.Sc., Mathematical Master, Clifton College, Bristol; formerly Examiner in Mathematics, University of St. Andrews; late Scholar of Clare College, Cambridge; and G. J. B. WESTCOTT, M.A., Head of the Department of Mathematics and Physics, Bristol Grammar School; formerly Scholar of Queen's College, Oxford, and University Mathematical Exhibitioner. [Pp. xx + 196.] (London: G. Bell & Sons, Ltd., 1918. Price 3*s.* 6*d.*)

THIS is the latest volume in "Bell's Mathematical Series for Schools and Colleges," and in it "no attempt whatever has been made to base the subject on the modern concept of Number and the Continuum" (p. v) Surely nobody would doubt the wisdom of the authors on this point. Again: "It is assumed that the student has drawn a considerable number of graphs dealing with statistical data, physical phenomena, etc., and that he has a clear-cut notion of what these curves are like as they present themselves in practical work" (p. v). First comes an historical introduction in which the most novel part is the abundant credit given to Isaac Barrow in the history of the development of the infinitesimal calculus: of course this innovation is based on the striking results of the research of Mr. J. M. Child. The whole book is extremely simply written and forms a very easily grasped introduction to the calculus—though one might wish that the authors had not let the task of history cease with the provision of a few pages of biography. The principal features are the extensive use made of graphs on squared paper and the treatment of differential coefficients as rates of gradients (pp. 1, 10, 17). It is to be noted that "differential equations" are spoken of (p. 78) even when they are of the form that leads only to the problems of the ordinary "integral calculus." It would, perhaps, have been advantageous from a student's point of view if the character of the book were even more geometrical than it is: the example of Barrow's work must have shown the authors how a systematic calculus might be written starting from the treatment of the problems of tangents and areas and then unfolding the discovery—which is the real gist of the whole matter—that these are inverse problems. On pp. 55–6 there is a rather unsatisfactory treatment of curvature: the formula is given, but it is remarked that its deduction "requires too hard mathematics for the present stage of the student." The book concludes with useful applications of the calculus to centres of gravity, moments of inertia, and centres of pressures; miscellaneous examples; and an index.

PHILIP E. B. JOURDAIN.

ASTRONOMY

The Binary Stars. By ROBERT GRANT AITKEN, Astronomer in the Lick Observatory. [Pp. xii + 316, with 5 plates and 12 figures]. (New York: Douglas C. McMurtrie, 1918. Price $3.15 net.)

THIS volume forms one of a series of Semi-centennial Publications issued by the University of California. The fact that it is written by one of the foremost of living double-star observers is a guarantee that the practical point of view has been kept throughout in the forefront and the volume will therefore be of special use to the practical double-star observer. It should also prove of great value in the astronomical library as a reference volume on questions connected with double stars. For this purpose the volume has not been spoilt, as so frequently happens, by insufficient indexing. The indexes are very thorough, and give references not only to names and subjects, but also to the star systems to which reference is made in the text.

In the first two chapters the author gives an historical sketch of double-star observation, in which reference is made to all the important early literature. Chapter III deals with methods of observation, and includes a description of the double-star micrometer. Sections on the resolving power of a telescope, eye-pieces, diaphragms, etc. are included. The whole of this chapter should be carefully studied by the embryo observer, as it is full of practical hints. Even the veteran may pick up some useful hints. Chapter IV deals with methods of

computing orbits for such systems as show orbital motion : a bibliography of the literature on the subject is added for the use of those desirous of pursuing the the matter further. Chapter V, on the "Radial Velocity of a Star," is written by Dr. J. H. Moore, again from practical experience, and deals with the determination of radial velocities with the aid of the spectroscope. In the following chapter the methods of computing the orbits of spectroscopic binaries are explained. Both in this chapter and in Chapter IV numerical illustrations are included, so that the method used may be made clearer. In Chapter VII the much more difficult problem of determining the orbits of eclipsing binary stars is dealt with : the author has copied extensively from the writings of H. N. Russell and Harlow Shapley on this subject, and their treatment of it could not be bettered. In Chapter VIII are collected together the results derivable from the known orbits of visual and spectroscopic binary stars, which are summarised in two useful tables at the end of the book. The following chapter gives a fairly detailed account of the binary systems of more particular interest whose orbits have been well determined. In Chapter X is given a statistical study of the visual binary systems in the northern sky. The information contained in this chapter and Chapter VIII contain the data at present available which must form the basis of any discussion of the origin of binary systems ; the various theories of their origin which have already been proposed are summarised in Chapter XI. The mathematical treatment of this branch of the subject has been somewhat neglected in the past, though some important papers by J. H. Jeans have recently appeared and form a valuable contribution to it.

Mr. Aitken is to be congratulated on a lucid, well-written book which is a valuable contribution to astronomical literature.

H. S. J.

A Treatise on the Sun's Radiation and other Solar Phenomena. By FRANK H. BIGELOW, M.A., L.H.D. [Pp. x + 385, with 44 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd. 1918. Price 23*s*. net.)

THIS volume is a continuation of the author's meteorological Treatise on Atmospheric Circulation and Radiation which was published in 1915. In that volume an attempt was made to work out a system of dynamics for the terrestrial atmosphere which should be capable of representing the observational data provided by balloon ascents. Owing to the circulation and radiation in the atmosphere, the assumption of static adiabatic conditions is naturally far from the truth, and Prof. Bigelow proceeded to derive a system of equations in which the specific heats and gas-constants were not assumed to remain constant.

In the present volume an attempt on somewhat similar lines is made to explain the phenomena of the sun's atmosphere. We do not propose to discuss in detail the amount of success attained by the author in attempting to explain the many solar phenomena brought under review. It is necessary, however, for the reader of the volume to bear in mind that the system of dynamics proposed by the author is not only revolutionary in character but is entirely without support from direct laboratory experiment. Its acceptance would involve the entire recasting of many of our physical ideas, and we doubt whether the author does not tacitly introduce more difficulties than he attempts to explain. Some of its consequences are the variabilities of the gas constant, of Planck's quantum h, and of Avagadro's Constant. It also requires the temperature at a given level in a gas mixture to be different for the different elements, in fundamental opposition to the kinetic

theory of gases. The reader will be prepared, therefore, to look with a critical eye at the conclusions arrived at by the author.

Perhaps the point of chief interest attaches to the value of the Solar Constant of radiation. The careful observational data collected by the physicists of the Smithsonian Institute lead to a value for this constant of 1·94 gm. calories per sq. cm. per minute. This value has not been universally accepted, and other physicists argue in favour of a value exactly double this, viz. 3·98: they contend that a large proportion of the radiation is reflected or scattered in the outer layers of the earth's atmosphere. Whilst the question cannot be regarded as definitely settled, the balance of evidence appears to be in favour of the smaller value. Prof. Bigelow claims to have established the validity of the larger value. Before we can accept his proof we should like to have some justification for his system of dynamics, which forms its basis. The same reason leads us to doubt his other conclusions.

The compilation of this volume must have involved an enormous amount of work. The author has collected a large mass of data and made lengthy series of computations. The determination of variable constants to several decimal places from ordinary observational data is, however, but labour lost. The whole volume leaves one with the impression of a massive edifice built without any foundation.

H. S. J.

The Australian Environment (especially as Controlled by Rainfall). By GRIFFITH TAYLOR, D.Sc., B.E., B.A., F.G.S., F.R.G.S., Physiographer in the Commonwealth Bureau of Meteorology. [Pp. 188, with 15 contour maps in colour, a solar-control model, and 167 other maps and diagrams.] (Commonwealth of Australia, Advisory Council of Science and Industry, Memoir No. 1. Melbourne, 1918.)

THE sub-title of this memoir is, "A regional study of the topography, drainage, vegetation, and settlement; and of the character and origin of the rains," and this sub-title adequately explains the purpose of the memoir. It is one of a series dealing with the climatic control of settlement in Australia, and follows logically upon an earlier memoir in which the effects of temperature and humidity on settlement were investigated, in an attempt to decide which portions of the continent were most suitable for white settlement.

The climate of Australia is peculiarly suitable for study in this manner. This is due to the fact that Australia offers the widest stretch of low and level land in the world, which is affected by the trade wind. This results in many phenomena, such as seasonal rainfall, being presented in their least complex form. Thus, the relationship between the motion of the sun in declination and the seasonal variation in rainfall is remarkably close: it has been illustrated by the author by means of an ingenious solar-control model. In this way he links up the rainfall areas, pressures, storm systems, and permanent winds.

Other subjects discussed, which are of particular importance to the agricultural settler, are rainfall "reliability" and rainfall "uniformity." A place with high rainfall reliability is one in which the percentage variation in any year of the total ainfall from its mean value is small; a place with a high rainfall uniformity is one in which the amount of rainfall is well distributed throughout the year. In some parts of Australia the rainfalls are very erratic from year to year and also from month to month. It is shown that, from this point of view, the Perth region is one of the most suitable for settlement.

The major portion of the memoir consists of detailed regional studies, the whole of Australia being divided into fifteen regions whose physiography is discussed with the aid of numerous maps and diagrams. In these studies such matters as topography, drainage, vegetation, rainfall distribution, health, economics, etc., are dealt with.

The memoir forms an important contribution to our knowledge of the meteorology of Australia. Meteorology often consists merely in the endless compilation of statistics of one sort and another, which are put to little or no use. The studies of Australian climate, upon which Mr. Griffith Taylor is engaged, ensure that this will not be the case in Australia.

H. S. J.

A Star Atlas and Telescopic Handbook. By A. S. NORTON, B.A. [Pp. 26, with 16 maps.] (London: Gall and Inglis, 1919. Price 8*s.* 6*d.* net.)

The appearance of a second edition of Norton's useful little star atlas is worthy of mention. The atlas contains 16 maps, opening in pairs. Successive sheets have a wide overlap, so that on one folio about one-fifth of the entire heavens is seen at once. Some 6,500 stars and 600 nebulæ are shown for the epoch 1920, so that practically all the naked-eye stars are included. The new edition will form an excellent companion to the new edition of Webb's *Celestial Objects for Common Telescopes.* The opportunity has been taken to correct errors in the first edition: the introductory matter has been extended; amongst the material added being an index to the sketch-map of the moon, notes on lunar formations, etc. The atlas will prove of great use to students and amateurs.

H. S. J.

PHYSICS

Mirrors, Prisms and Lenses. A Text-book of Geometrical Optics. By JAMES P. C. SOUTHALL. [Pp. xix + 579, with 247 diagrams.] (New York: The Macmillan Company, 1918. Price 17*s.* net.)

IN the preface to this volume the author remarks that "At present, geometrical optics would seem to be a kind of Cinderella in the curriculum of physics, regarded, perhaps, with a certain friendly toleration as a mathematical discipline not without value, but hardly permitted to take rank on equal terms with her sister branches of physics." Although this remark was intended to apply to American colleges and universities, it was equally true, at least until quite recently, of this country. The war roused us to a realisation of the importance of the study of geometrical optics, and a department of Applied Optics was formed at the Imperial College of Science and Technology, with immediate success. The superiority of the optical designs of the German munitions of war over those of the British and French is attributable largely to the importance which the Germans have attached to this subject.

The appearance of a comparatively elementary text-book by such an authority as Prof. Southall is therefore very opportune. To a certain extent the volume is an abridgment of his well-known treatise on *The Principles and Methods of Geometrical Optics*, with, however, the addition of a considerable mass of new matter, such as an account of the fundamental principles of ophthalmic lenses and prisms. This latter is to be welcomed; spectacle optics is now leaving behind empirical methods and becoming a scientific subject, and its inclusion in a scientific text-book will extend the appeal of the book to a wider circle of readers.

The contents of the volume do not call for detailed description: they have been selected with care, and the treatment has been adapted for those who have but little mathematical knowledge. A detailed account of the eye is included: the general theory of the symmetrical optical instrument is explained, and the theory of chromatic and spherical aberrations are briefly treated. An account is also also given of Von Seidel's formulæ for the five spherical aberrations in the case of a system of infinitely thin lenses. An account of central collineation of object and image spaces has been wisely included. Numerous examples for the student to work are given at the end of each chapter. Attention should be drawn to the fact that figures 31 and 32 have been entirely omitted.

The author is an experienced teacher, and we hope that the volume will succeed in stimulating interest in the study of geometrical optics, and form a ground-work from which the student can proceed to the study of more advanced treatises.

H. S. J.

CHEMISTRY

A System of Physical Chemistry. By WILLIAM C. McC. LEWIS, M.A., D.Sc., Brunner Professor of Physical Chemistry in the University of Liverpool. (Textbooks of Physical Chemistry. Edited by SIR W. RAMSAY.) (London: Longmans, Green & Co.)

Vol. I. *Kinetic Theory* [Pp. xii + 494, with diagrams.] (London, 1918. Price 15*s*. net.)

Vol. II. *Thermodynamics.* [Pp. vi + 403, with diagrams.] (London, 1919. Price 15*s*. net.)

Vol. III. *Quantum Theory.* [Pp. viii + 209, with diagrams, and 2 Appendices by JAMES RICE, M.A., Lecturer in Physics in the University of Liverpool.] (London, 1919. Price 7*s*. 6*d*. net.)

THE first two-volume edition of this work was reviewed in these pages in January 1917. The present edition differs chiefly in the addition of a third volume on the Quantum Theory. Vol. I now contains certain fresh chapters dealing with X-rays and Crystal Structure, the Colloidal State, Theories of Catalysis, Displacement Effect, the Mechanism of Surface Effects, and a brief discussion of Entropy; whilst in Vol. II space has been found for chapters on Osmotic Pressure, the Modern Theory of Dilute Solutions, and the Theory of Vegetable Tanning.

Vol. III deals, of course, primarily with Planck's Quantum Theory and the various researches and theories connected therewith, notably those of Einstein, Nernst, Lindemann, Bohr, and Moseley.

There are three Appendices, of which the first two are by Mr. J. Rice, and are concerned respectively with "Maxwell's Distribution Law and the Principle of Equipartition of Energy" and "Foundations of the Quantum Theory," the third Appendix containing a summary of Krüger's papers on the Theory of Gyroscopic Molecules.

Whilst it is evidently desirable that English readers should have an authentic account of modern development in the field of subatomic phenomena and radioactivity and all which deals with that borderland of science lying between chemistry and physics, one is seriously tempted to inquire whether the third volume has not overstepped the limits of chemistry altogether, or, at most, whether it would not be more correct to include it in a "System of Chemical Physics"!

Still, whichever way one chooses to take it, Prof. Lewis's *trio* will doubtless find a large and appreciative audience of chemists and physicists even though the

latter may occupy most of the front seats for the performance of the third movement! F. A. MASON.

Recent Advances in Physical and Inorganic Chemistry. By ALFRED W. STEWART, D.Sc., with an Introduction by (the late) SIR WILLIAM RAMSAY, K.C.B., F.R.S. Third Edition. [Pp. xvi + 284, with 23 illustrations.] (London: Longmans, Green & Co., 1919. Price 12s. 6d. net.)

ALTHOUGH nominally a third edition of Dr. Stewart's well-known book, so much fresh matter has been inserted and so much has been rewritten that it is practically a new work.

The second edition contained fourteen chapters, whilst the present edition contains twenty, of which twelve are entirely new and the remainder have been largely rewritten; eight chapters deal with subjects selected from pure and applied Inorganic Chemistry, six chapters with Physico-chemical subjects, and four with Radioactivity.

Perhaps not the least interesting chapter is the last, which, under the name of "Conclusion," deals with various points in Inorganic and Physical Chemistry still requiring further investigation and should serve to stimulate would-be researchers who may be on the look-out for new worlds to conquer. Amongst the subjects dealt with may be noted Electric Furnaces and their products, Nitrogen Fixation, the Permutites, Peroxides and Peracids, Active Nitrogen, Absorption Spectra, X-rays and Crystal Structure, Rare Earths and Gas Mantles, Pseudo-Acids, Rare Gases, Positive Ray Analysis, and several chapters on Radioactive Phenomena. Readers thus have a wide choice of subjects, though a cursory inspection leads rather to the view that the chapters dealing with purely theoretical matters are perhaps more satisfactory than those on the practical applications; possibly, however, this may be due to the absence of illustrations of actual plant used, as, for instance, in Nitrogen Fixation; it is always somewhat difficult to deal satisfactorily with technical subjects without a large number of photographs and diagrams.

Chemical science develops so rapidly almost from day to day that those who desire to become acquainted with the latest work on Inorganic and Physical Chemistry and Radioactivity will be grateful to Dr. Stewart for providing them with the requisite information in so palatable a form. F. A. M.

Osmotic Pressure. By ALEXANDER FINDLAY, M.A., D.Sc., F.I.C. Second Edition. [Pp. xi + 116, with 10 figures.] (London: Longmans, Green & Co. Price 6s. net.)

SINCE the publication of the first edition, six years ago, a good deal of fresh light has been thrown on the subject by new researches and theories, and Prof. Findlay has taken advantage of the issue of a second edition to incorporate these results in his book, whilst a fuller description has been given of the apparatus employed by Morse and his collaborators.

The importance of osmosis in many branches of chemistry, and above all its rôle in the mechanism of the vital processes in plants and animals, renders the present volume of great value.

The subject is dealt with largely from the standpoint of the mathematical physico-chemist, but, having regard to its importance in the living organism, one could have wished that space could have been found for a chapter on the biological aspect of the subject, which is one that should always be kept in mind.

Prof. Findlay's monograph will, doubtless, find many new readers besides serving to refresh the memories of those who are already familiar with the first edition.

F. A. M.

Monographs on Industrial Chemistry. Edited by SIR EDWARD THORPE, C.B., LL.D., F.R.S. (London: Longmans, Green & Co., 1919.)

(1) **The Natural Organic Colouring Matters.** By A. G. PERKIN, F.R.S., F.R.S.E., F.I.C., and A. E. EVEREST, D.Sc., Ph.D., F.I.C. [Pp. xxii + 655.] (Price 28*s*. net.)

THE shortage of synthetic dyes occasioned by the war has caused a renewed interest to be taken in the natural dye-woods and colouring matters, so that from the practical point of view no less than from the theoretical the present volume is very welcome. It is somewhat remarkable that although a large amount of research has been carried out on this subject during the last half-century, particularly in this country, there has hitherto been only one book dealing with it, and that not in English, namely Rupe's *Chemie der Natürlichen Farbstoffe*, which is already somewhat out of date, so that the work under review should supply a definite need among English chemical literature.

The chapters deal with dyes belonging to the following groups: Anthraquinone, Naphthaquinone, Benzophenone, Xanthone, Chalkone, Flavone, Flavonal, γ-Pyrane (including the Anthocyan group), Dihydropyrane, Coumarine, Dicinnamoylmethane, Diphenyldimethylolide, Tannins, Coumarane, Indole, and Isoquinoline, and also with the colouring matters derived from Lichens and those of unknown constitution. An appendix is given containing a list of the natural dyes. The work is well indexed, and a full supply of references is provided, so that those chemists whose interests lie in this direction will appreciate so complete and authoritative an account of the subject from the pen of Prof. A. G. Perkin, to whom we owe so much of our present knowledge of the natural colouring matters, whilst those who have hitherto given little attention to this important and fascinating department of organic chemistry will probably be surprised at the large amount of research that has been done which is so well summarised here, and it should serve as an incentive to further work in this field.

As stated in the preface, the intention has been to make the book not only of interest to the student but of value also as a work of reference, and to this end special attention has been directed, in the case of many of the substances, to the details which are essential for their isolation and purification. This is especially the case with the chapter on the Anthocyans, which affords a valuable account of the latest work on this group.

One could wish, however, that in a volume dealing with Industrial Chemistry room could have been found for a few more details as to the practical methods for handling and treating the crude dye-woods, etc., on a commercial scale.

The book is well printed and reasonably free from typographical errors, and one's only regret is that the exigencies of the moment have not permitted the use of a more satisfactory quality of paper for so excellent a work.

(2) **Catalysis in Industrial Chemistry.** By G. G. HENDERSON, M.A., D.Sc., LL.D., F.R.S. [Pp. x + 202.] (Price 9*s*. net.)

THE subject-matter of this volume is divided up into seven chapters, dealing respectively with the General Theory, General Inorganic Reactions and Prepara-

tions, Nitrogen Products, Hydrogenation, Oxidation, Hydration, Polymerisation, and Condensation, and a last chapter on miscellaneous organic preparations and processes. It is a little difficult to follow the classification adopted, nor does the Table of Catalysts on pp. 194–6 help one much; in fact, the Table in question, which is merely a list of substances without any indication of the reactions they catalyse or even the page upon which further information may be found, strikes one as being somewhat valueless, nor does the Index of Subjects offer any help in most cases.

The applications of catalysis are so numerous and varied, and also so scattered, that any attempt to deal with them is to be welcomed, and purely destructive criticism would be as useless as it would be ungrateful. Nevertheless, one cannot but feel that full justice has not been done to this important and fascinating subject, as the main bulk of the work appears to deal chiefly with extracts from the patent literature, whilst discussion of the underlying theories are confined for the most part to the fourteen pages of the introductory chapter. The general treatment of so absorbing a subject is, in fact, rather disappointing. The work may serve as a useful general introduction for senior students to indicate the large extent to which catalysts are used nowadays in chemical industry, but is hardly likely to prove of much assistance to those engaged in chemical industry.

F. A. M.

Coal-Tar Dyes and Intermediates. By E. DE BARRY BARNETT, B.Sc. Lond., A.I.C. [Pp. xviii + 213.] (Rideal's "Industrial Chemistry." London: Baillière, Tindall & Cox, 1919. Price 10*s.* 6*d.* net.)

THE chief point that strikes the reader of Mr. de Barry Barnett's work is the relatively large amount of space devoted to the Intermediates; thus, of 213 pages no fewer than 84 deal with these most important substances. This is quite as it should be, as it is, of course, a well-known fact that about three-quarters of the capital required to build and equip a synthetic dye factory has to be expended upon plant for the manufacture of the essential intermediates, the final stages in the process leading to the actual dyes being usually of a far less involved character.

The book is simply and clearly written, and affords a readable introduction to a very complex and important branch of chemistry. On one point only must a word of protest be uttered—namely, the use of the antiquated terms *benzole* and *toluol* for the pure chemical substances *benzene* and *toluene*. It may be argued that the chemical trade is familiar with these terms, and with such anachronisms as "amido-benzole" for aniline, "nitro-toluol," etc.; but surely that is no reason why modern scientific nomenclature should be discarded in favour of out-of-date terms and formulæ. If chemists are really to be governed by the empirical rules of early Victorian technologists, then let us by all means be thorough, and refer to benzene not as "benzole," but as "bicarburet of hydrogen" (Faraday's original name for the substance), and write aniline as "phogisticated azotic bicarburet of hydrogen"; then surely no one could object!

Whilst we have a carefully thought-out system of chemical nomenclature for organic substances, let us make use of it and have done with "benzole" for benzene and "amido" instead of amino, and so on. Mr. Barnett is not, by the way, guilty of this latter crime.

However, apart from this flaw there is little fault to find with the work, which should be of considerable service to those who have no desire to study more voluminous works on coal-tar dyes. F. A. MASON.

Everyman's Chemistry. The Chemist's Point of View and his Recent Work told for the Layman. By ELLWOOD HENDRICK. [Pp. x + 320.] (London: University of London Press, 1918. Price 8*s*. 6*d*. net.)

THIS is really quite a delightful and refreshing book, which hardened chemists as well as non-scientific folk will read with zest and pleasure. It is a pity that war conditions have necessitated fixing a price which will prevent one from buying half a dozen copies and distributing them among one's friends.

The preface itself explains matters tersely:

"The whole thing is, in a way, a sporting proposition between you, the reader, and me. If I can hold your attention until you have read it through, I shall have succeeded in my undertaking, and you will know something about the Ways of Stuff as the chemist has to do with them. . . . You will not know how the chemist works so much as you will of the way he thinks; and instead of presenting him to you as a superman with potentialities and powers beyond his kind, I have tried to make it clear that his problems are very like those of a business man. . . . My only stipulation is that you shall not attempt to read the book backward."

It would serve no purpose to give a summary of the contents; these the reader must see for himself, but the titles of Chapters I, II, and III, "Chemical Miseries"—including the salutary tale of Dr. Noyadont and how his chemical knowledge saved his fellow Directors from catastrophe—"The Heart of the Thing," and "Phases of Matter," will indicate the nature of the book; or again, Chapter VIII, "The Red-headed Halogens," in which Mr. Hendrick reminds us that "we are wont to attribute to men and women having auburn hair a certain quickness in response to a stimulus. This is a distinct quality of the halogens."

There are one or two slight errors which might be corrected in a later edition: on p 73 it is stated that in the manufacture of cyanamide "the extra carbon is burned away in the heat"; this is, of course, not the case; also Germany is, not was, the largest producer of cyanamide. On p. 149 some reference to rustless steel should surely be included, and the formulæ on pp. 284–5 for trinitrochlorbenzene and picric acid respectively are incorrect.

These, however, are small points, and one can honestly wish the book a large sale, and hope that this will not be Mr. Hendrick's first and last production!

F. A. M.

An Introduction to the Study of Biological Chemistry. By S. B. SCHRYVER, D.Sc. Modern Outlook Series. [Pp. 340.] (London: J. C. & E. C. JACK, 1919. Price 6*s*. net.)

"THE aim of this new series is to present in readable volumes the results of the most recent scholarship in all departments of human thought."

The expectations raised by this statement of the publishers on the wrapper are considerably modified by the statements of the author in his preface that "no attempt has been made to deal with more than one part of the study of biological chemistry—namely, that concerned with the structure of materials of which the bodies of living objects are composed" and that in many parts the book "does not differ markedly from an elementary text-book on organic chemistry." Accepting the position thus taken up by the author, the book is well written and can be recommended to those who desire to obtain an elementary knowledge of that part of organic chemistry treated by the author. Nevertheless, one regrets that the physico-chemical aspect has not been considered, and especially that the part dealing with plant chemistry only occupies 15 pages as contrasted with 140 devoted to animal chemistry.

I. J.

Recent Advances in Organic Chemistry. By ALFRED W. STEWART, D.Sc., with an Introduction by J. NORMAN COLLIE, LL.D., F.R.S. Third Edition. [Pp. xx + 350.] (London: Longmans, Green & Co., 1918. Price 14*s.* net)

WITH the ever-increasing complexity of organic chemistry and the growing tendency towards specialisation it is becoming more and more difficult to keep abreast of the whole field of organic research, and for this reason a book, such as the one under review, must be welcomed by those who wish to acquire some knowledge of branches other than their own, without the labour involved in searching through original papers.

The book opens with an excellent account of the advances in Organic Chemistry during the twentieth century, which makes most interesting reading, and next follow chapters on the Terpenes and the Alkaloids. By cutting out the subject-matter of some of the chapters of the earlier editions, such as the Grignard Reaction, Asymmetric Syntheses, etc., and increasing the size of the book, room has been found for chapters on "Rubber," "Some Aromatic Derivatives of Arsenic," "Trivalent Carbon," "Elements which Exhibit Abnormal Valency," and other subjects which have not as yet found their way into the ordinary textbooks. The author is to be congratulated upon having correctly gauged the spirit of the times by directing the attention of his readers to the chemistry of vital products, such as Chlorophyll and the Anthocyanins, to each of which important subjects a chapter is devoted. The book also includes a chapter on "Some Theories of the Natural Synthesis of Vital Products," largely from the pen of Prof. Collie, whose views, if not accepted in their entirety, must at any rate command attention. The author has successfully maintained the critical tendencies of the earlier editions, and it will be generally acknowledged that no opportunity has been lost of stimulating the spirit of inquiry and pointing out the lines along which investigations might profitably be pursued in the future. Supplemented as it is by frequent references to original papers, the book will prove invaluable to most serious students of organic chemistry.

P. H.

Catalytic Hydrogenation and Reduction. By EDWARD B. MAXTED, Ph.D., B.Sc., F.C.S. [Pp. viii + 104, with 12 illustrations.] (London: J. & A. Churchill, 1919. Price 4*s.* 6*d.* net.)

THIS little book is one of a series of monographs known as "Textbooks of Chemical Research and Engineering," edited by W. P. Dreaper. It is divided into eight chapters, the first three of which are introductory and deal with the preparation of catalysts and the practical methods of catalytic hydrogenation; the three following chapters give examples of the hydrogenation of unsaturated chains and rings respectively, and of miscellaneous other reductions; brief accounts of the experimental conditions are here given, together with references to original papers. Chapter VII is devoted to the reverse reaction of dehydrogenation notably affected by copper and palladium. The last chapter deals with the technical hydrogenation of oils, and describes the more important methods of the commercial preparation of hydrogen. The book contains a quantity of useful information in a small compass, and forms a handy compendium of the rather scattered literature of the subject it deals with.

P. H.

CRYSTALLOGRAPHY

A Manual of Geometrical Crystallography. Treating solely of those portions of the Subject useful in the Identification of Minerals. By G. MONTAGUE BUTLER, E.M. [Pp. viii + 155, with 107 figures.] (New York: John Wiley & Sons, 1918. Price 7*s*. net.)

ACCORDING to the preface this book is intended for those whose sole aim in acquiring a knowledge of crystallography is to use it in the sight-recognition of minerals, and consequently every part of the subject not pertaining directly thereto is rigidly omitted. The detailed treatment of the crystal systems is prefaced by an introductory chapter giving the fundamental definitions and followed by concluding sections dealing with twinning, cleavage, habit, and so forth. The major part of the book is occupied by a description of the forms occurring in the various systems and the means by which they can be readily recognised.

The treatment of the idea of symmetry is very unsatisfactory, as, except for an erroneous definition of a symmetry axis on p. 4, the only symmetry element considered throughout the book is the plane. This neglect of axial symmetry in the discussion of the various crystal classes compels the author to introduce a "law of crystal axes" to explain the presence of two parallel faces in monoclinic and triclinic forms, and then to violate this law in his definition of hemimorphic crystals. The omission, in the systematic portion of the book, of those crystal classes in which no mineral is known to crystallise is to be commended, but the utility of this part would have been increased by the mention of a few mineral examples, particularly as many of the figures represent combinations characteristic of particular minerals. The Weiss notation is used throughout in place of the more usual Miller's symbols, though at the end of each system comparative tables of symbols of several notations are given, but without any explanation of the methods of derivation. In the section on twinning the statement that in certain classes a twin plane may be parallel to a symmetry plane seems to be based on a misconception of supplementary twinning.

It is very doubtful if such a radical departure from the orthodox method of treatment will result in any appreciable conservation of the student's time, particularly where simplicity is attained at the expense of accuracy. The possibility of the student making further excursions into the subject should not be forgotten, and consequently care should be taken that there be as little to "unlearn" as possible.

Many of the figures are set obliquely and a few misprints occur, the most notable being on pp. 100 and 121.

A. SCOTT.

Lectures on the Principle of Symmetry and its Application in all Sciences. BY F. M. JAEGAR, PH.D., Professor of Inorganic and Physical Chemistry in the University of Gröningen, Holland. [Pp. xii + 333, with 170 figures.] (Amsterdam: Publishing Co. "Elsevier," 1917.)

ALTHOUGH the principle of symmetry is of considerable importance in many branches of science, yet very few works, with the exception of some mathematical and crystallographic treatises, discuss it in a systematic way. Morphological symmetry is indeed mentioned in some general biological books, while molecular symmetry, or rather the so-called "asymmetry," is usually discussed in connection with organic chemistry; but the treatment, particularly in the latter case, is often very inadequate, and certainly never systematic. This book, therefore, is assured a welcome, particularly as few authors are so well qualified as Professor Jaegar to deal with this subject. His chemical researches have been largely

connected with optically active compounds, both organic and inorganic, while his crystallographic work has included investigations, not only of the external morphology of crystals, but also of the internal structure as revealed by X-rays.

The earlier chapters of the book are concerned with the general problem of symmetry and the derivation of the theoretically possible types, the latter being based on the methods of Schönflies. Numerous examples of the various types taken from biology as well as crystallography are given, and the symmetry demonstrated by means of excellent illustrations. Although the subject-matter is such that a simple presentation is difficult, the discussion is admirably lucid and sufficiently non-mathematical to be easily followed. Unfortunately a fresh notation for the symmetry groups is introduced, and a tabular comparison with those already in use would have been useful for purposes of reference. The succeeding two chapters deal with the application to crystallography, and cover not only the geometrical theory, based on the limitations imposed by Haüy's law, but also the hypotheses which have been advanced regarding crystal structure, as well as the results of the X-ray investigations of the latter.

The geometrical theory of crystal structure is now fairly complete, and, as several elaborate treatises on it have already been published, in the present book only a general account of the derivation of the space lattices and point systems is given. On the mechanical side, however, the solution of the problem is still very incomplete, and this is instanced by the unsatisfactory nature of the structural theories which have been proposed, while X-ray analysis still suffers from the disadvantage that the results have sometimes been interpreted in more than one way. The author's discussion of the valency-volume theory is scarcely impartial, as comparatively slight mention is made of the weighty objections which have been raised against it. With regard to the interpretation of X-ray analysis, the view that is taken is that, while the crystal structure is built up by the interpenetration of space lattices, each point of which is occupied by an atom, the chemical molecule maintains its entity in the solid state. Although the latter cannot be held to be proved or disproved, yet it seems scarcely correct to say that from the crystallographic point of view the atoms can be arranged into molecules in any arbitrary fashion. Geometrically this may be so; mechanically it cannot be, and crystallography is as much concerned with the mechanical side as with the geometrical. Most of the arguments which have been advanced in favour of the view that the "solid molecule" of sodium chloride contains one sodium and one chlorine atom are equally favourable to the view that the "solid molecule" contains a sufficiently large number of atoms of each type to give a first approximation to an unlimited point system. The description of the author's experimental verification of the limitation of the Laue radiogram—owing to the centro-symmetrical nature of the radiation the thirty-two crystal classes are reduced to eleven—ought to have been completed by an explicit statement of Friedel's earlier theoretical deduction.

The latter portion of the book is occupied by a very able and complete account of Pasteur's law and the associated phenomena. Emphasis is rightly laid on several points which are often lost sight of; for example, that compounds may contain "asymmetric" carbon atoms and yet be inactive and non-resolvable, and that optical activity depends on the compound possessing only axial symmetry. The recent work in this field is critically surveyed and an indication given of the immediate problems awaiting solution. One is glad to find that the author cannot accept Werner's supposed resolution, by spontaneous crystallisation, of a compound, the racemic form of which is less soluble than the active antipodes; according

to present chemical theory such a phenomenon is impossible. The book closes with a suggestive discussion of asymmetric syntheses in nature.

The book is well worth careful study, as much on account of the suggestive discussion of those problems the solution of which is still incomplete, as for the well-digested and complete summary of the work already done. When it is remembered that English is a foreign language to the author, the diction must be regarded as exceedingly good, but although two pages of errata are appended these by no means include all the misprints which ought to be corrected in a future edition. A. SCOTT.

ZOOLOGY

The Quantitative Method in Biology. BY JULIUS MACLEOD, Professor of Botany in University of Ghent. [Pp. viii + 222.] (Manchester: at the University Press; London: Longmans, Green & Co. 1919. Price 15*s*. net).

THIS book, written by Professor MacLeod during his enforced absence from his country, and published by Manchester University Press, is a valuable contribution to the mathematical and quantitative aspect of modern biology. The author writes well, and his thoughts are tersely expressed; the book begins by the demonstration of the underlying fact that the notion of species is a chemical notion, and that though the number of organic substances known to science do not exist in sufficient diversity to explain the occurrence of millions of animal and plant species, the mixture of many of these substances in varying quantities in different species is the explanation of the phenomenon of the multiplicity of animals and plants.

Professor MacLeod distinguishes carefully between "plasticity" of a species such as found in *Leontopodium alpinum* when the alpine form becomes transformed into the low altitude form, and between "complexity" such as is due to the occurrence in one species of several distinct hereditary forms. The author discusses the questions of variation under cultivation and in the state of nature, and considers that the idea that variation is more common under cultivation than in nature is not really true, and he gives examples in support of his claim.

The author draws the attention of zoologists to the wide and as yet hardly explored field of quantitative biology such as is offered to the embryologist and to the systematist, in the development of legs, antennæ, wings, etc. With regard to the question of variation in embryology, the author draws attention to the fact that observers are wont to call a piece of work, such as the examination of two tortoise embryos, a "Memoir on the Embryology of the Chelonidæ," etc., and to base their observations on too small a collection of material. Professor MacLeod believes that the application of the quantitative method to embryology would enable us to reduce the complicated notion of Development to terms of the simple notion of growth of the Primordia (or simple properties).

The last sections of this work are taken up by an explanation of the law of probability in its application to the measurements of variable properties of animals and plants.

Owing to the concise and terse manner of writing and to its arrangement in short paragraphs this book is not very readable; but for consultation purposes the arrangement is very convenient.

Professor MacLeod is to be congratulated on having written a book which is sure to be of use both to the expert and to the student.

J. B. G.

The Origin and Evolution of Life. By HENRY FAIRFIELD OSBORN, Sc.D., Research Professor, Columbia University. [Pp. xxi + 322.] (London: G. Bell & Sons, Ltd., 1918. Price 25*s*. net.)

THIS book is "some of the initial steps toward an energy conception of Evolution and an energy conception of Heredity, and away from the matter and form conceptions which have prevailed for over a century." With this introductory remark the reader expects to find something new, but what he does find is a wearisome reiteration of the words "action, reaction, and interaction of energy," which Prof. Osborn evidently believes is the key to a solution of the problems surrounding life phenomena. The effect of this interpretation of life phenomena in "terms of energy" is, to say the least, fruitless. On the one hand, the author disclaims himself to be either materialist or mechanist, while in a later part of his book he proceeds to give an unconvincing explanation of how life began "by the assemblage one by one of several of the ten elements now essential to life. . . ." Before beginning this physico-chemical explanation of the inception of life, Prof. Osborn postulates the presence in living substance of a hypothetical undiscovered life element "bion," and his several conceptions of life phenomena are contradictory! One does not expect a palæontologist to write a very illuminating essay on chromatin, but one must remark that the author's statement that "Boveri has demonstrated that all body-cells lose a portion of their chromatin, and only the germ-cells retain the entire ancestral heritage," is preposterous. It is a pity that such ill-informed statements should be made by one whose writings are rightly taken by some to be of prime importance. Boveri, it is true, showed that in Ascaris there is a chromatin-diminution process, but this is an exception to the rule that the nuclei in the segmenting egg are equipotent at the time to which Prof. Osborn refers.

Exactly what the author means by distinguishing between "heredity-chromatin" and "body-chromatin" is hard to ascertain. Possibly his misinterpretation of Boveri's views accounts for the attempt to distinguish between the nuclei of the germ-cells and those of the so-called body-cells. The author's use of the term "heredity-chromatin" instead of Weismann's "germ-plasm" is bound to find favour in some quarters. The reader leaves the first part of this book with mixed feelings, but above all with the opinion that the author's "energy concept" of the origin and evolution of life leads to a cul-de-sac, and is not likely to produce any satisfactory result. The second part of Prof. Osborn's book deals with evolution from a palæontological aspect, and here the author is dealing with his own subject. He gives a beautifully illustrated and clear description of various well-known lines of evolution in vertebrates, from materials collected mainly by American authors; while one cannot stint admiration for the valuable work of the latter observers, one is bound to say that Prof. Osborn does not give due acknowledgment to the work of British palæontologists. In his conclusion and elsewhere Prof. Osborn clearly recognises the importance of hormones or chemical messengers in their possible relationship to heredity, especially from the aspect of Lamarckism. The book is well illustrated, and contains a bibliography at the end.

J. B. G.

The Evolution of the Earth and its Inhabitants. Edited by R. S. LULL, Ph.D. [Pp. xi + 208, with frontispiece, 4 plates, and 38 figures.] (Yale University Press; London: Humphrey Milford, 1918. Price $2.50, or 10*s*. net.)

THE contents of this pleasing volume are a series of lectures delivered before the Yale Chapter of the Sigma XI during the Academic Year 1916-17. The following

lectures were delivered: 1. "The Origin of the Earth," by Prof. Barrell; 2. "The Earth's Changing Surface and Climate," by Prof. Schuchert; 3. "The Origin of Life," by Prof. Woodruff; 4. "The Pulse of Life," by Prof. Lull; and 5. "Climate and Civilisation," by Prof. Huntington.

In scope, of course, it is extremely broad, dealing as it does with most of the aspects of the past history of this planet and the forces that have been at work to produce the world and its inhabitants as we see them to-day. It seems rather a pity that it did not include one further chapter containing a more detailed account of the origin of man himself and our knowledge of the development of pre-Grecian arts and civilisations since, although this is but a small branch of animal evolution in general, and as such is treated in lectures 4 and 5. It is a topic of such immediate interest to ourselves, and could have been treated so as not to interfere with either of the two lectures just mentioned. This would, we feel, have rounded off the treatment of the subject as a whole, and have been a fitting inclusion in one of the most interesting and stimulating series of lectures we have read.

At first sight it would appear that such a series, with each subject being treated by a specialist, would of necessity be uneven and scrappy. As a matter of fact the reverse is the case, and the series forms one whole with quite a remarkable unity of idea and continuity of thought. Indeed the plan of the work and of each lecture might have been the work of one man, if such a one could be found, and the differences in treatment and expression are only such as must follow from the variety in the subjects and the personalities of the lecturers.

Perhaps one chapter, the last, does provoke a minor criticism. There does not appear to be a consistent attitude towards the relationship between climate and its effect upon man. It is not quite clear throughout how it is supposed that climate influences human civilisation; whether it is by directly adapting mankind to its conditions as a direct causal agent (a point of view necessitating the acceptance of the doctrine of the inheritance of acquired characters in a modified form) or by the elimination of the non-adapted types. Thus it is suggested that by a series of chemical changes due to climatic influences, the negro has "acquired" an indolent character; again, he has "*not* acquired any special adaptation to a hot climate"; or, further, that climate stress has permanently modified man's mental response. However, "All's well that ends well," and the final conclusion that climate "is not a determiner of civilisation but a condition which prevents civilisation from advancing in some places and stimulates it to greater activity in others" will probably be accepted, and is in accordance with what may be called orthodox biological views.

It is not often that a reviewer is able to agree with the claims of the publisher on behalf of a book, but that pleasure has been forthcoming in the present instance. The statement that "for those who are interested in the various stages of development through which the earth and its forms, organic and inorganic, have passed from their origin to the present age, these chapters, written by scientists of note, offer a comprehensive and readable account," can be thoroughly endorsed. Indeed, it can further be added that the method of presentation in all the chapters is such as to maintain the interest, stimulate thought, and arouse further curiosity.

C. H. O'D.

Forced Movements, Tropisms, and Animal Conduct. By JAQUES LOEB, M.D., Ph.D., Sc.B. [Pp. 209, with 42 text-figures.] (London and Philadelphia: J. B. Lippincott & Co., 1918. Price $2.50 net.)

THIS book is the first of a series of Monographs on Experimental Biology, which

will cover not only that subject in the sense in which it is generally understood, but also that usually designated as General Physiology. The line of demarcation between these two sciences has always been a difficult one to draw, and since they both aim at the investigation and analysis of the phenomena of living beings and the expression of such activities in terms of physics and chemistry, it is wise in such a series of volumes to include both branches of study, which are, after all, fundamentally the same. The editors of the series are Jaques Loeb, T. H. Morgan, and W. J. V. Osterhaut, and these in themselves are sufficient guarantee of the quality of the books to come. This is but the first, and as the list of volumes in preparation, besides those by the editors, includes such names as Conklin, Pearl, and Jennings, the width of its appeal will be manifest Such a series has long been needed in order that biologists in all countries may be kept in touch with this prolific school of American scientists, and it is assured of a good reception.

Loeb's own views on what may be called, for lack of better terminology, the mechanistic conception of life, are too well known to be again made the subject of discussion, and it will suffice to say that they are here presented in his usual vigorous manner. Much of the matter in the first chapters, which are introductory, is familiar to followers of Loeb's work; but its inclusion is necessary for the right appreciation of the discussion. The main part of the book is occupied with the description of new and interesting experiments on the tropisms of the lower animals. Apart from the actual matter contained, which is in itself valuable, there are also described certain experiments and pieces of apparatus that could easily be utilised for demonstrating these reactions in class work.

The author seeks to show that tropisms are the result of a mass-chemical reaction, and the phenomenon mainly investigated was that of heliotropism. It is claimed that all the numerous reactions obtained in the light experiments of other workers and of the new ones here described are the direct result of the photo-chemical activity of the light. Moreover, the law of Bunsen and Roscoe, that the photo-chemical effect produced is a function of the intensity into the duration of the illumination, is held to be valid in all cases of animal and plant heliotropisms. Several very ingenious experiments were devised to test this claim, and the results in all cases appear to justify the author's assumptions.

Although heliotropism is the subject most studied, there are other portions dealing with goetropism, chemotropism, thermotropism, stereotropism, etc., and it is interesting to note that the author has found that the same fundamental tropisms are exhibited by sessile animals, *e.g.* hydrozoa and plants on the one hand, and by motile plants and animals on the other, a point that emphasises very clearly the basic nature of these reactions in living beings.

Whether one agrees with the author's mechanistic explanation of the behaviour of organisms or not, this is a book that will have to be considered seriously by all workers on the subject. The "vitalist" will find in its pages trenchant criticisms of his attitude that cannot be dismissed by any airy platitudes, since they are supported by a wealth of experimental results.

The volume is a fascinating and a useful one, and should its successors in the series maintain the standard it has set, they will perform a signal service to biologists the world over.

C. H. O'D.

A Junior Course of Practical Zoology. By A. M. Marshall, M.A., D.Sc., F.R.S., and C. H. Hurst, Ph.D. Eighth Edition. Edited by F. W. Gamble, D.Sc., F.R.S. [Pp. xxxvi + 515, with 94 figures.] (London: John Murray, 1918. Price 12*s.* net.)

It is unnecessary to review a book like "Marshall and Hurst," for it is in the

hands of every student of zoology, and is the standard practical textbook throughout the laboratories of Great Britain. The fact that the work is in its eighth edition is in itself sufficient guarantee of its general utility. There have been included in this edition a useful note on the *Trypanosome* of the Dogfish, and diagrams of the ventral aspect and a median longitudinal section of the skull of the dog. C. H. O'D.

Civic Biology. A Textbook of Problems, Local and National, that can be solved only by Civic Co-operation. By C. F. HODGE, Ph.D., and J. DAWSON, Ph.D. [Pp. viii + 381, with 2 coloured plates and 168 figures.] (London and New York: Ginn & Co., 1918. Price 7*s*. net.)

THE sub-title of this useful little book indicates very clearly its contents. It is prepared for use in schools and training colleges, and forms a course in biology for older scholars. The particular problems that are studied are those that concern the community as a whole, and can be dealt with only by co operative action, either voluntarily or by means of legislation. This, however, implies an appreciation of the problems involved and an intelligent grasp of the means of studying and dealing with them. The aim of the book is "to make it possible for every one to understand what these [biological forces] are, for good or ill, and how to do his part for his own good and that of the community." Certainly any one who had the good fortune to have worked through a course on the lines suggested by this book would have obtained not only what was aimed at but also a very sound general training in field biology.

The subjects treated—insect pests, fungoid diseases, rat plagues, etc., on the one hand, and the preservation of useful or attractive animals on the other—are illustrated by examples chosen from the United States. The actual species and their life histories therefore would not be the same in Great Britain, at any rate in most cases, but a course drawn up on similar lines dealing with local problems would be an excellent finish to the "Nature study" lessons now given. In fact, in agricultural districts in particular it might replace the so-called "Nature study" with considerable gain to the biological training and future effectiveness of the student as a member of the community. It is a book to be commended.

C. H. O'D.

GENETICS

Genetics in Relation to Agriculture. By ERNEST BROWN BABCOCK, Professor of Genetics, University of California, and Roy Elwood Clausen, Assistant Professor of Genetics, University of California. [Pp. xx + 675.] (New York: McGraw-Hill Book Company; London: Hill Publishing Co., 1918. Price $3.50 net.)

THIS work is really considerably more than its title indicates. It is not merely an account of genetics applied to agriculture; it is an authoritative textbook on the facts and principles of genetics as well as on the application of these to plant and animal breeding.

The book is divided into three parts: fundamentals, plant breeding, and animal breeding. The first part deals entirely with the pure science of genetics, and constitutes a text-book of the subject. While excellent works exist which deal with parts or certain aspects of the subject the present account covers the whole ground of the study of heredity and allied problems. The methods of genetics are, according to the authors, four in number. These are (1) the method of observation, of which Charles Darwin's work is the most brilliant example, (2) the method of experimental breeding to which we owe the great

progress in the study of heredity during the last twenty years: the names of Mendel and de Vries are inseparably connected with this method; (3) the method of cytology, the value of which the authors fully appreciate; and (4) the method of experimental morphology. In genetics this last method has for its object the investigation of the development of the individual as it is related to pro blems of variation and heredity. Work along these lines has as yet scarcely commenced.

The greater part of Part I naturally deals with Mendelism, and special attention is directed to recent developments in Mendelian analysis; naturally much reference is made to the work of Morgan on *Drosophila*, which has extended knowledge in the subject so vastly. Apart from this the authors cite a large amount of work on different species and by different workers, and have exercised great wisdom in their choice of subject-matter. Other matters discussed in the part on Fundamentals are variation in general, the statistical study of variation, in which the principles of biometry are described, species hybridisation involving questions of hybrid form and vigour and sterility, pure lines and mutations.

These Fundamentals occupy nearly half the book. In the second part, on Plant Breeding, the questions of varieties and their origin, selection, hybridisation and hybrids, and mutations in plant breeding are among practical problems dealt with. A chapter is also devoted to the composition of plant populations in which methods of reproduction and fertilisation are considered. Two interesting chapters are devoted to graft hybrids and bud selection, while a chapter on the breeding of disease resistant plants is very stimulating.

The scope of Part III, which deals with Animal Breeding, is similar to that of Part II, selection, hybridisation, disease, the inheritance of acquired characters as well as breeding methods being among the questions discussed. In regard to the inheritance of acquired characters, the authors, after a careful sifting of the evidence, come to the orthodox conclusion that there is no experimental demonstration of the inheritance of any acquired character. The questions of sex and fertility in animals are very adequately dealt with.

As an example of how the different methods of genetics may be correlated in the solution of problems may be mentioned the case of the sterility of the mule, a well-known result of experimental breeding. It appears that the germ-cells of the horse have 19 chromosomes, and those of the ass 32 or 33. Consequently "reduction divisions in the mule are prevailingly abnormal as to chromatin distribution, and no functional spermatozoa appear to be produced."

The book is clearly written, but in the chapter on variation there appears to be some confusion between an external factor or condition and a stimulus, which is a change in the condition. Also the division of variations into four classes, morphological, physiological, psychological, and ecological, does not seem very happy—as, for instance, variations in leaf colour due to habitat might be classed either as morphological, physiological or ecological, at any rate in some cases.

It is impossible in a small space to indicate adequately any more than the general scope of this book, but enough has been said to make it clear that the work supplies a real need. The whole ground of genetics is covered, a wide range of material has been chosen for subject-matter, as is indicated sufficiently in the length of literature cited, and a good balance is maintained in regard to the space devoted to different questions. The work will be invaluable not only to students and teachers of biology and agriculture, but also to research workers both in genetics and plant and animal breeding, and in plant and animal physiology.

In preparing a text-book on a subject which has undergone such enormous and rapid development, the authors imposed on themselves a great and difficult

task, and they have done their work well. The book is well illustrated, and the production leaves nothing to be desired.

W. S.

Genetics Laboratory Manual. By E. B. BABCOCK, Professor of Genetics, University of California, and J. L. COLLINS, Instructor in Genetics, University of California. [Pp. xi + 56.] (New York: McGraw-Hill Book Co.; London: Hill Publishing Co, Ltd., 1918. Price 5*s*. net.)

GENETICS, although essentially an experimental study, tends to become in university courses a lecture subject only. It is to be hoped that the little volume by Prof. Babcock and Mr. Collins will help to serve as a corrective to this tendency.

This book is in many ways a rather wonderful production. In the course of some fifty pages detailed instructions are given for a laboratory course in genetics extending over fifteen or sixteen weeks, one three-hour period being devoted to the subject per week. For each exercise two alternatives are given, so that work is indicated for three such courses. The instructions are strictly limited to those necessary for the laboratory work, the book being intended to supplement the textbook.

The material seems admirably chosen. Recognising genetics as a subject common to the two branches of biology, both animals and plants are selected as material for experimental study. One of the greatest difficulties arising in the planning of courses in genetics is the time factor. Much material requires so long a time to give results that it is ruled out as impracticable for class-work for this reason. Hence the choice by the authors of the fruit-fly (*Drosophila melanogaster*) for breeding experiments is a very happy one. The various experiments with this fly form the subject of the first section. The second section indicates materials for the study of variation in plants. Here the introduction of field study in regard to variation in *Stellaria media* is an excellent idea. The student is also introduced to bud variation, chimeras, and graft hybrids. In the third section Mendelism in plants forms the subject of study; maize, cereals, and other plants are the species selected. The fourth and last section deals with plant and animal breeding. Here the student is instructed in the methods used in the hybridisation of plants. As regards animal breeding, work is designed for acquainting students with the methods of recording pedigrees. This is, no doubt, useful for agricultural students, but for others different material may probably be substituted with advantage.

Although designed primarily for North American workers, the manual should be found extremely useful in British universities and colleges as a guide to what can be done in a short time in introducing students to the methods of genetics. It is to be hoped, however, that the expressions "to hand pollinate," "Mendelizing" used in regard to characters, "to flame" in the sense of sterilising an instrument by putting it in a flame, will not become accepted expressions in British institutions of learning. Apart from the use of these terms, the authors are to be heartily congratulated on this little volume.

W. S.

MEDICINE

Intravenous Injection in Wound Shock. By W. M. BAYLISS, M.A., D.Sc., F.R.S. [Pp. xi + 172, with 59 figures in text.] (London: Longmans, Green, 1918. Price 9*s*. net.)

THE subject-matter of this book constituted the Oliver-Sharpey Lectures, but they

have been considerably amplified and numerous illustrative cases, both experimental and clinical, have been added.

Although confessing at the outset that the primary cause of the symptom-complex, which is known as Wound Shock, is still obscure, the author has none the less rigidly adhered in his researches to the consideration of the treatment of the condition. A mass of evidence, both clinical and experimental, is brought forward to show that the most important physical sign in Wound Shock is the lowering of the blood pressure. The question of "acidosis" is discussed at some length, but much of this section is of necessity somewhat technical and will be lost on the average reader. The results obtained by the intravenous injections of acid solutions are striking, but the author does not allow himself to be "side-tracked" from his original purpose in order to follow up this subject.

From the evidence which Prof. Bayliss brings forward it is clear: (1) that cases of Wound Shock with a low blood pressure—and that includes the vast majority—are greatly benefited by the intravenous injection of gum-saline; (2) that the rise of blood pressure so produced is permanent; (3) that the only cases which fail to react satisfactorily are those in which the condition has been present for so long a period that the vaso-motor centres have become paralysed. That being the case, it is rather disappointing to find that clinicians have not had correspondingly good results in cases of Post-operative Shock treated by this method.

As the reader follows the author's argument as to the *rationale* for the means to be taken to restore the blood pressure to the normal, he will be apt to overlook the brilliant character of the work, so logical and so, apparently, obvious is the author's reasoning.

The views and results of other workers are frequently given, and the references to the literature included at the end of the book are as complete as they are valuable. T. B. J.

The Statics of the Female Pelvic Viscera. By R. H. Paramore, M.D., F.R.C.S. [Pp. xviii + 383, with 26 illustrations, including 23 plates.] (London: H. K. Lewis & Co., 1918. Price 18*s*. net.)

The main purpose of this work is to emphasise the practical functional importance of the musculature of the pelvic floor. In the author's view, the pelvic viscera are retained in place by the interaction of the downward pressure exerted, through the abdominal to the pelvic viscera, by the diaphragm and the muscles of the abdominal wall, on the one hand, and the muscles of the pelvic floor on the other. Consequently, he does not rate the ligaments of Mackenrodt and the other localised thickenings of the pelvic connective tissue as of primary importance for the support of the pelvic viscera. The author strongly opposes the views recently expressed by leading gynæcologists, notably by Fothergill, and does not hesitate to break a lance with certain anatomists. The morphology of the levator ani muscle is discussed in a very convincing manner, but the suggestion that partial regression occurred "because the visceral pressure in the pelvis at some time during evolution became too great for the muscles" (p. 350) is open to too many objections to be considered satisfactory, as the author himself seems to suspect (p. 358).

Unfortunately, there is a tendency to prolixity in the earlier chapters, and the author's penchant for long sentences and parentheses may tend to prejudice the unbiassed reader against the views brought forward, which are none the less eminently sound and supported by evidence which opponents will have great difficulty in controverting.

Contemporary and earlier works are freely quoted and a very complete bibliography is appended. The illustrations are of a high standard, but the identification of (*b*), fig. 25, requires further explanation than it receives in the text.

T. B. J.

The Elementary Nervous System. By G. H. PARKER, Sc.D., Professor of Zoology, Harvard University. [Pp. 229, with 53 illustrations.] (Philadelphia and London: J. Lippincott Company. Price $2.50 net.)

THIS is the second of thirteen monographs on Experimental Biology to be issued under the editorship of Prof. J. Loeb of the Rockefeller Institute and his two colleagues, but of which only two volumes have hitherto been published. Dr. G. H. Parker, the author, is a well-known worker and writer in this field, and the high reputation of the Editors is an assurance that the volume will be appreciated as supplying a need.

The author suggests that, as the dependence of human affairs is so absolute upon the nervous system, there is a place for a work describing its earliest appearance in the simplest and most elementary forms of animal life. He points out that the activities of the simpler animals are often interpreted in terms of human experience, and his researches based upon experimental and quantitative investigations, rather than upon observation and inference, are designed to give a description of the nervous system as it actually is rather than as it is believed to work.

Human beings have often been likened to mere strands in the web of all living things, and we have need to know comparative physiology—as well as pathology—before we can speak authoritatively upon conditions that appertain to ourselves; for, as is well known, parts of the nervous system that are apparently useless and vestigial in man subserve important functions in lower creatures, and conditions that are rudimentary or morbid in man may be normal in the lower animals. For instance, cancer is almost unknown in wild animals, whether free or in captivity, and atheromatous changes in the arteries, which are conditions sequential upon advancing years in man are unknown in animals, even the oldest; nor is the condition described as phlebitis met with in animals, neither are rabbits nor pigeons affected by even large doses of morphia. Therefore, any reliable records of structural dispositions, especially if relating to the nervous system in rudimentary forms of animal life, tend to throw light upon and to assist in the explanation of functions in human beings, and this work is an endeavour to simplify the intricacies of the nervous system in man by unravelling its earliest phases in the lower forms of life.

All fully developed nervous reactions require for their execution a receptor, an afferent conductor, an adjustor, an efferent conductor and an effector. In the lowly multicellular Spongidæ there is no trace of a nervous system. The work under review begins with observations upon the type *Stylotella* as a representative of this class, and interesting experiments demonstrate the fact that the water currents through the pores of the Spongidæ are not due to the flagellated epithelial cells lining the canals, but depend upon the controlling power exercised over the closing or opening of the pores of the osculum, which is effected by the sphincter band of cells on the canal walls; this, in the opinion of the author, being the prototype of involuntary muscular fibres that are the primitive elements of a neuro-muscular mechanism, the elementary muscular substance acting as the effector, yet itself being merely nerveless, irritable protoplasm. As this proceeds in development the next stage is the appearance of a receptor apparatus to be found

in the *Actiniæ* and *Metridium*, in which, for the first time, there appears the dawn of a reflex act. The species *Cordylophora* are studied in detail and are fully recorded, the twelve or more sets of muscles (contracting protoplasm) constituting the only effector by means of which, through the stimulation of touch or heat, the nervous system in the *Actiniæ* is capable of reacting—the cilia, mucous glands, and the nettle cells being apparently uncontrolled by any nervous mechanism whatsoever.

The study of the sensitivity of the marginal bodies in the *Medusæ* shows a gradual development of the receptor element, as isolated sections of this may be associated with the whole—thus anticipating the co-ordinated and unified action of the vertebrate response. The "nerve net," which confers autonomy upon the part supplied, is an illustration of the diffused non-centralised nature of action and response to stimuli, as contrasted with action where there is a central controlling nervous system giving rise to unified movement.

In regard to the origin of Mind, the author points out that in the *Actiniæ* behaviour is conditioned by the immediate environment, and the organism is merely a reactive mechanism, or, in other words, a bundle of reflexes; reaction being caused by present stimuli rather than being a response due to internal states brought about by past activities. The last chapter is a continuation of analogous research upon the Hydroidæ with *Corymorpha palma* as type, and in which, although there is no organised nervous centre, there is a reflex control of the elementary muscular substance.

The conclusion summarises the results of investigations by the author and others in regard to the earliest dawn of a nervous system, which, in the most elementary kind, exists in multicellular animals as smooth muscular fibre. This acts as an independent effector without any trace of a nervous system. The next step in the higher Cœlenterata is the development from epithelial cells of a receptor area, which shows the intimate relation between the epiblastic elements of the nervous and epithelial cells, these becoming responsive to chemical pressure (touch), heat and light stimuli, and in the sea anemones existing as the proto-neurone intermediately placed between the sensory area and the muscle effector, the final neurone being due to the extension of processes of the cell, the union of cells being further effected by intercalary or inter-nuncial neurones, which, in the fully-developed brain, account for its extreme complexity. There are several pages of bibliography which cannot but add to the usefulness of a volume devoted to so specific a study.

ROBERT ARMSTRONG-JONES, M.D.

What is Psycho-analysis? By ISADOR H. CORIAT, M.D. [Pp. 124.] Kegan Paul, Trübner & Co. (London: 1919. Price 3*s*. 6*d*. net.)

IT is two-thirds of a century since Sir William Hamilton arranged the phenomena of Mind under the three categories of cognition, feeling and conation. He maintained that these were discovered by an analysis of consciousness, or by introspection, and he instanced the three phenomena firstly of Knowledge, then of Feeling, *i.e.* of Pleasure and Pain, and lastly of the Will and Conation, by the illustration of a person approaching a picture and becoming conscious, in the first instance, of perceiving a certain complement of colour and form which was cognition; secondly, of experiencing certain feelings, either of gratification if the picture be a masterpiece, or of a more mingled character if the picture be of indifferent merit; and, thirdly, that these experiences would be succeeded by a growing desire to become the possessor of the picture, and, as a consummation of the desire, the will would tend towards its possession.

These three groups of elementary mental units were compared to the red, yellow and blue rays of the solar spectrum, with their three separate qualities,

one associated with heat, the other with light, and the third with chemical effects, and, although each acted its part, it was the unified whole which operated; and the same analogy applied to the mind. But the analysis was carried even further: the red rays were at one end of the spectrum, the blue at the other and the yellow stood in the middle, and these corresponded to the exercise of the mental faculties during the different periods of life: feeling was predominant in childhood, intelligence in the autumn of life, and desire, with its impetuosity and passions, during the period of youth. In may be stated that this analysis of the mental faculties prevailed as the basis of textbook teaching in Psychology until the objective or experimental period when Mercier urged that, in studying the Mind, Conduct should be the ultimate criterion and guide. Following upon this teaching some foreign psychologists, notably Freud and Breuer, based the origin of human activity upon desire; that the strongest desires were implanted in animals and in man as instincts, and that the strongest of these related to self-preservation, to the feeling of hunger, to the seeking for warmth, to sex, and then to a number of binary and even ternary types. The teaching of foreign psychologists laid particular emphasis upon the instinct of sex, going so far as to assert—contrary to general experience—that sex disturbances were at the root of all nervous and mental abnormalities and diseases. The teaching of this school very rightly assumed, however, that the varied operations of the intellectual life, of memory, imagination and reason, of the emotions and the will, resulted from the interaction of sensations and ideas under the laws of association. They further taught that ideas grouped themselves into complexes, which may conflict one with another, some being repressed, whilst others found expression in some sensory, motor or other mental or sympathetic outlet; but that in disease, or under any great overwhelming stress, or even under a stress of a lesser severity but long continued, these groups of ideas became dissociated, dissociation and repression tending to occur chiefly and mainly in connection with ideas which existed during early infantile life; these ideas also carrying with them certain definite emotions, which could not be ascertained because they were only unconsciously active, never rising into consciousness themselves but, being transferred or sublimated into some other form of expression; the difficulty experienced in identifying them being due to what was described as the mental resistance of the individual. If it were possible to discover the links of connection between the present abnormal condition and early infantile emotional states, and if these could only be brought into consciousness and reasoned with, then the abnormal condition disappeared. It will thus be seen that the new school laid as much stress upon the laws of mental association as did its discoverer Hobbes; or Hartley, who was the first to make use of its application to the whole intellectual system. One special method used by the new school to discover the hidden links of association was by means of psycho-analysis—or, according to English pychologists, through psychological analysis—which included in its scheme the interpretation of dreams, because dreams were regarded as unfulfilled desires, and an exploration of these desires, hidden in the unconscious mind, gave the clue to the connection between outward symptoms and their inner and deeper real origin or proximate cause.

It is with the view of making clearer what is meant by this method of examination described as Psycho-analysis that this little volume has been issued, in the form of question and answer, by a well-known supporter and disciple of the practice. In order to carry out this scheme a special vocabulary had to be invented and words such as "sexual," wish, *libido*, have been employed with a new meaning; new terms have also been introduced into mental science, such as "sublimation," when a group of ideas has been transferred to some new social

activity; conversion, if they are represented by sensory, motor or ideational changes; "dramatization," "condensation," and other words are employed in these analytic examinations in such a way as to demonstrate a wealth of imagination hitherto unthought of. It is claimed by his supporters that the work of Freud is, tothe neuroses, what Hume described the law of association to be in the realm of mind, or gravitation in that of matter; but, without derogating from its value in the interpretation of abnormal mental phenomena, it is impossible to subscribe to many of the explanations offered by his advocates. All dreams are not the product of repressed causes, nor is forgetfulness a designed exclusion or a purposeful repression from consciousness; nevertheless, this little Freudian handbook will be of much help to those interested in mental phenomena.

ROBERT ARMSTRONG-JONES, M.D.

The Effect of Diet on Endurance. BY IRVING FISHER, PH.D., Professor of Political Economy, Yale University. [Pp. viii + 55.] New Haven: Yale University Press; London: Oxford University Press, 1918. Price 2*s*. 6*d*. net.)

THIS little book, which owes its appearance to the necessity for Food Economy imposed by the War, is a reprint, revised and abbreviated, of a communication published in the *Transactions of the Connecticut Academy of Arts and Sciences*, 1907.

As the result of an experiment which is fully detailed, the author concludes that, if thorough mastication of food is practised, and if, at the same time, the choice of food is left entirely to instinct, the diet shows a marked reduction in the amount of protein taken. Further, this dietetic change is accompanied by slight loss of weight and strength, but by a marked increase in powers of endurance. The tables, in which the results of the author's experiment are set out, are remarkable not only for the degree of improvement in endurance powers which they show, but also for the fact that there seems to be no relation between the general improvement and the individual improvement in given tests.

The author recognises that the chief obstacle to prevent most people from following his experiment is the practical impossibility in the average home for the food to be selected by appetite only. He does not attempt to state the converse proposition that, if the amount of protein in the diet is voluntarily reduced and substituted by the required amount of fat and carbo-hydrate, thorough mastication will be sufficient after a few weeks to make the alteration palatable.

T. B. J.

MISCELLANEOUS

Library Ideals. By HENRY E. LEGLER. Compiled and edited by his son, HENRY M. LEGLER. [Pp. x + 78.] (Chicago and London: The Open Court Publishing Company, 1918. Price $1.50 net.)

THE late Mr. Henry E. Legler was for some years Librarian of the Chicago Public Library, after serving previously as Secretary to the Milwaukee Board of Education and Secretary of the Wisconsin Free Library Commission. His son, Mr. Henry M. Legler, has compiled and edited, in the present volume, some essays and addresses on library work delivered by his father on various occasions.

In these essays the author explains to us his library ideals. He is a firm believer in the power of Free Libraries to further the development of a rea democracy, to make rural life as attractive as city life, to encourage adult education, to develop the technical resources of the country and to weld together

the miscellaneous races of which the United States makes its citizens. His Librarian is no mere storekeeper of books, but a man of talent and education who will assist readers and be an inspiration and a help to them

Although libraries have long been considered of first importance in the University system of education, their use in schools has been largely neglected. In an essay entitled "Library work with Children," the author discusses how this may best be rectified. To what extent these library ideals have already been attained in the United States may be gathered from various statements by the author. It is plain that the citizens of the United States are far better provided with library facilities than we are in this country, especially in the smaller towns and country districts, the latter being supplied by travelling libraries. As an example of this development of Free Libraries in a country district, it is stated that during the year 1907 in the state of Wisconsin 122,093 books were circulated to the rural population by these travelling libraries.

The book is delightfully written, and we recommend it to those interested in books and in education. We wish that it might be read by our local legislators who are responsible for the application of the Free Libraries Act.

R. E. S.

Joseph Priestley. (Pioneers of Progress: Men of Science.) By D. H. PEACOCK. [Pp. 63.] (London: Society for Promoting Christian Knowledge; New York: The Macmillan Company, 1919. Price 2*s*. net.)

AN excellent sketch of the life and activities of this eminent philosopher is given in the five short chapters which constitute this little book. Theologian, educationist, historian, chemist, and companion to a nobleman—all these aspects of Priestley are properly dealt with by the author. The wretched business of the Birmingham riots which forced Priestley to leave that town and ultimately his country, practically mark the end of his scientific discoveries, and his subsequent life in America is treated in consequence with great brevity.

Priestley gives us the impression to-day of a dilettante rather than a serious student of science. "Chemistry," says our author, "was little more than a hobby to him; theology was his life-work." His failure to do more than he did in advancing scientific knowledge was due, in most part, to two things—to his almost fanatical adherence to the Phlogiston Theory, and to his failure in the matter of quantitative measurement. "Priestley was not only an experimental but also a speculative philosopher and a theologian. His studies in other directions than experimental science would not be such as to lead him to attach weight to quantitative data" "He was at times surprisingly inaccurate in measurement." He had, on the other hand, a contempt for theories and hypotheses, and his view that "if a former theory will sufficiently account for all the facts, there is no occasion to have recourse to a new one, attended with no particular advantage," might be taken to heart a little more in our own time.

The author gives an adequate account of Priestley's chemical work on gases. Perhaps a little more might have been made of Priestley's experiments on plants, several of which revealed fundamental facts of plant physiology. Apart from this the only criticism this writer has to make is in regard to the statement that "the Priestleys were a sturdy, long-lived stock, with the evenness of temperament that comes from generations of healthy ancestors." It would be interesting to know what evidence forms the basis for the view that evenness of temperament is a result of "generations of healthy ancestors."

W. S.

Progress of Education in India, 1912–1917. Seventh Quinquennial Review. Vol. I. By H. SHARP, C.S.I., C.I.E. [Pp. folio ii + iv + 215, with 45 pages of illustrations.] (Bureau of Education, India, 1918. Price Rs. 3.10 or 5*s.* 6*d.*)

FORTUNATELY for the reader, this Review of recent progress of education in India begins with an able summary, whereby the mass of statistics contained in the report becomes intelligible. If a fault is to be found anywhere in the matter of lucidity, it lies in the treatment of the examination system, or rather systems. These have such great influence, often disastrous, in India, that a short chapter might have been added to summarise and elucidate the references to the subject which are scattered throughout the volume.

It is not easy for the Englishman at home to visualise the conditions in India, especially as regards education. Our own system, we think, is none too simple. There we have a white race ruling over an empire of many different races, religions, and castes; in which one sex is almost entirely illiterate; and in which but 3 per cent. of the population is undergoing instruction. The central Government, fortunately, has not made the blunder of trying to centralise education: it deliberately confines itself to the consideration of the various problems in the broadest manner and allows each province to continue to form and administer its own system. Education is mainly in the hands of the universities, local bodies, associations, and individuals. Even the privately managed institutions are in receipt of financial aid from Government or from local bodies. These private institutions include the purely indigenous schools in which are taught Sanskrit, Pali, Arabic, the Koran, etc.

The schools are of three grades—primary, middle, and high. At about five years of age children may enter the primary schools, where the courses are designed to take five or six years, though most of the pupils leave after about four years. The instruction there is entirely in the vernacular. In some of the middle schools, too, no English is taught; in others, as in the high schools, although the teaching is in English, the vernacular is also taught. Still higher in grade come the colleges, which are affiliated to the universities. The education in the elementary schools is now so cheap that it lies within the grasp of all. A system of scholarships of gradually increasing value may carry a pupil from one institution to another until he may win a substantial post-graduate scholarship.

The Review calls attention to the striking top-heaviness of the education. Although the percentage of the population enrolled in the elementary schools is less than it is in Russia or Brazil and is only about one-seventh of that in England, yet the percentage for the secondary schools is not far removed from the figure for this country, and it is also high in the case of the university colleges. The reasons given for these facts are: (1) the education is not compulsory, (2) child labour is prevalent, (3) those who are sufficiently enlightened to send their children to school frequently place a high value upon the education which is given. This disproportion seems to be growing; and as those who leave school too early soon forget what they have learnt and in time marry totally illiterate wives, there are growing up two well-marked classes, the well-educated and the ignorant—an undesirable state of affairs.

The narrowness of the education is to be deplored. "The higher education in India runs in a literary groove and the development of special vocational schools is far behindhand. The genius of the country is speculative rather than practical. The literary courses lead to Government employ and are a necessary preliminary to the study and practice of the law. They adapt themselves to the traditional

method of teaching and to the highly developed memorising faculty which characterises so many Indian students. Technical and industrial studies . . . offer a less easy and less lucrative career. . . . Were industrial employment assured, students would readily come forward and technological institutions would fill and multiply." Yet the teaching of science has not been entirely stagnant. It has become a regular subject of instruction in Bombay, has become compulsory in the high schools of Madras and in the Madras matriculation examination, and optional in the corresponding examinations of the Punjab and Allahabad. The curriculum in science for the Bombay matriculation examination is on liberal lines and is far less dry and formal than in any matriculation examination in Great Britain.

Speaking generally, the examination evil is very prominent. The schools are dominated by the system of external examinations, the passing of which is essential for employment. "Considerable truth is lent to the idea that the modicum of knowledge which can be acquired by a close study and memorising of the text pays better than general mental development."

The influence of the war on progress has, of course, been harmful; yet it has not been wholly adverse: "It has excited interest," says the Bombay report, "among people of all ranks and all ages in great world issues, enhanced their historical and geographical knowledge, broadened their outlook, . . . and united them in the common endeavour of all parts of the empire to contribute toward its successful prosecution." One direct effect of the war has been the awakening of education in Mesopotamia. A new school for teachers and six Government schools have been established there. Six other schools are in receipt of State aid, and a school for instruction in surveying has been opened.

Substantial progress has been made toward the Government of India's resolution of 1913, which indicated as an aim the doubling of the number of primary schools and pupils at no distant date. Thus the public expenditure on education has increased by over 50 per cent.; the number of pupils has grown by 16 per cent.; and the position of the teachers has been ameliorated. The 16 per cent. may seem small as compared with the 50, but the expenditure of every additional pound sterling has resulted in the addition of one pupil to the enrolment. Who would dare say that the money has been wasted?

During the quinquennium, not a little has been done to improve the position of education for girls, though great difficulties, especially of the social kind, have to be overcome. The number of girls under instruction has increased by 29 per cent. More striking still, the number of women training as teachers has grown by 74 per cent.; and two excellent colleges for women have been opened in Madras. The special difficulties in the way of education for the Muhammadans and certain wild tribes and depressed classes are also yielding in a remarkable manner.

Coming to higher education: the five universities of Calcutta, Bombay, Madras, the Punjab, and Allahabad were founded between 1857 and 1887; and until quite recently the number had not been increased. These universities are mainly affiliating bodies, their chief work consisting in the prescription of courses and in the conduct of examinations. Development has taken place in these universities. Thus that of Calcutta now maintains a large law college, has a college of science which is primarily intended for research, and has instituted a department of experimental psychology. The resolution of 1913 called attention to the necessity "to restrict the area over which the affiliating universities have control by securing in the first instance a separate university for each of the leading provinces in India, and secondly to create new local teaching and residential universities within each of the provinces. . . ." Since then, the following have come into existence:

the Benares Hindu University, the Patna University, the University of Mysore, and the Indian University for Women. The schemes for the Dacca, Rangoon, and Nagpur Universities have also progressed, although these have not matured.

These are chief points of the Review, though much has of necessity been passed without comment. There are, for instance, chapters devoted expressly to professional and to technical education. Throughout the work there are frequent references to the social and economic conditions which bear upon the subject-matter. These help to enliven the work, as do the seventy-eight excellent reproductions of photographs—most of them of the buildings which have been erected during the period under review—which are printed as an appendix to the volume. While describing progress, no attempt seems to have been made to minimise the serious defects which still exist. The absence of any suggestion that those responsible will "over-much recline upon achievement" is, perhaps, the happiest augury for the future of education in India.

C. L. BRYANT.

The Australian Army Medical Corps in Egypt. An Illustrated and Detailed Account of the Early Organisation and Work of the Australian Medical Units in Egypt in 1914–15. By Lt.-Col. J. W. BARRETT, C.M.G., M.D., F.R.C.S., and Lieut. P. E. DEANE, A.A.M.C. [Pp. xiv + 260, with 37 Illustrations.] (London: H. K. Lewis & Co., 1918. Price 12*s*. 6*d*.)

IN 1914 we should have thought that Macaulay's Maori gazing on the ruins of London was not much less likely to exist than an Australian R.A.M.C. "orderly-for-the-day" gazing on the ruins of Egypt; but it is never safe to prophesy. The world upheaval has brought many of us into strange lands, and the very title of a book like *The Australian Army Medical Corps in Egypt* cannot but send a thrill through the veins of even the veriest Little Englander. The great Imperial dreams of statesman like Joseph Chamberlain have taken shape and awakened to real life at last, and Germany's savage attempts to sever our Empire have but welded it more firmly together, in bonds that we trust may never come apart. Australia's effort in the war will live for ever in the tales of Gallipoli, Egypt, and Flanders; and her medical service (when it had overcome the initial difficulties of new work in a new country) became as well organised a service as any that have taken part in the great war.

The authors of this volume—Sir James Barrett and Lieut. Deane—are well yoked in harness for the purposes of their book, for the one was the Registrar and the other the Quartermaster of the first Australian General Hospital; and their work thus threw them together in the organisation and setting up of a 500-bed hospital, which eventually became the "mother-hospital" of some 10,000 beds. Sir James Barrett is a well-known oculist in Melbourne, and was for a time A.D.M.S. of the Australians in Egypt, and afterwards a member of the Executive Committee of the British Red Cross there. He has criticised in this book in no mild terms some of the ways of the Australian Red Cross, and there is probably much searching of hearts in Australia at this moment as a result. Whether he was right will be a matter for the future to decide, but the system of dual control, with the balance of weight in favour of the civil authorities, which prevailed at one period of the existence of the Australian Red Cross, was probably not a good one in a theatre of war.

The book is illustrated by many photographs, mostly by Pte. Frank Tate, and for a war-time publication is well got up by Messrs. H. K. Lewis & Co.

T. R. ST.-JOHNSTON.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Empirical Formulas. Mathematical Monographs, edited by Mansfield Merriman and Robert S. Woodward. No. 19. By Theodore R. Running, Associate Professor of Mathematics, University of Michigan. New York: John Wiley & Sons; London: Chapman and Hall, 1917. (Pp. 144.) Price 7*s.* net.

Differential Calculus. By H. B. Phillips, Ph.D., Assistant Professor of Mathematics in the Massachusetts Institute of Technology. New York: John Wiley & Sons; London: Chapman and Hall, 1916. (Pp. v + 194.) Price 9*s.* 6*d.* net.

Synopsis of Linear Associative Algebra. A Report on its Natural Development and Results Reached up to the Present Time. By James Byrnie Shaw, Professor of Mathematics in the James Millikin University. Washington, D.C.: Published by the Carnegie Institution of Washington, 1907. (Pp. 145.)

Lectures on the Philosophy of Mathematics. By James Byrnie Shaw. Chicago and London: The Open Court Publishing Company, 1918. (Pp. vii + 206.) Price 6*s.* net.

Cours d'Analyse Mathématique. By Édouard Goursat, Professeur à la Faculté des Sciences de Paris. Troisième Édition, Revue et Augmentée. Tome I. Dérivées et Différentielles Intégrales Définies. Développements en Séries Applications Géométriques. Paris: Gauthier-Villars et Cie, Quai des Grands-Augustins, 55, 1917. (Pp. 666.)

Matrices and Determinoids. By C. E. Cullis, M.A., Ph.D., Professor of Mathematics in the Presidency College, Calcutta; formerly Fellow of Gonville and Caius College, Cambridge. Cambridge: at the University Press, 1913. (Pp., Vol. I. xii + 430; Vol. II. xxiii + 555.) Price 42*s.* net.

Fermat's Last Theorem. Three Proofs by Elementary Algebra. By M. Cashmore. Revised Edition. London: G. Bell & Sons, 1918. (Pp. 55.) Price 2*s.* 6*d.* net.

From Nebula to Nebula, or the Dynamics of the Heavens. Containing a Broad Outline of the History of Astronomy. By George Henry Lepper, Member of the Pittsburgh Bar. Fourth Edition, Revised and Enlarged. Privately Printed. Berger Building, Pittsburgh, Pennsylvania, 1919. (Pp. 401.)

This volume claims to explain every astronomical phenomenon: startling hypotheses are put forward as a basis of the explanations. Needless to say, these will not stand examination. Whilst acknowledging this, it must be admitted that the book contains much interesting reading and much that is amusing.

Notes, Problems, and Laboratory Exercises in Mechanics, Sound, Light, Thermo-Mechanics, and Hydraulics. Prepared for Use in Connection with the Course in National and Experimental Philosophy at the United States Military Academy. By Halsey Dunwoody, Acting Professor Natural and Experimental Philosophy, U.S.M.A. New York: John Wiley & Sons; London: Chapman and Hall, 1917. (Pp. v + 369.) Price 13*s.* net.

Electrical Phenomena in Parallel Conductors. Vol. I., Elements of Transmission. By Frederick Eugene Pernot, Ph.D., Assoc. Member A.I.E.E., Assistant Professor of Electrical Engineering, University of California; Captain Signal Reserve Corps, U.S.A. New York: John Wiley & Sons; London: Chapman and Hall, 1918. (Pp. xii + 332.) Price 18*s.* 6*d.* net.

An Advanced Course in Quantitative Analysis, with Explanatory Notes. By Henry Fay, Ph.D., D.Sc., Professor of Analytical Chemistry in the Massachusetts Institute of Technology. New York: John Wiley & Son; London: Chapman and Hall, 1917. (Pp. vi + 111.) Price 5*s.* net.

Deals chiefly with methods of mineral and steel analysis.

Recent Discoveries in Inorganic Chemistry. By J. Hart-Smith, A.R.C.S., F.I.C. Cambridge: at the University Press, 1919. (Pp. x + 91.)

This small volume contains a concise and clear account of the more important discoveries in this branch of chemistry within the last fifteen years, so far as they concern the subject as usually taught in schools.

The Physical Chemistry of the Proteins. By T. Brailsford Robertson, Ph.D., D.Sc., Professor of Biochemistry and Pharmacology in the University of California. New York and London: Longmans, Green & Co. (Pp. xii + 483.) Price 25*s.* net.

An Introduction to the Physics and Chemistry of Colloids. By Emil Hatschek. London: J. & A. Churchill, 1919. Third Edition. (Pp. x + 113.) Price 4*s.* 6*d.* net.

Van Nostrand's Chemical Annual. A Handbook of Useful Data. For Analytical, Manufacturing and Investigating Chemists, Chemical Engineers and Students. Fourth Issue, 1918. Thoroughly Revised and Enlarged. Edited by John C. Olsen, A.M., Ph.D., Member of American Institute of Chemical Engineers. Assistant Editor, Maximilian P. Matthias, Ch.E. London: Constable & Co., 10, Orange Street, Leicester Square, W.C., 1918. (Pp. xviii + 778.) Price 15*s.* net.

This new and revised edition should be of great use in the chemical laboratory. The list of all the more important books on chemistry published since 1913 in England, France, U.S.A. and Germany is very valuable.

norganic Chemistry. By James Walker, LL.D., F.R.S., Professor of Chemistry in the University of Edinburgh. Eleventh Edition, revised and enlarged. London: G. Bell & Sons, 1919. (Pp. viii + 327.) Price 5*s.* net.

Outlines of Theoretical Chemistry. By Frederick H. Getman, Ph.D. Second Edition, thoroughly revised and enlarged. New York: John Wiley & Sons. London: Chapman & Hall, 1918. (Pp. xiii + 539.) Price 16*s.* 6*d.* net.

Boiler Chemistry and Feed Water Supplies. By J. H. Paul, B.Sc., F.I.C., Consulting and Analytical Chemist. London: Longmans, Green & Co., 39, Paternoster Row, and Bombay, Calcutta, New York, and Madras, 1919. (Pp. ix + 242.) Price 14*s.* net.

Technical Handbook of Oils, Fats, and Waxes. By Percival J. Fryer, F.I.C., F.C.S., and Frank E. Weston, B.Sc., F.I.C. Vol. II. Cambridge: at the University Press, 1918. (Pp. xvi + 314.) With 69 Illustrations. Price 14*s.* 6*d.* net.

The first volume of this Handbook has already been reviewed in these pages (SCIENCE PROGRESS, April 1918), the present volume giving the necessary practical details for the analysis and estimation of oils, fats, and waxes.

Military Geology and Topography. A Presentation of Certain Phases of Geology, Geography and Topography for Military Purposes. Editor, Herbert E. Gregory. Prepared and issued under the Auspices of the Division of Geology and Geography National Research Council. New Haven: Yale University Press, 1918. (Pp. xv + 281.) Price $1.25.

Shore Processes and Shoreline Development. By Douglas Wilson Johnson, Associate Professor of Physiography, Columbia University. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. xvii + 584.) With 73 Plates. Price 23*s.* net.

The Strawberry in North America. History, Origin, Botany and Breeding. By S. W. Fletcher, Professor of Horticulture at the Pennsylvania State College. New York: The Macmillan Company, 1917. (Pp. xiv + 234.) Price $1.50 net.

Botany of the Living Plant. By F. O. Bower, Sc.D., F.R.S., Regius Professor of Botany in the University of Glasgow. London: Macmillan & Co., St. Martin's Street, 1919. (Pp. x + 580.) With 147 figures. Price 25*s.* net.

A Practical Handbook on British Birds. Edited by H. F. Witherby, F.Z.S., M.B.O.U., Editor of *British Birds Magazine*. In Eighteen Parts. London: Witherby & Co., 326, High Holborn, W.C.1. (Pp. xvi + 64.) Part I, March 3. With Coloured Plate and Text-figures. Price 4*s.* net.

Jungle Peace. By William Beebe, Curator of Birds, New York Zoological Park, and Director of Tropical Research Station. Illustrated from Photographs. London: Witherby & Co., 326, High Holborn, 1919. (Pp. 295.) Price 8*s.* net.

Text-book of Embryology, Vol. II. Vertebrata with the Exception of Mammalia. By J. Graham Kerr, Regius Professor of Zoology in the University of Glasgow. London: Macmillan & Co., St. Martin's Street. (Pp. xii + 591.) Price 31*s.* 6*d.* net.

Sanitation Practically Applied. By Harold Bacon Wood, M.D. New York: John Wiley & Sons; London: Chapman & Hall, 1917. (Pp. vi + 573.) Price 13*s.* 6*d.* net.

Immune Sera. A Concise Exposition of our Present Knowledge of Infection and Immunity. By Charles Frederick Bolduan, M.D., and John Koopman, B.S. Fifth Edition. New York: John Wiley & Sons; London: Chapman & Hall, 1917. (Pp. viii + 206.) Price 7*s.* net.

Animal Parasites and Human Disease. By Asa C. Chandler, M.S., Ph.D. New York: John Wiley & Sons; London: Chapman & Hall, 1918. (Pp. xiii + 570.) With 254 Figures. Price 21*s.* net.

The Human Skeleton. An Interpretation. By Herbert Eugene Walter. New York: The Macmillan Company, 1918. (Pp. xv. + 214.) With 175 Illustrations. Price 10*s.* net.

Catalogue of Lewis's Medical and Scientific Circulating Library. Including a Classified Index of Subjects, with the names of the Authors who have treated upon them. New Edition, Revised to the end of 1917. London: H. K. Lewis & Co., 136, Gower Street, 1918. (Pp. 492.) Price 12*s.* 6*d.* net to subscribers.

The New Physiology and Other Addresses. By J. S. Haldane, M.D., LL.D., F.R.S., Fellow of New College, Oxford. London: Charles Griffin & Co., Exeter Street, Strand, W.C.2, 1919. (Pp. vii + 156.) Price 8*s.* 6*d.* net.

Fresh Hope and Health. For Hospital Patients and Invalids. By Cecilie Muller, Sister of Lieut. Muller, Royal Danish Engineers. Second Edition. London: G. Bell & Sons, 1919. (Pp. 63.) With 26 Illustrations. Price 2*s.* net.

Bacteriology and Mycology of Foods. By Fred Wilbur Tanner, M.S., Ph.D. New York: John Wiley and Sons; London: Chapman & Hall, 1919. (Pp. vi + 592.) With 85 Figures and 10 Plates. Price 27*s.* 6*d.* net.

The Year-Book of the Scientific and Learned Societies of Great Britain and Ireland. A Record of the Work Done in Science, Literature, and Art during the Session 1917-18. By numerous Societies and Government Institutions. Compiled from Official Sources. Thirty-Fifth Annual Issue. London: Charles Griffin & Co., Exeter Street, Strand, W.C.2, 1918. (Pp. vii + 333.) Price 9*s.* net.

Catalogue of the Scientific Serial Publications in the Principal Libraries of Calcutta. Compiled for the Asiatic Society of Bengal. By Stanley Kemp, B.A., with the Assistance of the Librarians of the Institutions Concerned. Calcutta: Printed at the Baptist Mission Press, and Published by the Asiatic Society of Bengal, 1, Park Street, Calcutta, 1918. (Pp. xii + 292.)

The Origin of Consciousness. An Attempt to conceive the Mind as a Product of Evolution. By Charles Augustus Strong, late Professor of Psychology in Columbia University. London: Macmillan & Co., St. Martin's Street, 1918. (Pp. viii + 330.) Price 12*s.* net.

Spirit Experiences. By Charles A. Mercier, M.D., F.R.C.P., F.R.C.S., sometime Examiner in Psychology and Mental Diseases in the University of London; Lecturer in Insanity at the Medical Schools of the Westminster Hospital, Charing Cross Hospital, and the Royal Free Hospital. London: Watts & Co., 17, Johnson's Court, Fleet Street, E.C.4, 1919. (Pp. 54.) Price 9*d.* net.

A Century of Science in America. With Special Reference to the American Journal of Science, 1818-1918. By Edward Salisbury Dana, Charles Schubert, Herbert E. Gregory, Joseph Barrell, George Otis Smith and others. New Haven: Yale University Press; London: Oxford University Press, 1918. (Pp. ii + 458.) With 13 Portraits. Price 17*s.* net.

The War Work of the Y.M.C.A. in Egypt. By James W. Barrett, K.B.E, C.B., C.M.G., M.D., F.R.C.S. London: H. K. Lewis & Co., 136, Gower Street, 1919. (Pp. xx + 212.) With 23 Plates and 3 Maps. Price 10*s.* 6*d.* net.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

RECENT ADVANCES IN SCIENCE

PHILOSOPHY. By Hugh Elliot.

The increasing influence of physiology in purely metaphysical studies is apparent in many directions. An important book, *The Origin of Consciousness*, by Prof. Charles A. Strong (Macmillan & Co., Ltd., 1918), carries a step farther the doctrine of panpsychism, by which some years ago the same author attempted to reconcile metaphysics with the conclusions of physiology. There are various conceivable ways in which the relation between mind and body may be imagined; there is the crude old view of interaction—the theory of two distinct existences acting on one another by forces analogous to physical forces; there are the different varieties of parallelism, which regard conscious processes as shadowy accompaniments of brain processes, but inert and powerless to alter them: such was Huxley's doctrine of epiphenomenalism; there is the theory that cerebral states cause mental states, but cannot be affected by them; and there is the reverse theory of Prof. Strong that mental states cause cerebral states, also without further reaction. The most notable feature in all recent discussions of this problem is the dominance of the physiological standpoint, and the marked tendency emerging therefrom to break down the old sharp distinction of mind and body, and adopt some kind of monistic position. The belief is rapidly gaining ground that the key to the problem lies in *identity*—as to how or where there is no general agreement; but an identity of something or other on the plane of mind with something or other on the plane of body. When the nature of this identity has been determined, the rest may follow with less difficulty. Indeed, physiology has already ruled out so many alternative hypotheses as greatly to narrow the range of speculation, and to suggest that by a continuance of the process we shall ultimately be driven *per exclusionem* to the truth. The essence of the panpsychist

theory, as stated by Prof. Strong, is "the identification of the existence known to us in sense-perception, when what we perceive is the brain-process, with the existence known in introspection." The argument is technical and difficult, but it is carried out on a strictly scientific basis. It still shows, perhaps, a tendency to regard mind too much as a *thing*, rather than a *process*: too much as an *organ*, rather than a *function*.

Dualism, which is thus losing favour in psycho-physiology, has lost favour still more rapidly in other departments of philosophy. The old distinction between noumena and phenomena is abandoned by most philosophers. The new logic and the new psychology both lead to monism. In philosophy, as in physics, analysis discloses at the bottom a fundamental unity, in place of a superficial diversity.

Much discussion has been stimulated by the issue of *Brain* for September 1918, in which the whole number is devoted to a single paper by Dr. Henry Head on *Sensation and the Cerebral Cortex.* A description of this paper does not lie within the purview of the present review; of the many important philosophical corollaries flowing from it there need only be mentioned the illustrations given of a gradual merging of the physiological into the psychical: the condemnation of the introspective method, owing to the fact that the elements of sensation are prepared in a physiological underworld which never attains the sphere of consciousness: and the division of sensation into three aspects (*a*) recognition of spatial relations; (*b*) a graduated response to stimuli of different intensity; and (*c*) appreciation of similarity and difference in external objects. This paper indicates the final and complete subordination of psychology, which is established as a special branch of physiology.

Students of Freud will welcome the new revised and enlarged edition of *Papers on Psycho-Analysis* by Dr. Ernest Jones (Baillière, Tindall & Cox, 1918). This work contains twenty-one new chapters added since the previous edition, which has been out of print for over three years. Dr. Jones is one of the small school who still remain whole-hearted believers in the Freudian doctrine; and his book constitutes by far the best account in the English language of that particular school of Psychology.

Another important publication is the authorised transla-

tion, by Dr. M. D. Eder, of *Studies in Word-Association: Experiments in the Diagnosis of Psycho-Pathological Conditions carried out at the Psychiatric Clinic of the University of Zürich under the direction of Dr. C. G. Jung* (Heinemann, 1918). It consists of a collection of papers published in the *Journal für Psychologie und Neurologie*, by Jung and others of his school. The essence of this psychological method is in requiring the person experimented upon to respond to a given word by another word—the first that comes into his head on hearing the word given. The reaction-time is then recorded. It is found to average 1·5 seconds for the educated; 2 seconds for the uneducated. It is much longer where the word given has a strong emotional tone. There is also wide variation in the type of association. The word "bat," for instance, may induce the response "mammal" or "twilight," etc., or a mere rhyming response like "hat." It is curious that the more superficial types of reaction are especially characteristic of educated persons. The responses of the uneducated are deeper; with them words are more closely associated with their actual meanings, whereas in the educated they more resemble counters or symbols which are dealt with by the mind in relative independence of their meaning. One paper by Dr. Binswanger deals with the psychogalvanic phenomenon, where the occurrence of emotional processes sets up deviations in a galvanometer connected to the person experimented upon. The whole method of word-association is of extraordinary interest, in bringing to the surface highly significant clues to the deep, concealed and unconscious mentality of the individual.

Dr. Jung himself paid a visit to England in July; and on the 11th of that month read a paper at the Royal Society of Medicine in defence of a belief in the psychogenesis of mental diseases. He prefaced his remarks by an attack on scientific materialism, though it is hard to see how the views which he put forward in any way conflicted with that philosophy. Materialism is simply a theory as to the nature of psychical processes; the materialist regards a psychical or mental occurrence merely as the outward symbol of some neural occurrence, not yet experimentally isolated; and he can have no objection to the theory that disease may originate by disturbances of nervous functioning in regions less accessible to

observation, as much as in regions which happen to be more accessible.

Some days later, Dr. Jung again addressed a joint meeting of the Aristotelian Society, the British Psychological Society, and the Mind Association in the course of a symposium held at Bedford College. At the time of writing none of the papers then read have yet been published, though they have been circulated privately.

The British Journal of Psychology for May contains a paper on the *Psychological Interpretation of Sense Data* by John Laird; and several papers on the existence of "General Ability." Professor Carveth Read discusses *The Unconscious*, especially emphasising that common cause of forgetfulness which is due to unconscious repression of disagreeable memories.

Among the Monograph Supplements of the *British Journal of Psychology* must be mentioned *The Distribution of Attention*, by E. Neil McQueen, M.A., D.Sc. (Cambridge University Press, 1917), in which is discussed the capacity of dividing attention simultaneously on several subjects. This capacity does not appear to be specially characteristic of persons of high general intelligence. Motor abilities and general intelligence, on the other hand, seem to be inversely correlated. Where attention is simultaneously directed to two different operations, what actually happens is either that the attention alternates from one to the other, or that one of them becomes automatic, or there is a psychical fusion of the two. The true fixation of attention on two separate tasks simultaneously is found to be very rare.

The *Revue Philosophique* of January—February is notable for a long article by Dr. Pierre Janet, entitled *Les fatigues sociales et l'antipathie*. The main purpose of this distinguished writer is to insist on the contagious character of neurasthenic and hysterical conditions, as a result of which an affected individual constitutes a real danger to those with whom he lives. Dr. Janet points out, further, that mental instability is far more widely spread than commonly supposed, and that a very large number of people commonly regarded as normal are victims of minor mental disorders. We are glad to know that this extremely interesting article is being republished in a three-volume work by the author, entitled *Les médications psychologiques*.

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

IN the present summary, those references which consist of a number in parentheses after a man's name refer to the pages of the last number which has appeared of the *Revue Semestrielle* (1919, **27**, pt. 1). This number is supposed to contain an account of publications in mathematics from April to October, 1918—though many of the papers reviewed in it are of earlier date (cf. SCIENCE PROGRESS, 1919, **13**, 520).

Education.—R. C. Archibald has published, with the co-operation of D. E. Smith, W. F. Osgood, and J. W. A. Young, a report on mathematical teaching in the countries represented in the International Commission on the Teaching of Mathematics of which the organisation was begun at the Congress at Rome in 1908. At present 18 countries have already published 178 reports; Germany has published 50, Great Britain 34, and the United States 14. This report is the fifteenth of those published by the United States, and is entitled *The Training of Teachers of Mathematics* (Washington: Department of the Interior: Bureau of Education, 1918).

Hints for a lecture at an undergraduate mathematical club (cf. SCIENCE PROGRESS, 1919, **13**, 521) on the history of investigations on the number π are given in *Amer. Math. Monthly* (1919, **26**, 209–12). W. Dieck (22) writes on the foundations of geometry as an introduction to the theory of knowledge which is suitable for schools. Other papers of interest to teachers of mathematics are by C. Andriessen (31) and W. Lorey (31).

History.—A new publication devoted to the history of science is the *Archivio di Storia della Scienza*, a quarterly published at Rome and edited by Aldo Mieli, of which the first number appeared in March 1919. The articles and notes which are of the greatest interest in the present connection are as follows: A. Favaro (**1**, 28–38) gives the forty-first article of his series on the friends and correspondents of Galileo: the article is on Matteo Carosio, and a note is given with detailed references to the previous forty articles of the series. G. Loria (*ibid.* 39–47) has a note discussing Cajori's remarks in *Science* for 1918 on a history of mathematics in the nineteenth century. It is of interest that a methodical bibliography on works of the history of science published in Italy will be under-

taken by the *Archivio* (*ibid.* 84–6). There is also a useful set of bibliographical and other notes (*ibid.* 76–83) on Leonardo da Vinci. The appearance of this new journal is a very welcome sign of the movement towards a systematic study of the history of science, which has occupied so much of our thoughts during recent years, chiefly owing to the work of Sarton and others (cf. SCIENCE PROGRESS, 1919, **14**, 2).

G. A. Miller (*Math. Gaz.*, 1918, **9**, 247) draws attention to an error arising from Gow's mistranslation of the German "*Ruthen*" which was used by Eisenlohr to translate an Egyptian word for a measure of length. Gow uses "ruths," while the Egyptian word was entirely different, and seems to have denoted certain pieces of wood. Thus it would seem natural to translate into English by "rods," which is the equivalent of "*Ruthen*"; and "*Ruthen*" was perhaps used by Eisenlohr in a non-mathematical sense—since the length of this measure has not come down to us—just as our word "rods" is commonly used in a non-mathematical sense. The error mentioned has infected other books.

L. M. Klinkenberg (49–50) continues and ends his papers (cf. SCIENCE PROGRESS, 1919, **13**, 521) on ancient Greek geometry. J. A. Schouten (50) gives the first part of an address on modern geometry and algebra. M. Lecat (52) published in 1916 a bibliography of the calculus of variations from its beginnings up to 1850.

A posthumous article, which is to serve as an introduction to the book on *Descartes Savant* (cf. SCIENCE PROGRESS, 1919, **14**, 4), by Gaston Milhaud (*Rev. de. Métaphys*, 1919, **26**, 297–311), justifies in detail the belief of the author in the essential sincerity of Descartes; the accusations of plagiarism which have so often been made against Descartes are explained by the "lesson which results with some clearness from the history of sciences, that there is a kind of current which dominates more or less individual researches at the same time, and which leads men of science to the same truths."

F. Klein, M. Brendel and L. Schlesinger (52) published in 1918 the sixth part, by Ph. Maennchen, of their collection of materials for a scientific biography of Gauss (cf. SCIENCE PROGRESS, 1919, **13**, 522).

Biographies of Darboux (cf. *ibid.* 346) have been written by M. d'Arsonval and A. Lacroix (38) and É. Picard (38). An

account of the life and work of Milhaud (cf. above) is given by A. Lalande in *Rev. Gén. des Sci.* (December 15, 1918, **29**). Among deaths of mathematicians not mentioned before in this quarterly may be noticed A. Benteli of Bern (November 10, 1917, seventieth year); E. Ott of Bern (November 17, 1917, seventieth year); R. Jentzch of Berlin (March 21, 1918); M. B. Weinstein of Berlin (sixty-fifth year) (*Amer. Math. Monthly*, 1919, **26**, 179).

An account is given (*Journ. Indian Math. Soc.*, 1919, **11**, 41–4) of the circumstances which led to the foundation in 1907 of the Indian Mathematical Society by V. Ramaswami Aiyar, under the name of "The Analytic Club."

Logic, Principles, and Theory of Aggregates.—J. Nicod (*Rev. de Métaphys.*, 1919, **26**, 375–86) makes a critical study of E. Goblot's *Traité de Logique* (Paris, 1918), and gives an interpretation of some logical points in modern symbolic logic.

H. Eklund (11) criticises proofs of Schoenflies, Whitehead, Russell, and Brodén (cf. SCIENCE PROGRESS, 1919, **13**, 346), of the non-existence of "aggregates which are members of themselves," and gives an attempt at a theory of such aggregates. H. Dingler (22) continues his considerations on well-ordered aggregates, and gives a reduction to the axiomatic method of that part of Cantor's work which relates to ordinal numbers. L. E. J. Brouwer (49) gives a set of additions and corrections to his Dissertation of 1907 on the foundations of science (cf. SCIENCE PROGRESS, 1918, **13**, 3).

In the theory of point-aggregates, Brouwer (46) considers linear inner limiting sets, and F. Hausdorff (23) gives a generalisation of Carathéodory's concept of measure.

A. Reymond (*Rev. de Métaphys.*, 1919, **26**, 313–34), in a sequel to a criticism of his which rests on a confusion between cardinal numbers and ordinal numbers, attempts to explain his view that, from a philosophical point of view, the definition given by Cantor of transfinite ordinal numbers can be attacked, although technically these numbers are legitimate; and then attempts a strict definition of them. The confusion mentioned seems to be still in force, and to vitiate the proposed definition of transfinite ordinals as certain different arrangements of integer numbers, in which it is not stated whether the integers are cardinal or ordinal. D. Wrinch (*Proc. Cambridge Phil. Soc.*, 1919, **19**, 219–33) investigates the neces-

sary and sufficient conditions that P^Q should be Dedekindian or semi-Dedekindian when P and Q are well-ordered series. In this notation the field of P^Q is the class of Cantor's "*Belegungen.*" The greater part of the paper makes use of the symbols introduced by Whitehead and Russell in their *Principia Mathematica.*

D. Hilbert (17–18; two papers) gives a research, based on the axiomatic method, on the foundations of physics, and F. Klein and Hilbert (21) contribute a note on the first of these papers. On the theory of relativity we may notice papers by A. Einstein (14; two papers), G. Nordström (48), A. D. Fokker (48), Sir Oliver Lodge (42), G. W. Walker (43), J. P. Kuenen (46–7), and L. de la Rive (50).

On the calculus of probabilities, cf. O. Knopf (31), J. Haag (34), and F. Schuh (48).

Arithmetic and Theory of Numbers.—R. D. Carmichael (*Amer. Math. Monthly*, 1919, **26**, 137–46) gives an account of Fermat's numbers $\frac{F}{n} = 2^{2^n} + 1$. In view of the known facts about the factors of $\frac{F}{n}$, Fermat's question whether $(2k)^{2^m} + 1$ is always a prime except when divisible by an $\frac{F}{n}$ is without further particular interest, and in the present paper all the essential facts known about the factorisation of the numbers $\frac{F}{n}$ are gathered together and proved in the simplest ways possible. Also a few minor results are given which appear to be novel. N. M. Shah and B. M. Wilson (*Proc. Cambridge Phil. Soc.*, 1919, **19**, 238–44) give some calculations which originated in a request made by G. H. Hardy and J. E. Littlewood that they should check a suggested asymptotic formula for the number of ways of expressing a given even number n as the sum of two primes—Goldbach's empirical theorem being that any even number is the sum of two primes. Other allied formulæ are also discussed. Hardy and Littlewood (*ibid.* 245–54) give some indication of the genesis of their particular formulæ and others of the same character. S. Ramanujan (*ibid.* 207–10) guesses by induction a general theorem—as yet unproved—on congruence properties of $p(n)$, the number of partitions of n, and gives proofs of two particular cases of

this general theorem. H. B. C. Darling (*ibid.* 217–8) also gives proofs of these two particular cases. G. H. Hardy (*ibid.* 211–6) communicates, with an introductory note, simple proofs of certain identities in combinatory analysis by L. J. Rogers and S. Ramanujan. Rogers first discovered the identities in a paper published in 1894, and Ramanujan rediscovered them in 1913; since then three proofs have been published, but the present two proofs—which are in principle the same—are much simpler than any published hitherto.

Cf. also J. G. van der Corput (48), A. L. Bartelds and F. Schuh (49), O. Szász (13), G. Humbert (33, 34, 35), W. Jänichen (13), N. G. W. H. Beeger (45), V. Brun (11), S. C. van Veer (50), G. H. Hardy (9), P. A. MacMahon (40), E. Maillet (37), E. Hecke (18 [two papers], 29), E. Landau (15, 16 [two papers], 18 [two papers], 24, 27), G. Pólya (13, 20, 26).

Algebra.—G. A. Miller (*Journ. Indian Math. Soc.*, 1919, **11**. 57–9) gives a new interpretation of the ordinary complex numbers $a + bi$ as the translations in a plane defined by $x' = x + a$ and $y' = y + b$. In his words this is a new "proof of the legitimacy" of complex numbers which "is based largely on the group concept, but can clearly be given without the explicit use of this concept." G. W. Smith (*Amer. Journ. Math.*, 1919, **41**, 143–64) discusses nilpotent algebras generated by two units i and j such that i^2 is not an independent unit.

G. A. Miller (*Quart. Journ. Math.*, 1918, **48**, 147–50) gives some theorems relating to substitution-groups on the terms of symmetric polynomials.

Sir Thomas Muir (*Proc. Roy. Soc. Edinburgh*, 1919, **39**, 35–40) gives a note on the determinant of the primary minors of a special set of $(n - 1) - \text{by} - n$ arrays. W. H. Metzler (*ibid.* 41–7) exhibits the rational and real factors of certain forms of circulants.

H. W. Turnbull (*Proc. Lond. Math. Soc.*, 1919, **18**, 69–94) shows how Gordan's system of invariants for two quaternary quadratics can be very much simplified; in fact, the system is reduced to 125 forms instead of Gordan's 580 forms. Turnbull (*Proc. Cambridge Phil. Soc.*, 1919, **19**, 196–206), in connection with this reduction, gives geometrical interpretations to most of the members of this system (which is here stated to number "123 at most").

Cf. also D. R. Curtiss (10), O. E. Glenn (8), I. Schur (26–7,

29), G. Szegö (29), O. Blumenthal (28), C. H. Müntz (19), E. Fischer (15, 17), J. A. Schouten (47), E. Jahnke (12), G. Julia (35, 33), and H. B. A. Bockwinkel (47).

Analysis.—G. Prasad (*Bull. Calcutta Math. Soc.*, 1918, **9**, 1–9) studies in a number of typical cases the question of the existence of the normal derivate of the Newtonian potential due to a surface distribution having a discontinuity of the second kind. B. Datta (*Amer. Journ. Math.*, 1919, **41**, 133–42) obtains by a new method the chief results of C. Niven (1880) on the non-stationary state of heat in an ellipsoid, and shows how this method can be applied to arrive at new results in the case of the ellipsoid with three unequal axes. A. E. Jolliffe (*Proc. Cambridge Phil. Soc.*, 1919, **19**, 191–5) proves a generalisation of the theorem previously proved on certain trigonometrical series which have a necessary and sufficient condition for uniform convergence by T. W. Chaundy and himself (cf. SCIENCE PROGRESS, 1916, **11**, 269). S. R. Ranganathan (*Journ. Indian Math. Soc.*, 1919, **11**, 50–6) establishes the Fourier expansion of Bernoulli's polynomials, and deduces the sums of certain interesting types of series in a finite form.

On the theory of functions of real variables, cf. E. Landau (13), J. Wolff (49), H. Bremekamp (49), M. T. Béritch (33, 34), G. Julia (34), C. J. de la Vallée Poussin (34, 35), A. Denjoy (31), T. Fort (10), G. Pick (15), W. G. Simon (10), P. J. Daniell (10), A. F. Andersen (11), J. Kürschák (21), C. Carathéodory (28), H. Hahn (25), S. Bernstein (22), J. Pérès (34, 35), O. Szász (21, 26, 30), B. Jekhowski (32), and K. Knopp (13). On the general theory of analytic functions, cf. Valiron (32, 33), O. Szász (26), W. Schmeidler (22), R. König (21, 23), P. Stäckel (21), J. Schur (21), L. Lichtenstein (20, two papers), S. Lattès (33), J. F. Ritt (32), G. Faber (30), and H. Bohr (19); on conformal representation and so on, cf. H. Bohr (29), C. Carathéodory and H. Rademacher (11), P. I. Helwig (48); on special functions, cf. G. N. Watson (40, 43), O. Szász (26), G. Pick (25), E. Hilb (25), N. Nielson (11), H. B. A. Bockwinkel (45), J. C. Kluyver (45), N. G. W. H. Beeger (49), L. Crijns (49), W. C. Post (50), and E. Hecke (15).

S. Banerji (*Bull. Calcutta Math. Soc.*, 1918, **9**, 43–58) treats in detail the problem of the forced vibrations of a heterogeneous string, which is a case of boundary problems of ordinary differential equations. A. L. Nelson (*Amer. Journ. Math.*, 1919,

41, 123–32) has a note on seminvariants of systems of partial differential equations.

On ordinary and partial differential equations, cf. F. A. Willers (13), J. B. Pomey (36), G. Hamel (27), O. Perron (24), E. Hilb (19), R. Garnier (31, 33), T. Hayashi (49), L. Fejér (25), G. Giraud (35), and H. Liebmann (30). On the calculus of variations, cf. W. Blaschke (25), and K. Boehm (16); on difference equations, cf. J. Horn (25); and on integral equations, cf. L. Lichtenstein (27), J. Horn (13), T. Lalesco (32, 34), and É. Picard (32).

Geometry.—A. Emch (*Amer. Math. Monthly*, 1919, **26**, 194–201) gives a long and detailed review of Veblen and Young's *Projective Geometry* (cf. SCIENCE PROGRESS, 1919, **13**, 669).

S. M. Ganguli (*Bull. Calcutta Math. Soc.*, 1918, **9**, 11–8) introduces a new method of studying inclinations of spaces of any given number of dimensions less than n which are contained in a space of n dimensions. A systematic algebraic treatment—as distinguished from geometrical points of view—of the subject is attempted on simple principles which are applicable only to Euclidean spaces.

R. Vythynathaswamy (*Journ. Indian Math. Soc.*, 1919, **11**, 46–9) gives some theorems on conics with vanishing Θ, Θ'. P. Franklin (*Amer. Math. Monthly*, 1919, **26**, 146–51) discusses the problem in solid geometry analogous to that of the construction of a circle in a plane from three elements (points, tangent lines, or tangent circles), which is the determination of a sphere from four elements (points, tangent planes, or tangent spheres), and is greatly simplified by the use of certain principles given. W. Sensenig (*Amer. Journ. Math.*, 1919, **41**, 111–22) discusses the invariant theory of involutions of conics.

C. E. Cullis (*Bull. Calcutta Math. Soc.*, 1918, **9**, 23–42) shows (1) that rotations of a rigid body about concurrent axes fixed in space can be replaced by the same rotations about the same axes moving with the body, provided that the order in which the rotations are applied is reversed; (2) that the polar of a spherical polygon admits of a unique definition and has the same uses as the polar of an ordinary spherical triangle; (3) that these results can be used to obtain complete generalisations of Rodrigues' and Sylvester's theorems regarding rotations.

E. H. Neville (*Proc. Cambridge Phil. Soc.*, 1919, **19**, 234–7) proves, in a form purely intrinsic, the relation discovered by Bonnet between the integral curvature of a bounded region on any bifacial surface and the integrated geodesic curvature of the boundary.

On geometry cf. W. Blaschke (13), F. Schuh (49), G. C. A. Koopmans (50), V. Thébault (36, 37), A. Auric (36), De Pulligny (33), M. Brillouin (32), H. Wolff (31), R. Lohnstein (31), W. A. Wythoff (46), M. Pasch (21), E. Kruppa (14), N. Agronomof (37), R. Bricard (34, 36), P. Appell (36), R. Goormagtigh (36, two papers), R. Sturm (29, two papers), W. A. Versluys (46, 49), M. Weill (36), F. Balitrand (36), A. Myller (36), G. Darboux (35), A. Buhl (35), H. du Bois (12), R. de Montessus de Ballore (32), B. Giovanni (44), C. Segre (44), H. Mohrmann (19, 23), E. Müller (12), S. Jolles (24, 28), T. Reye (24), L. Braude (12), P. Tortorici (44), R. Bouvaist (36), K. Popoff (12), C. Guichard (32), H. Vermeil (20), E. Magin (12), J. de Vries (44, 45, two papers), C. H. van Os (46), J. Andrade (34), B. P. Hallmeijer (44, 46), A. D. Fokker (46, 50), A. Denjoy (45, 46), A. Winternitz (28), H. Berliner (22), J. A. Schouten (44, 45), and J. Arnovlievitch (36).

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Royal Observatory, Greenwich.

The Parallactic Motions of the Stars.—Astronomers are under a great obligation to Prof. Kapteyn and his collaborators at the Astronomical Laboratory at Groningen for their valuable discussions of large masses of statistical material bearing upon the arrangement of the stars in space. They have shown a remarkable skill in dealing with diverse results, and though some of their earlier conclusions were based upon comparatively scanty material, there has been no reason to doubt their substantial accuracy. Of recent years, largely through the development of new methods, much additional material has accumulated. Thus, in *Groningen Publications*, No. 8, the determination of the sun's velocity was based upon the radial velocities of only fifty-one stars. Reliable determinations are now available for nearly two thousand. As regards directly determined, or trigonometrical parallaxes, our knowledge is accumulating at a remarkable rate, and is probably at least

twentyfold what it was in 1902; further parallaxes to the extent of three or four hundred will be published very shortly from the Allegheny and Leander McCormick Observatories. It is now possible to discard all the visual determinations except the best of those made with the heliometer, as modern photographic methods give results with a much smaller probable error. In addition, a large number of determinations by the spectroscopic method are accumulating at the Mount Wilson Observatory, and their publication is being temporarily deferred merely until the method of reduction has been made definitive. Shortly we may expect the publication of hundreds, if not of thousands, of spectroscopic determinations. The number of accurate determinations of stellar apparent magnitudes, both visual and photographic, also shows a remarkable increase since 1902: in fact, no really accurate photographic determinations of magnitude were available before 1902. Much work has been done of recent years at Greenwich in this country, at Harvard and Mount Wilson in America, and at Potsdam in Germany, towards increasing our knowledge in these directions. The difference between the photographic and visual magnitudes is called the colour-index of a star, and can be correlated with its spectral type. Our knowledge of the spectral types of stars is mainly due to the valuable work done at the Harvard College Observatory, and it will be enormously increased when the publication of the valuable Henry Draper catalogue (the first volume of which, covering 0*h* to 4*h* of right ascension, has already appeared) is completed. Further, the accurate determinations of the proper-motions of the stars show a great increase, as the interval of time since the first reliable determinations of position increases. The Greenwich and Albany Observatories have done much towards increasing the number of accurate proper-motions.

In view of this enormous increase in the material available for statistical discussion, Prof. Kapteyn has commenced a revision and extension of some of his earlier investigations. These earlier results were mainly applicable to the mean of the whole stellar system: an attempt is now being made to extend them for different galactic latitudes, over fainter stars and over the several spectral classes. In *Groningen Publications*, No. 29, 1918, he discusses the mean parallaxes of stars

of different magnitude, galactic latitude, and spectrum; and a further publication is foreshadowed, dealing with the number of stars of determined magnitude, galactic latitude, spectrum, and proper-motion. The publication under review is divided into two parts: in the first the mean secular parallaxes of stars of different galactic latitude and magnitude are determined. The various material is collected, reduced to a common basis and discussed, and it is concluded that the logarithm of the secular parallax can be expressed by the formula—

$$\log(\text{secular parallax}) = -0{\cdot}428 - 0{\cdot}096 \cos 2b - 0{\cdot}1373\, m,$$

where b is the galactic latitude and m is the apparent visual magnitude. The secular parallaxes require to be multiplied by 0·243 to reduce them to annual ones. In the second part of the publication the result is extended, and formulæ of a similar nature are derived which are applicable to the several spectral types separately. A comparison between observed values and those computed from the formulæ shows a very satisfactory accordance. At the end of the publication are given tables for the mean secular parallaxes of the separated types, tabulated according to visual magnitude and galactic latitude.

The Navigation of Aeroplanes.—An interesting article by Prof. H. N. Russell, in *Pub. Ast. Soc. Pacific,* **31,** 130, 1919, gives an account of some investigations made by him on the determination of the geographical position of aeroplanes by means of sextant observations made during flight. The investigations were made under the authority of the Division of Science and Research of the U.S. Bureau of Aircraft Productions, and Prof. Russell was assisted by Mr. J. P. Ault, navigating officer of the non-magnetic vessel *Carnegie,* of the Carnegie Institution. The development of long-distance aeroplane flights renders this problem one of great importance, which even the improvements in directional wireless telegraphy do not lessen. As bearing upon this question, it is of interest to recall that General Maitland, in his log of the voyage of the R34, puts on record a statement by the navigating officer that an error in position of fifty miles was easily possible from sextant observations using a cloud horizon.

Prof. Russell states that there is no difficulty in using a sextant in an aeroplane provided that it is small and light,

and that, if a telescope is used (one is not really necessary), it should have a low power and a large field of view. The graduations should be very clear, so as to be easily read, and a vernier can be dispensed with. When above flat land or in sight of sea, observations on the natural horizon can be made as easily as at sea, and almost with the same accuracy, the best and most distinct horizon being provided by level land. The value of the dip, of course, reaches remarkably high values, but even up to an altitude of 10,000 feet it is found that the relation holds that the dip in minutes is equal to the square root of the height in feet. With a good horizon the average error of one observation is only about two to three minutes of arc, which fixes the Sumner line with sufficient accuracy.

Under average conditions, there is too much haze for observations to be made on a land or sea horizon at a height exceeding one or two thousand feet. Frequently the haze has a sharp upper boundary at a definite level presenting a definite "false horizon," which is usually sharp in a direction opposite to the sun. Provided that the height above sea-level of the top of the haze is known, which can be determined with the aneroid during ascent or descent, observations upon such false horizons may in general be used, and accurate results obtained. Occasionally, however, very large errors were obtained, the causes of which were investigated. It was found that at times the surface of the haze was irregular, so that, instead of a true horizon being observed, the apparent boundary was the top of a ridge of haze, rising above the general level: thus, in one instance, the observations were in perfect accordance with the existence of a ridge at a distance of 100 statute miles and at a height of 14,800 feet above the sea-level. Since the earth's surface curves away from the tangent by 6,600 feet in 100 miles, the ridge was on the theoretical horizon of the observer at a height of 8,200 feet, and at lesser heights a negative dip was obtained. It is therefore of extreme importance to make certain that the haze is uniform: this can be tested with a dip-measuring instrument, which permits a direct determination of the amount by which the angular distance of two opposite parts of the horizon differs by 180°.

Frequently, however, weather conditions are such that observations, even on a haze horizon, are not possible. The

problem then is equivalent to that of finding the true vertical in a moving aeroplane. This is rendered difficult by the non-uniform motion of the aeroplane, which alters the apparent direction of gravity. Prof. Russell points out that, if a deviation of 15′ is adopted as the greatest allowable, the speed of the aeroplane must never be permitted to change at a rate exceeding one mile per hour in ten seconds, nor must the radius of curvature of the path be less than ten miles (assuming a speed of sixty miles per hour). This requires careful piloting, but is possible with a good pilot who is accustomed to his machine. The first apparatus used was in the form of an artificial horizon, and consisted of a pendulum mounted in gimbals so as to swing with freedom in any vertical plane, and bearing at its upper end a mirror adjustable so that its reflecting surface is horizontal when the pendulum swings freely. The lower end of the pendulum swung in viscous liquid to damp its motion, and the whole instrument was enclosed in a case with a glass cover, for protection from wind. The observations with such an instrument are similar to those with a mercury horizon, and, in spite of small, rapid vibrations, exhibit no difficulty after a little practice. This instrument was found to give satisfactory results, provided the machine was not accelerated, but suffered from the disadvantages of being heavy, difficult to move, and at times inconvenient for observation.

It was replaced by a bubble sextant, which is an attachment to the telescope of an ordinary sextant, containing a level bubble which is reflected into the field of view of the telescope. Any object which appears to be superposed on the centre of the bubble will actually be on the true horizon. The manipulation of this instrument is similar to that of a sextant: it is not affected by engine vibration, and wind does not usually offer any serious inconvenience. Accelerations of the motion of the aeroplane are not so conspicuous as with the artificial horizon, and with good piloting results of sufficient accuracy are easily obtained, by taking the mean of several consecutive readings.

Prof. Russell gives particulars of the reduction of the observations, which follows St. Hilaire's well-known method. He states that the average time required to secure a set of ten observations was 4·5 minutes, three minutes were required for computation, and two minutes for plotting the Sumner

line. As an example of the practical results, a summary of several sets of observations is given, which shows an average error of position of only about 10 miles; this accuracy is amply sufficient for the practical purposes of aerial navigation.

The following are some of the more important papers recently published:

LAU, H. E., Beobachtungen des Planeten Mars (4th paper), *Ast. Nach.*, **208**, No. 4983, 1919.

CHAPMAN, S., Theories of Magnetic Storms, *Observatory*, **42**, 196, 1919.

HALE, G. E., ELLERMAN, F., NICHOLSON, S. B., and JOY, A. H., The Magnetic Polarity of Sun-spots, *Astroph. Journ.*, **49**, 153, 1919.

FOWLER, A., and GREGORY, C. C. L., The Ultra-violet Band of Ammonia and its Occurrence in the Solar Spectrum, *Trans. R. S.*, Ser. A, **218**, 351, 1918.

CARTER, E., and KING, A. S., A Further Study of Metallic Spectra produced in High Vacua, *Astroph. Journ.*, **49**, 224, 1919.

LUNT, J., The Spectra of Nova Aquilæ No. 3: I, The Dark-line Spectrum, *M.N., R.A.S.*, **76**, 416, 1919.

SHAPLEY, H., Studies based on the Colours and Magnitudes in Stellar Clusters: XI, A Comparison of the Distances of Various Celestial Objects, *Astroph. Journ.*, **49**, 249, 1919.

SLIPHER, V. M., On the General Auroral Illumination of the Sky and the Wave-length of the Chief Aurora Line, *Astroph. Journ.*, **49**, 266, 1919.

GUTHNICK, P., Kunstliche Lichtkurven, *Ast. Nach.*, **209**, No. 4993, 1919.

SMART, W. M., The Position Line in Navigation, *M.N., R.A.S.*, **79**, 520, 1919.

METEOROLOGY. By E. V. NEWNHAM, B.Sc., Meteorological Office, London.

Note on Tornadoes, by J. Logie.—This paper was read before the Royal Meteorological Society on June 18, 1919, and will appear shortly in the Quarterly Journal of that Society.

The ordinary convectional theory attempts to explain the very low pressure near the centre of a tornado in the following way: Unequal heating of the ground causes a rising current of warm air which is maintained at a temperature higher than that of the surrounding air by means of the heat liberated by the condensation of water vapour which is required for the formation of the funnel cloud. Air moves in towards the centre along a spiral track, and in accordance with the principle of the conservation of angular momentum, acquires a very great rotational velocity as it nears the centre. There is a great reduction of pressure as a result of the centrifugal force of the revolving air. The author comes to the conclusion that the heat liberated by condensation of water vapour is not

sufficient to produce a tornado of such violence as actually occurs in nature, his argument being as follows:

$$\frac{dp}{dh} = -(g + A)D \quad . \; . \; .(1)$$

where p is the pressure at height h
g is the acceleration due to gravity
A is the vertical acceleration of the air
D is its density

is an equation which is true within the core of a tornado, as well as in the outside air. At the ground-level there is a difference of pressure between the core and the outside air, which decreases to zero at that height where the rotational movement comes to an end. It follows that the mean value of $\frac{dp}{dh}$ must be less inside the core than without. Now, the acceleration A would increase numerically the value of $\frac{dp}{dh}$ given by equation (1) if it were directed upwards, but, on the other hand, there cannot be an appreciable downward acceleration if the tornado is to persist for any length of time.

Taking, then, $\frac{dp}{dh} = -gD \; . \; . \; . (2)$ simply, and using the equation

$$p = R\,D\,T$$

where R is Charles' constant for air
and T is the temperature at any point

we get

$$\frac{dp}{pdh} = \frac{-g}{RT}$$

and therefore

$$\log \frac{B}{P} = \frac{gH}{RM} \; . \; . \; . (3)$$

where B = barometric pressure at the ground
P = barometric pressure at the top
H = height of the tornado
M = harmonic mean of T.

Outside the tornado the same equations apply, and the following relationship is arrived at:

$$\frac{\log\left(\frac{B}{P}\right)}{\log\left(\frac{C}{P}\right)} = \frac{N}{M}$$

where N is the harmonic mean temperature of the outside air, and C is the surface pressure.

Exact data are not available for the height to which a tornado extends, nor for the reduction of the surface pressure within the core. Six possible cases are considered—namely, where the reduction of pressure is 50 millibars, and the height of the disturbance is 5, 10, or 15 kilometres, and where the reduction is 250 millibars and the heights are the same.

Average conditions of temperature and pressure give for

1.	5 km. tornado	$C =$ 1008 mb.	$P =$ 534 mb.	$N =$ 270° A.
2.	10 km. tornado	$C =$ 1008 mb.	$P =$ 260 mb.	$N =$ 255° A.
3.	15 km. tornado	$C =$ 1008 mb.	$P =$ 125 mb.	$N =$ 230° A.

and using equation (4) the following figures are arrived at :

Height.	Tornado in which $B = C - 50$. $N - M$.	Tornado in which $B = C - 250$. $N - M$.
5 km.	23° A.	220° A.
10 km.	10° A.	68° A.
15 km.	5° A.	36° A.

These figures give the difference between the harmonic mean temperature inside and outside the core, but also give with sufficient accuracy the difference between the arithmetical means.

The lapse-rate for ascending saturated air is about 6° A. per kilometre less near the ground than it is for dry air, and the two become nearly equal at 10 kilometres height. The maximum difference of temperature that could be established in this way amounts to only about 30° A. Now $N—M$ exceeds this for all possible tornadoes of the class where $C - B = 250$ millibars, therefore these cannot be explained as due to saturated ascending air. The class in which the reduction of pressure at the surface is only 50 millibars is on a somewhat different footing, and might perhaps be explained in this way; but the margin is not so great as it appears to be at first sight, since the atmosphere in reality seldom, if ever, has the dry adiabatic lapse-rate from the ground up to a height of 5 kilometres or more, and a temperature difference of 30° A. could not be established in this way with a less rapid lapse-rate.

The author suggests, as an alternative theory for explaining the warmth within the core, that air occasionally has its electrical conductivity increased over a particular region,

either because it has rested for some time over ground of superior radio-activity, or because it contains an excess of oxides of nitrogen, or ozone. Should such air be drawn into an ordinary thunderstorm, it would offer an easy path for the discharge of lightning, and would have its conductivity still further increased thereby. A series of discharges might take place, and if an eddy were formed the heated air would not be so easily dissipated into the surrounding air as would happen in the case of an ordinary lightning flash. If the supply of electrical energy were great enough, a tornado might be formed in this way.

Wind- und Wasserhosen in Europa. Alfred Wegener (Vieweg & Sohn, Braunschweig, 1917).

This book deals at considerable length with European tornadoes ("grosstromben"); but whirlwinds in which there is not sufficient reduction of pressure for the formation of either a funnel cloud or a waterspout, and which are visible only as dust-whirls ("kleintromben"), are not included.

In the early part of the book there is a catalogue of 244 observations of tornadoes, and a full description of eleven of these is given. The author then passes on to a statistical discussion of the whole of the material that he has collected, and gives (Chapter IV) curves showing the monthly frequency of tornadoes in Europe (maximum in July) compared with those for America (maximum in May). The curves in both cases follow those for thunderstorm frequency very closely. The hourly frequency has its maximum between 16 and 17 hours in both Europe and America, the thunderstorm frequency again showing a close agreement. Davis has shown that in North America the tornado almost always occurs in the S.E. quadrant of a depression, but of the 49 European cases which admit of this kind of classification, 20 occur in the S.W. and 29 in the S.E. quadrant. Turning to the question of the direction of movement, in both countries this is most frequently from SW. or W.; in North America, 87 per cent. have been found to move from these points, for Europe the author obtains the lower figure of 52 per cent. The rate of travel is more often than not under 5 metres per second, but in a few instances amounts to as much as 20 metres per second. Both the length of track and the duration of the phenomenon show great

variations. There are very few cases where the total life of the storm amounts to an hour, or where the distance traversed is as much as 100 kilometres.

All the remaining part of the book, with the exception of the last chapter, which gives a brief summary of current theories regarding the origin of tornadoes, is devoted to a very detailed discussion of the appearance of the tornado cloud in all its stages, and of the wind and weather in its neighbourhood. Of especial interest is the discussion (p. 104–6) as to whether a tornado ever occurs without thunder and lightning. The evidence shows that this happens very seldom, if ever. The observations also suggest very strongly that the funnel cloud does not depend from the central region of the Cumulo-Nimbus cloud under which it forms, but from the margin. There are numerous cases where it forms between two neighbouring thunderstorms, but this is by no means a necessary condition for its formation. The surrounding winds are as a rule light, because the type of thunderstorm forming in still and sultry weather more often gives rise to a tornado than does the type which accompanies cyclonic depressions.

Investigations on Lightning Discharges.—The Sixth Annual Report of the University of Cambridge Solar Physics Observatory contains, under the section for Meteorological Physics, an account of important investigations on lightning discharges by C. T. R. Wilson. There is evidence in support of the theory that most positive discharges take place between the earth and the negatively-charged upper portions of a cloud, while a negative discharge is, as a rule, between the earth and the lower positively-charged part. The energy dissipated in an average flash is calculated to be between 10^9 and 10^{10} joules.

Rainfall in England: The True Long-Average as Deduced from Symmetry. By Alfred A. Barnes, A.M.I.C.E. (*Quart. Journal Royal Met. Soc.*, July 1919).

The Secular Variation of Rainfall.—By C. E. P. Brooks, M.Sc. (*Quart. Journal Royal Met. Soc.*, July 1919).

Two new cloud atlases have been published:

Le Nubi (Rome, 1917).—An Italian official publication.

Naval Meteorological Cloud Atlas.—Published and issued by the Hydrographer of the Navy, London, S.W.

The first of these is photographic, while the second is illustrated by oil paintings by G. A. Clarke, of Aberdeen.

PHYSICS. By James Rice, M.A., University, Liverpool.

Prof. Sir Ernest Rutherford has contributed four papers to the June *Phil. Mag.*, dealing with the collision between *α* particles and light atoms. These papers contain results for hydrogen, oxygen and nitrogen atoms, and in the case of the last, evidence is advanced in favour of the view that in certain cases disintegration of the nitrogen atom results from collision with *α* particles.

In the experiments the source of radiation was a small brass disc which had been exposed for a few hours, with an assisting electric field, to radium emanation in the usual manner. This renders the disc active, and twenty minutes after removal from the emanation, the *α* radiation arises entirely from radium C, and is homogeneous with a range of 7 cms. in air. Such an active disc was mounted in a tube which could be exhausted and filled with the gas under consideration. A small opening at one end was covered with a thin plate of a metal such as silver, aluminium, or iron. A zinc sulphide screen was mounted just outside this cover, about 1 mm. away, and viewed by a microscope, so that scintillations on the screen could be noted and counted in the usual way.

In order that none of the scintillations on the screen should be due to impact of the *α* particles themselves on the zinc sulphide crystals, it was necessary to ensure that the screen was beyond the range of these particles—*i.e.*, that the combined stopping power of the gas in the tube and the thin metal plate covering the opening should be at least equivalent to 7 cms. of air. Under these circumstances scintillations still exist, as was shown by Dr. Marsden, one of Prof. Rutherford's co-workers (*Phil. Mag.*, xxvii, 1914). These scintillations are due to the nuclei of light atoms set into swift motion by intimate collisions with *α* particles, which thus acquire velocities that carry them in some instances into impact with a luminescent screen placed far beyond the range of the *α* particles. Thus Marsden was able to detect scintillations presumably due to hydrogen nuclei (set in motion by the *α* particles from radium C) which had travelled over 100 cms. in that gas from the place of impact, a distance nearly four times as great as the range of *α* particles in hydrogen, viz. 28 cms. There was evidence also in Marsden's opinion that hydrogen nuclei are ejected by the radio-active

matter itself. Th departure of Dr. Marsden on active service interrupted the work; but Prof. Rutherford, in view of the importance of this latter result if substantiated, has contrived, despite considerable pressure of war work, to continue and extend the experiments in greater detail during the past five years. It is difficult to give a decisive answer on the possibility that hydrogen is a product of radio-active change, on account of the numerous factors involved. Firstly, it is difficult to be sure of the absence of hydrogen as a contamination in the source and absorbers of the radiation. Secondly, in experiments on impact between α particles and hydrogen atoms in the apparatus referred to above, the number of scintillations due to atomic nuclei set into swift motion by colliding α particles is much greater than is to be expected on simple theory. Thirdly, both nitrogen and oxygen atoms are also set in such swift motion by collision with α particles that they cause scintillations outside the range of the α particles. In fact, in Marsden's early experiments a large number of scintillations were probably not due to H atoms at all, but to high-velocity N and O atoms produced from the air between the source and the screen. It is on the latter two points that Rutherford concentrates in these four papers; for a satisfactory analysis of them is essential before any safe opinion can be formed on the radiation of H particles from radio-active matter.

At the time when Marsden was engaged on his experiments, Dr. C. Darwin, another worker in the Manchester laboratories, subjected the question of collision between α particles and atoms to mathematical analysis. Assuming that the forces involved arise from the charges carried by atomic nuclei which are to be regarded as points, and also assuming the inverse square law, Darwin was able to derive a formula for the number of swift velocity H atoms produced by a given stream of α particles with a given range, per cm. of their path. There will be, for instance, a certain maximum range for these H atoms, a range attained, doubtless, by such atoms as suffer a direct "head-on" impact from an α particle. For example, this is 28 cms. in air for H atoms set in motion by α particles with a range of 7 cms. in air (those from radium C). (If the α particles have a smaller range, the maximum range of the resulting swift H atoms is also smaller, but always about four times the range of the α particles). Extremely few of the H atoms will, however, be

set in motion by such a "head-on" collision. "Oblique impacts" will be much more numerous, leading to smaller resulting velocities, combined with angular deviation of direction from the path of the original α-particle stream. Darwin found that the number of H atoms with a range not less than $1/m$ th part of the maximum range bore to the number of α particles a ratio equal to

$$a(\sqrt{m} - 1) \qquad (1)$$

where a is a constant depending on the range of the α particles used. Thus for α particles with a range of 7 cms. $a = 1{\cdot}46 \times 10^{-6}$; for shorter-range α particles it is larger, varying in fact inversely as the 4/3 power of this range, so that, *e.g.*, for α particles with a range of 5 cms., $a = (7/5)^{4/3} \times 1{\cdot}46 \times 10^{-6}$.

The main result of Rutherford's work on hydrogen is to show that the number and distribution of the swift H atoms is entirely different from that suggested by the formula. According to it, the fraction of α particles giving rise to H atoms with ranges equal to or greater than R/m [R = maximum range] should decrease, as m decreases, to a zero value when $m = 1$ corresponding to the maximum range—a result which might be readily anticipated on general grounds. Instead, Rutherford found that there was no decrease in the observed fraction for ranges of H atoms between 9 and 19 cms.; thereafter there was a slow decrease for longer ranges, followed by a rapid fall to zero as the range approached the limit 28 cms. He was using α particles from radium C with a range of 7 cms., and the ranges above are estimated in air. Using α particles with a smaller range, the same general result was obtained—the observed fraction never decreased as rapidly with decreasing m (*i.e.* increasing range) as the law indicated, using α particles with ranges above 3 cms.

Even when the law in (1) is fairly well obeyed, as is the case when α particles with ranges under 3 cms. are used, the observed number is greater than the calculated—*i.e.*, the observed constant, a, in (1) is greater than the calculated constant, viz. $(7/3)^{4/3} \times 1{\cdot}46 \times 10^{-6}$, etc.

Rutherford concludes that the assumptions at the base of Darwin's analysis cannot be justified. He advances calculations to show that in these phenomena the nuclei of the α particles and the H atoms must in some cases approach as near as about

3×10^{-13} cm. Physicists have on occasion assumed the inverse square law to hold down to distances of the order 3×10^{-12} cm., without reaching conclusions discrepant with observation; but this distance of approach is ten times as small, and at such a distance—comparable with the dimensions of the electron itself—assumptions as to point charges and inverse square law must become very doubtful. Rutherford points out that at such a close distance an α particle composed of four hydrogen nuclei and two electrons would exert a very complicated field of force with rapid variations in direction and magnitude ; further, that the very considerable forces involved between two positive nuclei at such a distance—about 5 kilograms weight on the inverse square law—must distort not only the structure of the nucleus itself, but even of the electron. It is probably this distortion, and the consequent alteration in the magnitude and direction of the forces involved, which are responsible for the fact that a greater proportion of the collisions are of the " head-on " or nearly " head-on " type than could be anticipated on the simple theory of point-charges.

The second of the papers deals with an investigation by the usual magnetic and electrostatic deflexion methods of the velocity and e/m ratio of the swift particles in these experiments, yielding support to the assumption that they are H nuclei. The third deals with experiments on collision between α particles and atoms of nitrogen and oxygen. Theoretically it is to be expected that all atoms of weight not greater than that of oxygen, and *carrying unit electronic charge*, might be detected in such experiments beyond the range of the impinging α particles ; thus N atoms could have a maximum range 1·33 times as great, and O atoms 1·12 times as great, as the range of the α particles. Preliminary experiments have been made with other atoms, but they are difficult to carry out, and the results rather inconclusive ; but for elements in the gaseous state, such as N and O, conditions are much better. Briefly, Rutherford finds evidence for the existence of swift N and O particles in air, nitrogen, oxygen, and carbon dioxide, with maximum range about 9 cms. in air. As in the case of H atoms, the observed numbers for any given fraction of the range and over are larger than the numbers calculated on simple theory, and a similar explanation must hold here. It is in the fourth paper that Prof. Rutherford really deals with a matter of supreme importance—viz., the

possibility that in some of these intense and intimate collisions the α particles disintegrate the N atom, causing an ejection from it of an H atom or an atom of mass 2. The evidence is briefly this: Using the apparatus already referred to, with the air exhausted from the box, and absorbing foils of aluminium being employed to stop the various flying particles instead of air, one finds scintillations far beyond the range of the α particles. The swift atoms causing these scintillations carry a positive charge, are deflected in a magnetic field, and have about the same range and energy as the swift H atoms produced by the passage of α particles through hydrogen as already described. These "natural" scintillations are believed, as Marsden suggested, to be due to swift H atoms from the radioactive source, but it is difficult to decide whether they are expelled from the radio-active source itself, or are due to the action of α particles on occluded hydrogen. If dried oxygen or carbon dioxide is admitted into the vessel, there is a diminution in the number of scintillations of an amount to be expected from the stopping power of the column of gas. When dry air is admitted, however, the surprising effect is observed of an increase in the number of scintillations; in fact, at a distance from the source equivalent to 19 cms. of air the number is about doubled. As already pointed out, in air swift N and O particles arise with a possible range of 9 cms.; but the increased number of scintillations referred to above is observed at much greater distances than 9 cms., and cannot be ascribed to that cause. A number of control experiments were made ruling out the possibility of water vapour, or even the minute traces of hydrogen in the air, as being the source. Pure dry nitrogen, when introduced into the vessel instead of the dry air, still further increased the number of scintillations, as was to be expected if they are due to nitrogen. These long-range scintillations appear to be due to the collision of α particles with nitrogen atoms, the particle causing scintillation being a hydrogen nucleus or atom of mass 2 expelled from the N atom. Such a revolutionary conclusion, of course, will require careful investigation, and further experiments are suggested to test by various methods whether these particles are really H atoms, and are derived from the N atoms. One anomaly in the third paper would receive an explanation if this were so. In the case of swift N and O particles, they appear to have a common maximum range of

9 cms., while in theory the lighter N atom ought to have a range about 19 per cent. greater than the O atom. This would be accounted for if the energy of impact was shared between an H atom and the remaining part of the disrupted N atom.

No doubt the conclusion of this paper will be subjected to searching criticism before long—there are many obvious gaps in the experimental work to be filled. It is, however, a reasonable assumption that Prof. Rutherford will, by dint of his own energy and the enthusiasm he evokes among his pupils, soon be engaged in filling the lacunæ. His imagination has already suggested the feasibility of searching for a still more intense projectile than the *α* particle from radium C, wherewith to disintegrate the structure of other light atoms.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Latent Heat of Fusion.—The process of fusion of a solid has been investigated by Honda (*Science Reports*, Tohoku University, September 1918) from the point of view of the quantum hypothesis. The chief interest of the work lies in the view taken of the nature of the molecular and atomic movement in the liquid or fused state of a substance. Our long familiarity with the principle of the continuity of the gaseous and liquid states, and the important results gained in this field, have served to impress physicists and chemists with the close analogy which exists between a liquid and a gas. On the basis of Honda's considerations, we are led to expect a far closer analogy between a solid and a liquid, in so far as the type of molecular or atomic energy is concerned. Let us take the simple case of a monatomic solid. Its energy consists of vibration of the atoms. On the equipartition view this energy amounts to 3 RT per gram-atom, of which one-half is kinetic, one-half potential energy. On the quantum theory this expression is modified in the sense that T itself is replaced by a complex expression (involving T), which allows for the fact that the heat capacity diminishes as the temperature falls. In both cases we restrict ourselves to vibrational movement of the atoms alone; we do not consider any other possible mode of motion. According to Honda, however, the molecules or atoms may possess small dependent *rotational* vibrations about their centres of mass, and when we approach the

melting-point of the substance the amplitude of the vibration attains such a magnitude that the rotational vibration "at last becomes a continuous rotation." "As soon as the rotational vibration is changed into a continuous rotation the regular spacing of the molecules breaks down, disorder beginning to take place, and therefore the rotation becomes independent of the translation." The molecules have now two or three degrees of freedom in respect of rotation. "Since the molecular distances in the solid and liquid states do not differ much, the rectilinear motion of the liquid molecules is also vibratory, its amplitude remaining the same." The latent heat of fusion is therefore identified with the bringing into existence of the two or three degrees of freedom of rotation. Honda calculates what this energy amounts to on the basis of the quantum theory, and then proceeds to compare the result with the observed latent heats of fusion. For sixteen elements he finds very satisfactory agreement. In six other cases there is no agreement, and this he explains on the basis of the external and internal work done owing to the change in volume on melting. Honda has also applied his ideas to the fusion of compounds, and finds in certain cases that a similar relation holds good. "It is necessary to assume that these compounds have a molecular structure both in the liquid and the solid, and that in the liquid state the molecules have three degrees of freedom for rotation." In other compounds it is necessary to assume a number from 3 to 6 for the degrees of freedom in order to bring the calculated and observed latent heats into agreement. It is evident that Honda's views are not as applicable to compounds as they are to elements. Nevertheless, the importance of the contribution as pointing the way to an explanation of the mechanism underlying the process of fusion is very considerable.

Oxonium Compounds.—A further addition has been made to our knowledge of addition compounds between organic acids, phenols, and inorganic acids, by Knox and Richards (*Journ. Chem. Soc.*, **115**, 508, 1919), the method employed being that depending upon the solubility of the organic substance in solutions of hydrochloric, nitric, sulphuric, and in some cases formic and acetic acids. Of the mineral acids, nitric acid shows the greatest, and sulphuric acid the least, tendency to form compounds with organic acids and phenols.

Typical mono-, di- and tri-basic acids were investigated, as well as typical monohydric, dihydric, and trihydric phenols. The results are most conveniently shown by means of a graph in which the abscissæ denote the normality of the " solvent " acid (HCl etc.), the ordinates denoting the corresponding solubility (expressed in normality) of the dissolved acid or phenol. The curves obtained are of two main types according as the " solvent " acid is a mineral acid or an organic acid, but in each case the assumption of oxonium salt formation is sufficient to account for the observed results. In the first type—*e.g.*, phenyl acetic acid in HCl solution—the solubility of the organic acid diminishes rapidly at first, reaches a minimum, and afterwards increases steadily with increasing concentration of the solvent acid. The decrease in solubility is naturally ascribed to the mass action effect brought about by the presence of the common ion, the hydrogen ion. At the same time the oxonium salt is formed, and its greater solubility eventually more than compensates for the common ion effect, with the result that the solubility curve turns upward. When the " solvent " acid is an organic one, a different type of curve is obtained. Two factors have to be taken into account: (1) the weakness of the organic acids in general; and, (2) the wide difference between the solubilities of the " dissolved " acid in the water and the organic acid " solvent." As regards the first factor—since both the " solvent " acid and the " solute " acids are weak, the diminution in solubility, due to the common hydrogen ion, is too small to be detected, except in the case of oxalic acid, which exhibits a slight initial diminution in solubility. The other " solute " acids investigated show an increase in solubility from the beginning, except tartaric acid dissolved in acetic acid solution, and oxalic acid in lactic acid solution, in which cases no evidence of union was obtained. As regards the second factor—if the dissolved acid is more readily soluble in the " solvent " organic acid than in water—the resulting curve shows a steady rise, and no definite conclusion can be drawn from it. This is realised, for example, in the case of suberic acid in acetic acid solution. Other cases occur in which the solubility of the dissolved acid rises, passes through a maximum, and then falls. This is clear evidence of oxonium salt formation, the initial increase in solubility being due to the formation of the salt, which after-

wards becomes less soluble in higher concentrations of the "solvent" acid. This behaviour is shown, for example, by succinic acid in acetic or in formic acid, and by mandelic acid in the same "solvent" acids.

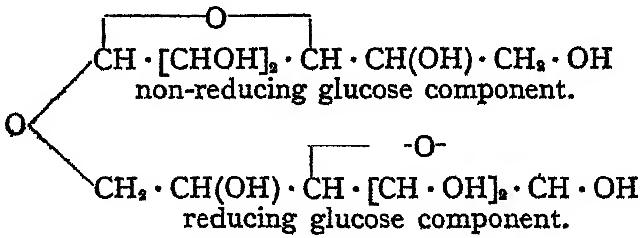
In view of these results and numerous others obtained by different investigators—cited fully in the introduction to Knox's paper—the generality of such oxonium compound formation can be regarded as demonstrated very satisfactorily.

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School, London.

With the object of establishing the constitutional formula of maltose, Irvine and Dick (*Jour. Chem. Soc.*, 1919, **115**, 593) subjected this compound to complete methylation followed by hydrolysis, intending to carry out the following series of reactions:

Maltose → methylmaltoside → heptamethylmaltoside,

which latter compound should yield trimethylglucose and tetramethylglucose. They have been able to confirm the earlier observation of Purdie and Irvine (*Jour. Chem. Soc.*, 1905, **87**, 1022) that the non-reducing glucose component of maltose possesses the butylene oxide structure by obtaining from it crystalline tetramethylglucose, but have not succeeded in isolating the trimethylglucose, which should form the other product of hydrolysis, as shown by the formula—



In place of the trimethylglucose, however, they obtained a pentose as the result of an entirely unexpected degradation occurring during the decomposition of maltose octa-acetate by means of hydrogen iodide; the resulting substance proved to be a C_{11} sugar methylglucoarabinoside, since it gave on hydrolysis crystalline tetramethylglucose and a dimethylarabinose. This observation opens the way to a new method of studying

the constitution of carbohydrates, by degrading them in acid solution and comparing the results with those already obtained by regulated oxidation in alkaline solution.

In a paper dealing with the resolution of Hyoscine and its Components, King (*Jour. Chem. Soc.*, 1919, **115**, 476) describes the isolation for the first time of *d*-hyoscine hydrobromide by fractional crystallisation of a quantity of crystalline hydrobromides of feeble lævorotatory power obtained as a by-product in the manufacture of the therapeutically valuable *l*-hyoscine. The hydrolysis with boiling 10 per cent. hydrobromic acid of *l*-hyoscine resulted in the formation of *l*-tropic acid and inactive oscine, which latter has been resolved into dextro- and lævo-components by means of *d*-tartaric acid. From these facts it is concluded that *l*-hyoscine is a molecular combination of *l*-tropyl *d*-oscine and *l*-tropyl *l*-oscine, while *d*-hyoscine is a similar combination of *d*-tropyl *d*-oscine with *d*-tropyl *l*-oscine.

The tar obtained by distilling animal matter contains aniline pyridine and quinoline which bear no relation to the amino-acids of proteins. With the object of determining whether these bodies are secondary products arising from pyrogenic transformations, or whether they form hitherto unrecognised constituents of the protein molecule, Pictet and Cramer (*Helv. Chim. Acta*, 1919, **2**, 188) have distilled 4 kilos of egg albumen under a pressure of 20–22 mm. The products of distillation can be differentiated into acid basic and neutral fractions. From the basic fraction the authors have isolated a primary amine of the formula C_6H_9N, which appears to be a dihydro-aniline:

$$CH \begin{matrix} \diagup CH - CH_2 \diagdown \\ \diagdown CH = CH \diagup \end{matrix} CH \cdot NH_2.$$

Among the neutral substances was found iso-hexoamide $(CH_3)_2\,CH \cdot CH_2CH_2CO\,NH_2$, which is, of course, closely related to isohexoic acid, one of the chief constituents of animal oil.

In the last number of this Journal (SCIENCE PROGRESS, 1919, **53**, 35) it was shown that the constitution of Turkish and Chinese tannins has been so far established by Fischer as to show that they are probably amorphous mixtures of closely related polygalloyl glucoses. Hamameli-tannin, on the other hand, being crystalline, is probably an individual substance, and for this reason it has been specially studied by Freuden-

berg (*Berichte,* 1919, **52**, [B], 177); on hydrolysis, by means of tannase, of a dilute aqueous solution covered with toluol, this author obtained gallic acid and a sugar (calculated as a hexose) in quantities corresponding with the formula of a digalloyl hexose.

The shortage of fats in Germany during the war necessitated the production of glycerol from other sources, and during the summer of 1917 the Germans were producing glycerol in large quantities by fermentation. At the instigation of the American Government, experiments were undertaken with a view to discovering some similar method for use by the Allies, and in three months' time, Messrs. Eoff, Linder and Beyer announced the production of glycerol in 20–25 per cent. yield by the addition of 5 per cent. of sodium carbonate (in solid form) to a 17–20 per cent. solution of sugar set in active fermentation by *Saccharomyces ellipsoideus.* On adding the sodium carbonate, a copious precipitate is formed and the evolution of gas ceases, while the yeast becomes dormant. After a while the precipitate disappears and the fermentation starts again. The formation of the precipitate and the dormant period are essential to the success of the whole process; it has also been observed that the addition of ammonium chloride to the fermenting liquid increases the yield of glycerol; the most favourable fermentation temperature is from 30–32° C. Trial runs on a commercial scale were carried out on molasses, but experiments have also been made with cane-sugar and starch glucose; in these cases, however, yeast food had to be added in such quantities as to increase the difficulty of purifying the glycerol. A fuller account of this work may be found in an article by Ling entitled "Production of Glycerin from Molasses" (*Jour. Soc. Chem. Ind.*, 1919, **38**, 175R.). More recently Schweizer (*Helv. Chim. Acta,* 1919, **2**, 167) has published the results of his experiments on the production of glycerol by fermentation. Starting from the assumption that fermentation glycerol was produced by the reduction of glyceric aldehyde or dihydroxyacetone, he has tried the effect of adding various reducing agents to the fermenting liquid; acid reducing agents were found to be deleterious for the yeast, but eventually sodium sulphite was found to produce an increased yield of glycerol; by the addition of 7–8 per cent. of this salt to a 10 per cent. solution of fermenting sugar he was able to

obtain a 21·3 per cent. yield of glycerol in 24 hours. Supporting the reduction theory, it was found that a poorer yield was obtained if the solution was thoroughly aerated. It should be stated in conclusion that the amount of glycerol obtained from 100 grams of sugar by Pasteur in 1857 was 3·6 grams, while Oppenheimer in 1914 obtained, by means of yeast juice, quantities varying from 3–12 grams.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

Geological Processes.—In a memoir entitled "Subsidence and Reef-encircled Islands," Prof. W. M. Davis brings forward several independent verifications of Darwin's theory of coral-reefs as upgrowths upon intermittently-subsiding foundations (*Bull. Geol. Soc. Amer.* 1918, **29**, 489–574). These proofs are: the embayed shore-lines of reef-encircled islands; the slope of reef-foundations in relation to the physiographic evolution of the islands; the unconformable contact of elevated reefs with their foundations; unconformable fringing-reefs at sea-level; the peculiar distribution of submarine banks in coral seas; the disappearance of great volumes of detritus from reef-encircled islands; the absence of reefs from emergent shore-lines; the unequal depths of lagoons and banks; and the forms of spur-ends on islands within barrier-reefs. In Prof. Davis's opinion these facts contradict all still-standing theories, and give a decisive superiority to Darwin's.

The article by the same author in Science Progress (1919, **13**, 420–44) on the geological aspects of the coral-reef problem, chiefly directs attention to the unconformable contact of reef limestones with their foundations. The evidence thus provided of erosion and subsidence prior to reef formation is strongly in favour of Darwin's theory.

Foye's observations on Fiji (*op. cit. infra*) lend support to this view. The earth's crust in this region has been very unstable, and each period of subsidence has been accompanied by reef formation. The elevated reefs rest unconformably upon eroded volcanic foundations.

Molengraaf has made a notable suggestion to explain the subsidence of volcanic islands around which reefs have grown up ("The Coral Reef Problem and Isostasy," *Proc. Akad. Vet. Amsterdam*, 1917, **19**, 610–27). He points out that volcanic

islands are not isostatically compensated, and invariably show a positive anomaly of gravity. Hence their bases must yield, and they sink slowly down into the crust under the influence of gravity. This suggestion of local subsidence is regarded by Davis as an important contribution to the coral-reef problem (" Isostatic Subsidence of Coral Islands," *Proc. Nat. Acad. Sci.* 1917, **3**, 649–54); but the phenomena of uplifted reefs and atolls show that other processes than isostatic subsidence are in operation.

The recent diastrophism of North-East America excellently illustrates the theory of isostasy (" Post-glacial Uplift of North-eastern America," H. L. Fairchild, *Bull. Geol. Soc. Amer.* 1918, **29**, 187–238. See also " Glacial Depression and Post-Glacial Uplift of North-Eastern America," *Proc. Nat. Acad. Sci.* 1918, **4**, 229–32). The amount and area of land depression beneath the latest continental ice-sheet (the Labrador glacier) is fairly proportionate to the thickness and extent of the ice. When the ice melted the land was much below its present altitude, which it has since attained by a tilting movement with the hinge south of New York. Ancient estuary features are traceable to a height of 800 feet in Vermont, and it has been found possible to map the isobases over a considerable part of the area.

Regional and Stratigraphical Geology.—J. F. N. Green advances reasons for believing that the red Mell Fell Conglomerate of the Lake District, hitherto regarded as the basal division of the Carboniferous, is really of Devonian age (*Proc. Geol. Assoc.* 1918, **29**, pt. 3, 117–25). Its characters are those of a torrential deposit formed at the foot of a mountain range in an arid climate; whereas the Carboniferous Limestone indicates deposition in a sea bordering a low-lying land whence little clastic detritus was derived.

In a study of the structural relations of the Skiddaw granite, J. F. N. Green presents evidence suggesting that the intrusion is earlier than the cleavage, the Devonian faulting, and the minor folds of the surrounding rocks (*Proc. Geol. Assoc.* 1918, **29**, pt. 3, 126–36). It is therefore believed to be connected with local folding towards the close of the Borrowdale volcanic episode.

O. Holtedahl, Bidrag til Finmarkens Geologi (*Norges Geol. Unders.* 1918, No. **84**, 314 pp.). This is a full account

of the geology of Finmark, the most northern province of Norway. There is a short résumé in English.

Numerous important papers on the stratigraphy of various areas in the western United States have appeared recently (see list in Bibliography at end of article), but as the writer has not seen them it is not possible to offer any comment. The reader is referred to a review by C. Schuchert (*Amer. Journ. Sci.* 1919, **47**, 74–9), for a discussion of the salient points in this new work.

W. G. Foye has recently made an extensive geological study of the Fiji Islands, the work embracing both general geology and coral-reef problems. The larger islands of Viti Levu and Vanua Levu have central cores of deeply eroded plutonic rocks, which are believed to be the remnants of an ancient continental mass. There have been four periods of volcanic activity in the islands, with the eruption first of rhyolites, then two series of andesites, and finally basalts. Two series of sedimentary rocks occur. One, early in the volcanic period, is believed to be of Miocene age. Later coralliferous limestones and marls are probably post-Tertiary, and are intruded by the latest basalts (" Geological Observations in Fiji: Part I., Geological History of Fiji," *Proc. Amer. Acad. Arts & Sci.* 1918, **54**, 1–95).

Petrology of Igneous Rocks.—In a paper entitled " A Type of Igneous Differentiation," F. F. Grout discusses the rocks of the great Duluth gabbro mass, probably the largest mass of basic igneous rock known (*Journ. Geol.* 1918, **26**, 626–58). The rock types fall into two groups, one related to the gabbros, the other to the granites, intermediate types being rare. The sharpness of the separation between the two groups is shown by means of variation diagrams. It is believed that none of the theories of crystallisation-differentiation can fully explain this separation, since they all necessitate the presence of a relatively large amount of intermediate rock. The field and laboratory studies strongly suggest an immiscible separation of the " red rock " (granophyre) from the gabbro. The variations within the gabbro itself, from magnetite-gabbro to anorthosite, are believed to be due to differentiation by crystallisation during convection, aided by a slight amount of settling of crystals.

Many problems in the crystallisation of silicate melts are

illustrated on a large scale in glass-making processes. N. L. Bowen ("The Significance of Glass-making Processes to the Petrologist," *Journ. Wash. Acad. Sci.* 1918, **7**, 88–93) shows, for example, that immiscibility between silicate and silicate does not occur, at least in glass melts. Two layers may separate in the glass pot, but their persistence is entirely due to slowness of diffusion, and the stratification may be eliminated by stirring. Glass often shows a density and composition gradient (illustrated by the famous Morozewicz experiment), but this is shown to be due to differential melting, with the sinking of the heavier and more easily fusible constituents at an early stage, and the rising of the difficultly-fusible silica grains at a later stage.

An important paper by W. J. Miller (*Bull. Geol. Soc. Amer.* 1918, **29**, 399–462) gives the results of an elaborate study of the great anorthosite mass of the Adirondacks, which figured largely in Bowen's recent work on the problem of the anorthosites. Although, like Bowen, Miller believes that the anorthosite originated by the settling of mafic minerals in a gabbro magma, his view differs from Bowen's in several particulars. The anorthosite did not arise by the subsequent settling of labradorite crystals; furthermore, the associated syenite-granite masses are intrusive into, and distinctly later than, the anorthosite, and hence are not direct differentiates as advocated by Bowen. Miller shows, moreover, that the anorthosite was at one time effectively molten, and not merely a mass of precipitated plagioclase crystals. The term *silexite* is proposed by Miller ("Pegmatite, Silexite, and Aplite of Northern New York," *Journ. Geol.* 1919, **27**, 28–54), for the bodies of pure, or nearly pure, silica of igneous or aqueo-igneous origin, occurring as dykes or segregations within or without their parent rock. The silexite of the area dealt with occurs in a granite, and represents a very siliceous facies of pegmatite, into which there is a complete transition. Both silexite and pegmatite are believed to be segregations which developed while the granite still preserved a considerable degree of fluidity, and continued to form until the granite almost or completely solidified.

In his recent work on the geology of Fiji (*op. cit. supra*, pp. 97–145), Foye gives brief petrographical descriptions, accompanied by chemical analyses and quantitative mineral deter-

minations, of tonalite and dioritic gabbro from the crystalline basement of Viti Levu, and of numerous lavas from the whole archipelago. These include various types of basalt, andesite, and rhyolite. Foye remarks the association of hypersthene and hornblende in the lavas of Vanua Levu, and suggests that these minerals are due to the retention of magmatic water in submarine flows owing to the rapid formation of a flexible impervious skin. Monoclinic pyroxene, on the other hand, should be formed at open vents, in which there is a free escape of magmatic gases.

A short paper by Friedlander (" Gesteine von Sakurashima," *Zeitsch. f. Vulc.* 1918, **4**, 202–3) is notable for six new chemical analyses of lavas and ashes from the recent eruption of Sakurajima, Japan.

Origin of Sedimentary Rocks.—The great work by L. Cayeux, " Introduction a l'Étude Pétrographique des Roches Sédimentaires " (*Mém. Carte Géol. de France*, 1916, pp. 524, and Atlas of 56 plates) will prove of the utmost value to petrographers. It is divided into three parts : 1. Methods of analysis of sedimentary rocks, with special consideration of microchemical methods ; 2. Diagnostic of the constituents of sedimentary rocks ; 3. Study of the microstructure of organisms considered as constituent elements of sedimentary rocks. This last is done in great detail, and illustrated by many magnificent plates.

Staurolite is the principal mineral of heavy residues from the Bunter Pebble Beds of Nottinghamshire, and T. H. Burton (*Quart. Journ. Geol. Soc.* 1918, **73**, pt. 4, 328–39) therefore believes that Scotland supplied the bulk of the heavy constituents, a north-eastern river and its tributaries being postulated as the means of transport.

W. H. Bucher brings forward evidence to support the view that oolites and spherulites are due to the change of at least one constituent from the emulsoid to a solid condition (*Journ. Geol.* 1918, **26**, 593–609). The difference between radial and concentric structures depends on the amount of other substances simultaneously precipitated, and mechanically entangled in the growing concretion. If the substance is relatively pure the structure is predominantly radial (spherulitic). If other substances are precipitated in quantity the structure tends to be concentric (oolitic). These views are based on the important work of Schade relative to the origin

of urinary calculi and the formation of concrements, and are applied in a brief survey of the literature of oolitic and spherulitic structures.

A similar view is adopted by W. A. Tarr in discussing the origin of siliceous oolites in some Triassic shales of Wyoming (*Bull. Geol. Soc. Amer.* 1918, **29**, 587–600). He regards the oolites as due to direct precipitation of colloidal silica in shallow, agitated, muddy water. The precipitation is ascribed to the electrolytic and saline character of the water, the resulting siliceous gel aggregating into oolites which were buried as the silts and sands accumulated.

Since De Geer's classic work in Sweden, the problem of rhythmic banding in the clays associated with glacial deposits, and its bearing on geochronology, has been much discussed. R. W. Sayles has recently made a general survey of the phenomenon in especial connection with the banded glacial clays of the Connecticut Valley and Rhode Island ("Seasonal Deposition in Aqueo-Glacial Sediments," *Mem. Mus. Comp. Zool., Harvard,* 1919, **47**, No. 1, pp. 67, 16 plates). As a result of this study the theory that each pair of alternately coarser and finer sediments represents the seasonal deposits of a year, is placed in a strong position. In the Boston region there is a formation of tillites and associated slates of Permian age. The slates show fine seasonal banding, and offer extraordinary resemblances to selected specimens of the Pleistocene banded clays, as is illustrated in several magnificent plates. Even the crumpling of layers by the grounding of icebergs, or the re-advance of an ice-sheet, is duplicated in the older formation. These phenomena are shown to occur in the argillaceous sediments accompanying ancient tillites in many parts of the world.

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BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., University College, London.

Genetics.—Variation as exhibited by the vegetatively-produced offspring of *Lemna minor* has been studied in an extensive series of cultures by N. B. Mendiola (*Genetics*, March). The range of area of the frond in 200 wild individuals which had attained maturity was from 2·26–6·45 sq. mm., with 4·019 ± ·037 as the mean. Variation was found to occur both as to the shape of the fronds and their speed of formation; a variation that was most marked under conditions favourable to growth. Races were isolated from the wild population which differed in these respects, and thus supported the view that pure lines were involved.

Sex segregation in the Bryophyta (*Jour. of Genetics*) is the subject of a paper by Collins, who reviews briefly the work of El. and Em. Marchal. These authors found that the spores of diœcious forms were unisexual, the two sexes being produced in approximately equal numbers. Protonemata produced from the vegetative axes were of the same sex as the plant from which they were derived, but those from the sporophytic tissue produced a low proportion of hermaphrodite axes.

Collins, in his experiments, has produced protonemata from antheridia, from perigonial leaves, and from spores. The last-named produced sporogonia, but the two former were entirely male. The author suggests a somatic segregation in the haploid tissue as a possible explanation.

In the same journal, Bateson and Sutton describe inheritance experiments on *Begonia* in relation to doubleness and sex. Single was found to be dominant, but the proportion of doubles in F_2 exhibited great fluctuation. A wild single species,

B. Davisii, when "selfed" yields singles only, but when the pollen of this species is employed for crossing with doubles, all the offspring have double flowers. The conclusion seems justified that the male flower alone carries the double character.

Ecology.—The ecology of tropical rain forest forms the subject of a paper by R. C. McLean in the current number of the *Journal of Ecology*. The author finds that, as might be expected from corresponding data from temperate woodlands, the air among the bushes of the undergrowth exhibits considerable stagnancy with an almost uniform humidity for periods of several hours, or even days. Here, too, the temperature was found to be very uniform with an average of about 20° C. Experiments with detached leaves of several different species showed a maximum transpiration of only 0·4 of the evaporation from a free water surface; estimations of the air-space in leaves of the shade plants showed this to be greater, per unit of weight, than in the sun leaves; also the number of stomata per unit area was smaller in the former than in the latter. The shade species, however, showed the higher mineral content. The author concludes from these results that the absorption of mineral salts is independent of the low foliar evaporation, and suggests that a slow water-current may be maintained by root-pressure accompanied by liquid excretion.

J. Gray and G. J. Peirce (*American Jour. Bot.*), having studied the stomatal reactions of Barley, Wheat, Oats, and Rye, by direct observation, conclude that the main factor determining the opening and closure of the stomata is illumination, though a minimum turgor is requisite for their action The observations were made on living leaves still attached to the plant, and the measurements showed that the maximum aperture was attained under maximum illumination irrespective of the high or low humidity.

R. B. Harvey, in a short but interesting note (*Bot. Gaz.*), describes the result of experiments which showed that ice formation within the tissue of a leaf is considerably retarded by the presence of wax or a dense covering of hairs.

Having regard to the important rôle played by Algæ in colonising bare, exposed surfaces, their capacity for resisting drought is an important feature. Miss Bristol has in this connexion made cultures of soil collected at Rothamsted

between 1846 and 1893. Two species, viz. *Nostoc muscorum* and *Nodularia Harveyana*, resumed their activity after a period of seventy years. Other highly resistant species belonged to the genera *Anabæna*, *Cylindrospermum*, *Chlorococcum*, *Trochiscia*, *Stichococcus*, *Nitzschia*, *Plectonema*, *Hapalosiphon*, and *Phormidium*. The water content of the soil appears to affect the retention of vitality, the greatest number of species being obtained from a sample with 12 per cent. of water.

Anatomy.—The anatomy of the stem in the *Berberidaceæ* receives treatment at the hands of Prof. Harvey-Gibson and Miss Horsman (*Trans. Roy. Soc., Edinburgh*). These authors record the occurrence of two rings of vascular bundles in the overground stem of *Epimedium alpinum* and of irregularly scattered bundles in *Achlys triphylla*, *Podophyllum* spp., and *Diphylleia cymosa*. Except for the presence of a cambium, the vascular anatomy closely resembles that of Monocotyledonous stems. In *Akebia* there is a well-marked endodermis.

C. E. Quinlan, writing in the same journal, treats of the anatomy of the Calycanthaceæ. All the species examined showed pairs of basipetal axillary buds, and four inversely orientated bundles in the pericycle. Several species exhibited thyloses and also lateral sieve plates.

Morphology.—M. Tison has recently described (*Rev. gén. d. Bot.*) the remarkable suspensor of *Trapa natans*. This is a robust structure which assists in the nutrition of the embryo and is furnished with a well-developed, tongue-like haustorium.

Taxonomy.—E. G. Baker (*Jour. Bot.*, June and July) has recently furnished a clavis to the African species of *Allophylus* (*Sapindaceæ*), and also descriptions of fourteen additions to the genus. In the same journal Mr. A. More describes a new species of *Phyllanthus*.

Mr. Pugsley, in the *Journal of the Linnean Society* for May, contributes an exhaustive revision of the genera *Fumaria* and *Rupicapnos*, including diagnoses of six additions to the former and seven to the latter, together with several new varieties. The same author, in an article on the British Euphrasias (*Jour. Bot.*), describes a new species from Exmoor under the name *E. confusa*. This is closely allied to *E. gracilis* and *E. nemorosa*, but is distinguished by its yellow flowers and broad emarginate capsules.

The hybrid between *Papaver rhœas* and *P. dubium* is de-

scribed by C. E. Salmon in the new *Phytologist* (Mar. and April). It has the elongated capsules of *P. dubium*, but the spreading peduncular hairs of *P. rhœas*. In the same journal N. Carter describes a new species of *Trachelomonas*.

PLANT PHYSIOLOGY. By WALTER STILES, M.A., University College, London. (Plant Physiology Committee.)

Absorption of Water and Dissolved Substances.—A view widely held in some quarters is that absorption of water and dissolved substances by the root is limited to the root-hair layer which extends for some distance from a point a little behind the root tip away from the latter. H. Coupin (*Comptes rendus*, **168**, 519–22, 1919) has set himself to combat this opinion, by growing seedlings in water-culture so that only 2 or 3 millimetres of the tip of the main root are in contact with water. Under such conditions the seedlings develop well, and as all the root system except the tip is merely in moist air from which absorption of water is very slight, and absorption of salts non-existent, the absorption of water and dissolved substances through the root tip must take place perfectly readily.

In the same journal (*Comptes rendus*, **168**, 467–70, 1919) are recorded the results of some interesting experiments by D. Cebrian de Besteiro and M. Michel-Durand on the effect of light on the absorption of organic matter from Knop's solution containing 0·4 per cent. of sugar. Four different light intensities were employed, ranging from full sunlight to one-ninth sunlight. It was found that increasing the light intensity also increased the quantity of sugar absorbed ; thus the plants in full sunlight absorbed five times as much sugar as the plants exposed to one-ninth sunlight. The hypothesis that, in dull light, plants are better able to utilise root-absorption of organic matter is thus disproved.

As regards the absorption of inorganic salts, R. B. Harvey and R. H. True (*Amer. Journ. Bot.*, **5**, 516–21, 1918) have made some observations on seedlings of squash, pea-nut, soy-bean, and sweet-corn growing in solutions of nutrient salts. The concentration of the solution was determined at a number of different times by measuring its electrical conductivity. If the solution is undisturbed, the salts in the culture solution are absorbed by the plant until an equilibrium concentration

is reached. The authors reached the important conclusion that for any particular species this concentration is independent of the nutrient salt used, as well as of the initial concentration and the volume of the solution employed, provided the concentration is not too great to be toxic nor so low as to be insufficient for the requirements of the plants for growth. The concentration reached at equilibrium is, on the other hand, specific for each plant. These conclusions are in rather sharp contrast to those of W. Stiles and F. Kidd (*Proc. Roy. Soc.*, *B*, **90**, 448–70, 1919) in regard to the absorption of inorganic salts by a storage tissue—namely, carrot root. In this case the initial concentration has a great influence on the equilibrium concentration reached after absorption. It is, however, to be expected that a root attached to a living plant and isolated storage tissue should not behave in the same way. Similarly, the latter authors have obtained evidence that the extent of absorption depends very greatly on the nature of the salt (*Proc. Roy. Soc.*, *B*, **90**, 487–504, 1919), the absorption of calcium salts and sulphates appearing to take place to a less extent than potassium and sodium salts and chlorides and nitrates. Here also the difference in the tissues used is quite sufficient to explain differences in the results.

The absorption of boron has been incidentally examined by F. C. Cook and J. B. Wilson (*Proc. Amer. Soc. Biol. Chem.*, *Journ. Biol. Chem.*, **33**, vi–vii, 1918). Plants are very specific in regard to their absorption of boron compounds; thus cereals such as wheat, oats, and rye, absorb very little boron, while succulent plants and plants of the natural order Leguminosæ absorb it in large quantity. Not only does the absorption of boron vary with the species of plant, it also varies with the solubility of the boron compound and the concentration of the compound in the soil, while it also depends on other soil factors, such as the amount of water in the soil, the composition of the latter, and so on.

In a study, by H. S. Reed (*Bot. Gaz.*, **66**, 374–80, 1918), of the absorption of sodium and calcium by wheat seedlings, the antagonistic action of these two ions was demonstrated in very dilute solutions. The experimental plants were analysed at the end of the experiment, and the sodium and calcium also determined in the culture solutions. The author finds that there is no antagonism in regard to the absorption of the

ions, but that the beneficial effect of calcium in lowering the harmful effects of sodium must take place in the plant.

A similar series of observations has been made by D. D. Waynick (*Univ. California Publications in Agric. Science*, **3**, 135–242, 1918), with barley growing in Shive's three-salt water culture medium to which other salts were added in various amounts and proportions. At the end of the experiment the shoots and roots were analysed and the various ions determined. A great number of results are recorded. In every case where the amount of growth was markedly reduced, the analysis of the plant showed that an excessive amount of calcium or magnesium had entered the plant. The author adduces this result as further proof of the close relation between antagonism and cell permeability. Both the experimental results and conclusions thus appear to be strongly opposed to those of Reed cited above.

The earlier work of Pantanelli (*Bull. Orto Bot. R. Univ. Napoli*, **5**, 1–54, 1915) on the absorption of ions by plants has now been extended by a further series of observations (*Bull. Orto Bot. R. Univ. Napoli*, **6**, 1–37, 1918). Species from different parts of the plant kingdom were investigated—namely, *Lupinus albus*, *Vicia faba*, onion, *Valonia utricularis*, and yeast, and the absorption of ions from a number of salts determined by chemical analysis of the culture solutions in which the plants were growing. The author confirms his earlier observation that the ions of a salt are absorbed independently of one another, and this holds both for the early rate of absorption and for the total quantity absorbed. The phenomenon is quite general, and holds equally for flowering plants and Thallophyta. For instance, in the absorption of anion from potassium chloride (0·02 N), after four days the quantity of chlorion absorbed is almost twice the quantity of potassium taken up, measured in equivalents (milligram-ions). Similarly, in the case of *Valonia*, after sixteen hours the ratio of kation to anion absorbed was found to be 16·7, an extreme case of independent ionic absorption. The rate at which the ions are absorbed varies with the species very greatly; in general any particular ion is absorbed much more rapidly by unicellular organisms or by organs rich in protoplasm than by pluricellular organisms or by organs poor in protoplasm. Not merely are the rates of absorption of different ions different, but this rate

also depends on the salt. Thus potassion is not absorbed with the same rapidity by the same plant from different potassium salts. The rate of absorption is also influenced by the internal and external conditions of the plant. It may occasionally happen that in experiments extending over some considerable time equivalent quantities of the constituent ions of a salt may be absorbed; but this is by no means always the case, and generally when equilibrium is reached an unequal absorption of ions has taken place.

In some cases ions accumulate in the cell and pass in against the apparent concentration gradient, while in other cases the passage of ions into the cell is never sufficient to reach that required for equality of distribution between the interior of the cell and the external medium. This result has also been obtained by Stiles and Kidd (*loc. cit.*), who have shown that the extent of ionic intake depends on the concentration of the salt and on its nature. Generally, with "nutrient" salts, the "absorption ratio"—that is, the ratio of final internal to final external concentration—varies from many times unity with low concentrations to a fractional value for high ones.

A very curious observation is recorded by Pantanelli, to the effect that when absorption is rapid, it is not continuous, but that absorption is followed by partial excretion of the same ion. Then fresh absorption follows, but less than the first, then excretion takes place, then further absorption, and so on, the whole process being comparable to the oscillations of a damped pendulum whose amplitude gets less and less with time. This phenomenon requires further investigation.

As a consequence of unequal absorption of ions, it follows that there must be either diffusion of ions from the tissue, or that one of the ions of water must enter along with the excess of salt ion absorbed. Resulting from this, the solution must become acid or alkaline. Pantanelli himself inclines to the latter view. D. R. Hoagland, however (*Science*, **48**, 422–5, 1918), questions the evidence upon which the opinion rests that water culture solutions become markedly acid or alkaline owing to excess absorption of one ion of a salt. On the contrary, he found that when plants were put into nutrient solutions with an acid reaction, this last became neutral after the solutions had remained in contact with the roots for some time. Similarly, when plants were transferred to solutions of

single salts, the solutions never became markedly acid or alkaline, and in the case of potassium chloride, where analyses were made, it was found that the two ions were absorbed in equivalent amounts.

ZOOLOGY. By PROF. CHAS. H. O'DONOGHUE, D.Sc., F.Z.S., University of Manitoba, Winnipeg, Canada.

Protozoa.—Calkins, in a paper "*Uropeltis mobilis*, Engelm I, History of the Nuclei during Division and Conjugation" (*Journ. Exp. Zool.*, vol. xxvii, Jan. 1919), has described the nuclear changes in this rare hypotrichous ciliate. The resting macronucleus consists of eight parts, which unite and divide three times before and once after cell division. The micronuclei vary in number from four to six, all but two are absorbed before division; these undergo two or three mitotic divisions, and then a variable number of them are absorbed. The macronuclei play no part in conjugation. The micronuclei undergo maturation divisions with a reduction of the chromosomes from eight to four. As many as eight pronuclei may be formed, but only two are functional, the others being absorbed. The migratory nucleus has an attraction sphere. Five days are required for the reorganisation of the cell after conjugation. Mast has investigated "Reversion in Orientation to Light in the Colonial Forms, *Volvox globator* and *Pandorina morum*" (*ibid.*, Jan. 1919), and suggests that the sense of orientation is dependent upon physiological conditions of the colony as well as the conditions of the culture medium; it is also dependent on the age of the colony. Reversion in colonies exposed to constant illumination is not influenced by photosynthesis, but is probably due to changes in permeability.

Other papers include:

Boyd, "Observations upon *Trichomonas intestinalis* in Vitro," (*Journ. Parasit.*, vol. v, March 1919; Chambers, "The Effect of Some Food Hormones and Glandular Products on the Rate of Division of *Paramœcium caudatum* (*Biol. Bull.*, vol. xxxvi, Feb. 1919); Flather, "The Effects of a Diet of Polished Rice upon the Metabolic Activities of *Paramœcium* (*ibid.*, Jan. 1919); Juday, "A Fresh-water Anaerobic Ciliate" (*ibid.*, Feb. 1919); and Mason, "A Case of *Balantidium coli* Dysentery" (*Journ. Parasit.*, vol v, March 1919).

Invertebrata.—Papers include:

Child, "Demonstration of the Axial Gradients by Means of Potassium Permanganate" (*Biol. Bull.*, vol. xxxvi, Feb. 1919); Child and Manning, "Axial Gradients in the Hydrozoa" (*ibid.*, March 1919); and Parker, "The Organisation of Renilla" (*Journ. Exp. Zool.*, vol. xxvii, Feb. 1919).

Investigations of certain of the glands in earthworms have been recorded by Stephenson and Prashad, "The Calciferous Glands of Earthworms" (*Trans. Roy. Soc. Edin.*, vol. lii, April 1919), and Stephenson and Ram, "The Prostate Glands of the Earthworms of the Family Megascolecidæ" (*ibid.*, April 1919). The calciferous glands are shown to be foldings of the œsophageal epithelium, although in extreme cases they may become sacs with a duct. Their epithelium is in all cases continuous with that of the œsophagus, and the differences in form are due to differences in complexity of folding. They are then not mesodermal, but entodermal, in origin. Tubular or lobate prostate glands at the termination of the sperm ducts are common in the family, and typical species have been examined by the authors. The glandular cells disintegrate to give rise to a granular secretion, and are regenerated by the formation of new cells at the periphery of the gland. The glands themselves are stated to be of mesodermal origin, and in early stages their cells resemble the chromophil cells of the pharyngeal mass.

Other papers include:

Chandler, "On a Species of *Hedruris* occurring Commonly in the Western Newt, *Notophthalmus torosus* (*Journ. Parasit.*, vol. v, March 1919); Kanda, "On the Reversibility of the Heliotropism of *Arenicola* Larvæ by Chemicals" (*Biol. Bull.*, vol. xxxvi, March 1919); Osborn, "Observations on *Microcephalus ovatus*, sp. nov., from the Crayfish and Black Bass of Lake Chautauqua, N.Y." (*Journ. Parasit.*, vol. v, March 1919); Pratt, "A New Cystocercus Cercaria" (*ibid.*, March 1919); Ransom and Foster, "Recent Discoveries Concerning the Life History of *Ascaris lumbricoides*" (*ibid.*, March 1919); Yoshida, "On the Development of *Ascaris lumbricoides*" (*ibid.*, March 1919); Crozier, On the Use of the Foot in Some Molluscs" (*Journ. Exper. Zool.*, vol. xxvii, Jan. 1919); Stark, "An Hereditary Tumour" (*ibid.*, Feb. 1919), a study of a sex-linked tumour occurring in the larvæ of one-half of the males of the fruit-fly (*Drosophila*), and causing the death of the same; Thomson and Snyder, "The Question of the Phylogenetic Origin of Termite Castes" (*Biol. Bull.*, vol. xxxvi, Feb. 1919).

Vertebrata.—In "Concerning Reissner's Fibre in Teleosts" (*Journ. Comp. Neur.*, vol. xxx, Feb. 1919), Jordan records his studies on two teleosts, the brook trout and the samlet, and suggests that this structure does not function either directly or indirectly as a nervous mechanism. It is produced by the joining together of a number of tiny fibres produced by certain of the ependymal cells, and its constituent fibrils can be traced to the nuclei of the parent cells. The ependymal fibrils of other cells in the roof of the canalis centralis, and also those that

produce Reissner's fibre stain, similarly to elastin. The peculiar uplifting of the tail that was recorded by Nicholls after severing the posterior end of the fibre is also stated to have been obtained by other injuries to the tail of the fish not involving the fibre itself.

Other papers include :

White, "Association and Colour Discrimination in Mud-minnows and Sticklebacks" (*Journ. Exper. Zool.*, vol. xxvii, Feb. 1919); Bigney, "The Effect of Adrenin on the Pigment Migration in the Melanophores of the Skin and in the Pigment Cells of the Retina of the Frog" (*Journ. Exper. Zool.*, vol. xxvii, Jan. 1919); Clark, "On the Relation of Certain Cells in the Tadpole's Tail toward Vital Dyes" (*Anat. Rec.*, vol. xv, Dec. 1919); Swingle, "Studies on the Relation of Iodin to the Thyroid : I, The Effects of Feeding Iodin to Normal and Thyroidectomised Tadpoles" (*Journ. Exp. Zool.*, vol. xxvii, 1919); Swingle, "II, Comparison of the Thyroid Glands of Iodin-fed and Normal Frog Larvæ" (*ibid.*, Jan. 1919).

"The Relation of Plumage to Ovarian Conditions in a Barred Plymouth Rock Pullet" has been described by Cole and Lippincott (*Biol. Bull.*, vol. xxxvi, March 1919). This pullet, normal at first, later assumed male plumage, and the apparent cause was the development of a large ovarian tumour. A piece of ovary from another pullet was subsequently implanted in the pullet, and later the feathers removed from the left side of the body. When these feathers were regrown they were of the hen type, so that now one side was male-plumaged and the other side was female—that is, as far as the structure and shape of the feathers were concerned; but, strangely enough, in both cases the width of the barring was that of the female.

Other papers include :

Allen, "Glycogen in the Chick Embryo" (*Biol. Bull.*, vol. xxxvi, Jan. 1919); Alsop, "The Effect of Abnormal Temperatures upon the Developing Nervous System in the Chick Embryo" (*Anat. Rec.*, vol. xv, Jan. 1919); Danforth, "The Relation of Brachydactylism to Other Characteristics in the Domestic Fowl" (*Amer. Journ. Anat.*, vol. xxv, March 1919); and Poynter, "Some Observations on Wound-Healing in the Early Embryo" (*Anat. Rec.*, vol. xvi, March 1919).

Hartman has added two further papers to his series on Marsupial embryology in "Studies in the Development of the Opossum (*Didelphys virginiana*): III, Description of New Material on Maturation, Cleavage, and Entoderm Formation ; IV, The Bilaminar Blastocyst" (*Journ. Morph.*, vol. xxxii, March 1919). In these papers the author records his observa-

tions on a series of 641 normal eggs in a close succession of stages, probably a more complete set than has been dealt with in any other animal. They include stages from the ovarian and tubal egg through maturation, cleavage blastocyst formation, entoderm formation, and the bilaminar stage, to the first proliferation of mesoderm. Entoderm appears almost as early as in the eutheria from mother cells of a unique type that differentiate in one-half of the blastocyst wall. A number of interesting features are described, and the whole is well illustrated. "Studies on the Ovary of the Spermophile (*Spermophilus citellus tridecemlineatus*), with Special Reference to the Corpus Luteum," have been placed on record by Drips (*Amer. Journ. Anat.*, vol. xxv, March 1919). This is an account of the activities of the reproductive organs of the animal during the period of functional activity, particularly emphasising the histological changes in the corpus luteum. Three stages are recognised: the stage of the formation of secretory granules, lasting through most of the period of pregnancy; the stage of formation of lipoid droplets; and the stage of regression. Various experiments in ovariectomy, transplantation, etc., are also described, and their import discussed. Jordan describes "The Histogenesis of Blood-platelets in the Yolk-sac of the Pig Embryo" (*Anat. Rec.*, vol. xv, Feb. 1919). They arise from the giant-cells mainly, but also from the hemoblasts, either by the segmentation of the pseudopodia or by the fragmentation of the larger areas of protoplasm. A similar mode of origin is encountered in both the yolk-sac and the red marrow of bones, and fortunately the absence of metachromatic granules from the osteoclasts enables them to be readily distinguished from the giant-cells.

Other papers include:

Allen, "Application of the Marchi Method to the Study of the Radix Mesencephalica Trigemini in the Guinea-pig" (*Journ. Comp. Neur.*, vol. xxx, Feb. 1919); Chapman, "A Study of the Correlation of the Pelvic Structure, and the Habits of Certain Burrowing Mammals" (*Amer. Journ. Anat.*, vol. xxv, 1919); Ellis, "A Preliminary Quantitative Study of the Purkinje Cells in Normal, Subnormal, and Senescent Human Cerebella, with Some Notes on Functional Localisation" (*Journ. Comp. Neur.*, vol. xxx, Feb. 1919); Hanson, "Nerve Foramina in the Pig Scapula: a Peculiar Relation existing between the Dorsalis Branch of Several Spinal Nerves and the Superscapula in the Pig" (*Anat. Rec.*, vol. xv, Jan. 1919); Hunt, "The Variations of the Inferior Thyroid Vein of the Domestic Cat" (*ibid.*, vol. xvi, March 1919); McJunkin, "The Origin of the Phagocytic Mononuclear Cells of the Peripheral Blood" (*Amer. Journ. Anat.*, vol. xxv, Jan. 1919); Pohl-

man, "Double Ureters in Human and Pig Embryos" (*Anat. Rec.*, vol. xv, Feb. 1919); Scammon, "On the Development and Finer Structure of the Corpus Adiposum Buccæ" (*ibid.*, Jan. 1919); Senior, "On the Development of the Arteries of the Human Lower Extremity" (*Amer. Journ. Anat.*, vol. xxv, Jan. 1919); Streeter, "Factors Involved in the Formation of the Filum Terminale" (*ibid.*, Jan. 1919); and Thuringer, "The Anatomy of a Dicephalic Pig (*Monosomus diprosopus*)" (*Anat. Rec.*, vol. xv, Feb. 1919).

General.—Jacobs, in "Acclimatisation as a Factor affecting the Upper Thermal Death Points of Organisms" (*Journ. Exp. Zool.*, vol. xxvii, Jan. 1919), has made a study of the time-temperature relation in bringing about the death of starfish larvæ and *Paramœcium*. The effect of a gradual rise of temperature is different in the two cases. In the starfish larvæ, submitted to a gradual rise of temperature, death occurs at the point where the summation of time-temperature factors for the temperatures passed, reaches the total that is fatal to the animal if it were submitted to a constant temperature for a sufficient length of time to bring the total to the same amount; in *Paramœcium*, on the other hand, it is found that, if the temperature be raised quite slowly, it is not killed until the summation of the *t-t* factors has exceeded the total of a constant temperature-time total. This extra amount of resistance is described by the author as "surplus resistance," and is interpreted by him as acclimatisation. Using the method of measuring adopted in the paper, the starfish larvæ are in the vicinity of zero, while the *Paramœcium* was about 45°.

Other papers include:

Allee, "Note on Animal Distribution following a Hard Winter" (*Biol. Bull.*, vol. xxxvi, Feb. 1919); Allen, "A Technique which Preserves the Normal Cytological Conditions in both Germinal and Interstitial Tissue in the Testis of the Albino Rat (*Mus norvegicus albinus*)" (*Anat. Rec.*, vol. xvi, March 1919); Lund, "Simple Method for Measuring CO_2 produced by Small Organisms" (*Biol. Bull.*, vol. xxxvi, Feb. 1919); Pohlman, "The Use of Bayberry Wax in Hardening Paraffin Blocks" (*Anat. Rec.*, vol. xv, Feb. 1919); "A Modification of the Born Paper-wax Reconstruction Plate," and "The Use of a Simple Method of Recording the Relations in Serial Sections, particularly for Use in Teaching Embryology" (*ibid.*, Feb. 1919).

ANTHROPOLOGY. By A. G. Thacker, A.R.C.Sc., Public Museum, Gloucester.

Much interesting matter will be found in the recently-established American quarterly publication entitled *The American Journal of Physical Anthropology*, which is edited by Prof. A. Hrdlicka. The first article in vol. ii, No. 1, of this journal

(being for January—March 1919) is entitled "Some Racial Characteristics of the Spleen Weight in Man," and is by Robert B. Bean and Wilmer Baker. The authors deal with the weight of the spleen in white people and the negro race respectively, giving data relating to both sexes, and their figures prove that the spleen is a much larger organ in the white race than in the negro. In the male sex the difference amounts to about 25 per cent., and white women would appear to have spleens 50 per cent. larger than those of negresses. These differences are to be observed in both normal and pathological spleens. A second paper in the same journal which calls for special mention is one entitled "Inheritance of Eye-colour in Man," by Heléne M. Boas. The authoress gives a series of statistics which she claims do not support the theory of the Mendelian inheritance of eye-colour. The arithmetical totals are, however, not large, and it is therefore impossible to base any certain conclusions upon them; and in regard to the cases in which brown-eyed children have been born of parents both of whom are alleged to have had blue eyes, there is the difficulty, as the authoress herself points out, that persons with some brown in their eyes may well have been included among the group of "blue-eyed" parents. A third paper, by L. R. Sullivan, deals with the "Samar" united twins, and gives a photograph of the boys.

The *Journal of the Royal Anthropological Institute*, vol. xlviii, pt. 2 (July to December 1918), is given up almost wholly to social anthropology. This number includes the first instalment of A. R. Brown's "Notes on the Social Organisation of Australian Tribes." These contributions are to include "notes on the tribes about which the author has collected first-hand information from the natives themselves," and the first instalment contains much interesting information about the Yaralde and other tribes.

The Proceedings of the chief anthropological society of Austria often contain extremely interesting articles, and have done so even during the period of the war. Reference must therefore be made to the *Mitteilungen der Anthropologischen Gesellschaft in Wien*, band xlviii, heft 6, which is the latest issue of the Society's publication received. Among other contributions, this issue contains two articles of special interest. One of them is by F. Kieszling, and is entitled, "Die Aurignacien-

station im Gruebgraben bei Kammern in Niederoesterreich." This article is illustrated by three good plates and by figures in the text. The second article is by Hella Schuerer von Waldheim, and is entitled, "Vorgeschichtliche menschliche Funde aus Stillfried." This article is illustrated by one plate showing Neolithic skulls. Stillfried is situated on the border between Lower Austria and Hungary.

The *Proceedings of the Prehistoric Society of East Anglia*, for the year 1917–18 (vol. ii, pt. 4), fully maintain the reputation of the new society. The first article in the new issue is the Presidential Address by Reginald A. Smith, and is entitled, "Our Neighbours in the Neolithic Period." In his address, Mr. Smith discusses in great detail the relics of the Neolithic period found in Scandinavia, North-Western Germany, the Low Countries, and Northern France. He deals particularly thoroughly with the Neolithic period in Scandinavia, the Neolithic culture of that region having been worked out in great detail. The remains of this epoch found in Scandinavia are very plentiful, and it has been found possible to base upon the evidence which they afford a detailed and probably fairly accurate subdivision of the Neolithic period into smaller epochs. It is probable that the Neolithic period lends itself best to a threefold subdivision. In the early stage the dog was the only domestic animal known, and the shell-mounds (otherwise known as kitchen-middens) contain the most numerous relics of the first period. It is probable that the so-called Azilian period should be included as a subdivision of the first division of the Neolithic. The Campigny culture of France must be regarded as a transition period between the Early and Middle Neolithic epochs. During the Campigny period pit-dwellings were constructed, and it is probable that cattle had begun to be domesticated. During the second subdivision, the Middle Neolithic or Megalithic period, agriculture (as well as cattle-farming) was well understood, and lake-dwellings and Megalithic tombs were abundant. After this comes the late Neolithic or "Epimegalithic" subdivision, and stone cists are then found. A feature of Mr. Smith's paper, which will be very valuable to students, is a table in which he gives a vocabulary of the technical terms used by archæologists in referring to the Neolithic period, the vocabulary consisting of the six languages, English, French, German, Norwegian, Swedish,

and Danish. The address is one of the most interesting which Mr. Smith has ever published. A paper on a very different subject is contributed to these *Proceedings* by the Hon. Secretary of the Society, Mr. W. G. Clarke. This is entitled, "The Icknield Way in East Anglia." The paper is a detailed study of the eastern section of the famous ancient highway. The Icknield Way clearly ante-dates the Roman occupation, and, though it was used by the Romans, it was not remade by them. "There is ample evidence," says Mr. Clarke, "that the East Anglian portion was an important highway in Saxon times, and it so continued throughout the Middle Ages, remaining one of the chief highways into Norfolk from the west and south-west until the end of the seventeenth century, when it was superseded by turnpike roads." Mr. Clarke believes that the way dates from the latter part of the Neolithic period.

A paper which throws much light upon the primitive mentality of the Lapps as recently as last century is to be found in *Folk-Lore*, vol. xxix, No. 3 (Sept. 1918). This article is by Charles J. Billon, and is entitled, "Some Mythical Tales of the Lapps." Most of the tales in question, as also many others, were obtained from a certain Pastor A. Fjellner, of Sorsele in Lappmark, who was born in 1795. He lived to a great age, and in his declining years dictated folk-tales and folk-songs to various scholars, including Donner, the author of the work *Lieder der Lappen.*

As is now well known, much has been discovered about the ancient Hittites during the last twenty years. Those interested in these people, who were in truth one of the most powerful nations of antiquity, may be referred to an article in the *Journal of the Royal Asiatic Society*, January 1919, which is by Prof. S. J. Crawford, and is entitled, "Decipherment of the Hittite Language." The great discoveries of Dr. Friedrich Hrozny, of Vienna, proved that the Hittites spoke a language of the Indo-European group—and belonging to the Western division of that group. Hittite appears, in fact, to have been closely related to Latin.

The following papers on Physical Anthropology may be recorded:

In the *American Journal of Physical Anthropology*, vol. ii, No. 1.—"Hair Colour of the Italians," by F. Boas; "Changes in Fetuses due to Formalin

Preservation," by A. H. Schultz; "Anthropometry," by A. Hrdlicka; "The International Agreement for the Unification of the Anthropometric Measurements to be made on the Living Subject," being the Report of the Commission appointed by the Fourteenth International Congress of Prehistoric Anthropology and Archæology at Geneva (1912); "English Translation of the Official Version," by W. L. H. Duckworth; and "The Effect of the War upon the American Child," by Ruth McIntire.

The following papers on Social Anthropology may be recorded:

In the *Journal of the Royal Anthropological Institute*, vol. xlviii, pt. 2.—"Some Sakai Beliefs and Customs," by I. H. N. Evans; "The Two-handed Clubs of the Maoris," by H. D. Skinner; and "Some Aspects of Nayar Life," by K. M. Panikkar.

In *Man*.—"Witches' Transformations into Animals" (December), and "Witches' Fertility Rites" (April), by Miss M. A. Murray; "The Agiba Kult of the Kerewa Culture," by A. C. Haddon (December); "Some Thoughts on the Subject of Language," by A. H. Gardiner (January); "Notes on Pokomchi (Guatemala)," by A. C. Breton (January); "Hut-burning in the Ritual of India," by W. Crooke (February); "Note on the Gourd as an Amulet in Japan," by W. L. Hildeburgh (February); "The Highest known Maya Number," by R. C. E. Long (March); "Mothers and Children at Laguna," by Elsie C. Parsons (March); "Milk Customs of the Todas," by Sir J. C. Frazer and K. R. Achariyar (March); and "The Northern Babunda," by E. Torday (April).

In the *Journal of the Royal Asiatic Society*, January 1919.—"Four Assyriological Notes," by S. Langdon, and "Labour Songs in India," by W. Coldstream.

In *Folk-Lore*, vol. xxix, No. 3.—"A Medieval Legend of the Terrestrial Paradise," by M. Esposito; "The Provenience of Certain Negro Folk-tales," by Elsie C. Parsons; "'Prentice Pillars: the Architect and his Pupil," by W. Crooke; and "The Black Pig of Kiltrustan," by Eleanor Hull.

And the following papers on Prehistoric Anthropology may be recorded:

In the *Proceedings of the Prehistoric Society of East Anglia*, vol. ii, pt. 4.—"The Ancestry of the Mousterian Paleolithic Flint Implements," also "The Flaking and Flake-characteristics of a Pre-Red-Crag Rostro-Carinate Flint Implement," and also "The Fracturing of Flints by Natural Agencies in Geological Deposits," these three papers being by J. Reid Moir; "Surface Paleolithic Implements from the Chilterns," by A. Peake; "Surface Implements of a late Paleolithic Site," by Major J. D. Hill; "A Flint-implement Factory Site near Milverton, Somerset," by C. F. Moysey; "Flint Implements from the Plough-lands of South-west Leicestershire," by A. J. Pickering; and "More about Windmill Hill, Avebury, and Grime's Graves," by the Rev. H. G. O. Kendall.

In the *Journal of the Royal Anthropological Institute*, vol. xlviii, pt. 2.—"Excavations Conducted at Ghar Dalam (Malta) in the Summer of 1917," by G. Despott.

In *Man*.—"A Piece of Carved Chalk from Suffolk," by J. Reid Moir (February), and a reply called, "The Latest Prehistoric Mare's Nest," by Sir H. H. Howorth (May).

PALÆONTOLOGY. By W. P. Pycraft, F.Z.S., A.L.S., F.R.A.I., British Museum (Natural History), London.

That the list of palæontological papers for the last twelve months is a short one is but natural, having regard to the troublous times through which we have just passed. Most will agree that at the head of that list, in importance, must be placed the Memoir on the Indigenous Mammals of Porto Rico, Living and Extinct, by H. E. Anthony (*Memoirs of the American Museum of Natural History*, New Series, vol. ii, pt. 2).

Bats are the only land-mammals now found in Porto Rico. But in the remote past Insectivora, Rodents, and Ground-sloths were represented. These, however, with one exception, became extinct before the advent of man on the island. This exception is found in the rodent *Isolobodon*, whose remains are found, with fragments of pottery, in the shell-mounds which bespeak a primitive people, and show that this animal was a common source of food to these early inhabitants. The nearest living representatives of *Isolobodon* are the Hutias (*Plagiodon* and *Capromys*) of Cuba and Jamaica, and the South American Coypu.

The modern Agoutis are represented by two genera, *Heteropsomys* and *Homopsomys*; though the author is of opinion that these must ultimately be merged into a single genus. But be this as it may, they are more nearly related to living forms than any others of this island save *Isolobodon*.

More important, from the palæontologist's point of view, are the remains of two large Chinchillids, *Elasmodontomys* and *Heptaxodon*. These present striking peculiarities of dentition, and seem to have been derived from South American ancestors; the living Chinchillids having developed along other lines, and being in many respects less specialised.

Perhaps the most interesting of all the remains which this island has yielded are those of the Insectivore *Nesophontes*, and the Ground-sloths *Acratocnus*.

Nesophontes represents an exceedingly primitive type, widely separated from any existing Insectivores. This much is shown not only in the skull and dentition, but also in the rest of the skeleton. No living Insectivore, indeed, has such an assemblage of generalised characters, so that the gap between any of them and *Nesophontes* is very great. On the

whole its affinities are with *Centetes* and *Solenodon,* themselves very primitive types. But it is more primitive than these, and should probably be regarded as ancestral to several of the living families of Insectivora.

The two species of Ground-sloths, to which reference has been made, were related on the one hand to the much larger *Megalocnus,* of Cuba, and on the other to the more primitive Santa Cruz *Megalonychidæ.*

Quite apart from the value of this memoir to the palæontologist and the zoologist, it furnishes most important evidence as to the origin of the West Indies. For it shows that the relationships of most of the island mammalia run back to the South American Miocene. Surveying the evidence as a whole, the author inclines to the belief that the Antilles, up till late Pliocene times, formed one continuous land mass, connected with Central or South America, or both. And with this interpretation most will agree.

His conclusions are based, it may be remarked, on ample material, and are supplemented by a number of very beautiful text-figures and plates showing not only skulls, teeth, and vertebræ, but also the appendicular skeleton.

Prof. Henry Fairfield Osborn's Memoir, on the Equidæ of the Oligocene, Miocene and Pliocene of North America, will ever be held in grateful remembrance (*Memoirs of the American Museum of Natural History*; New Series, vol. ii, pt. 1). All the genotypes, types, cotypes, paratypes, neotypes, and topotypes which have ever been figured are here reproduced in facsimile. And more than this, many unfigured types are here reproduced for the first time. The labour of compilation which has been compressed into a little over 200 quarto pages, and a separate volume of plates, would have been wellnigh impossible for one author, but it has been accomplished in brilliant fashion by the united efforts of six of the foremost American palæontologists. The condensed statements in regard to the tooth morphology of the fossil *Equidæ,* and the section on the geologic horizons, and life-zones, will repay very careful study.

Yet another has been added to the number of restorations of that remarkable reptile *Triceratops.* This one by Mr. Charles Gilmore (*Proc. U.S. Nat. Mus.,* vol. lv). The modelling of the limbs in this latest attempt is by no means convincing. The

author's observations on the osteology of the genus are valuable, but we note that he ignores the proposals of the American Committee on the nomenclature of the cranial elements of the Reptilia. The findings of that Committee, which were eminently sound, were published by the Geological Society of America in 1917.

In a short paper on the significance of the divergence of the first digit in the primitive mammalian foot, Mr. J. W. Gidley has contrived to raise some interesting and important points (*Journ. of the Washington Academy of Sciences*, vol. ix). He comes to the conclusion that the primitive mammalian foot must have been primarily terrestrial, and that from this generalised type of foot, with *divergent* but not *primarily* opposable first digit, have been developed all sorts of modifications of foot structure. The divergence of the first digit, he considers, is primarily an inheritance from the primitive reptilian condition, and cannot be considered as in any way supporting the hypothesis of an arboreal ancestry of the mammalia.

Finally, mention must be made of the memoir on the Appendages of Trilobites, by Prof. C. D. Walcott (*Smithsonian Miscellaneous Collections*, vol. lxvii, No. 4). For more than forty-five years the author has been working on this theme. He now publishes the notes and illustrations he has accumulated since 1894, and these will meet with the appreciation they deserve, not only by palæontologists, but students of living Crustacea, for the author not only commands an unrivalled knowledge of his subject, but he has had the further advantage of exceptionally fine material to work upon.

ARTICLES

OPTICAL ACTIVITY

By F. D. CHATTAWAY, F.R.S., D.Sc.

Towards the close of the seventeenth century Erasmus Bartholin,[1] a Danish physician and philosopher, noted that a ray of light, in passing through a crystal, which had been sent to him from Iceland, was divided into two equal parts. Some few years later his observations were confirmed and extended by Christian Huygens, who found that the two new rays acquired peculiar properties which became evident when they were passed through a second crystal.

Little further progress was made for over a century until Étienne Louis Malus, a French Colonel of Engineers, discovered in 1808 that light reflected at a certain angle from the surface of any transparent body acquired properties corresponding to those of the rays transmitted through Iceland spar. He was led to the discovery by casually examining, through a doubly refracting crystal of calcite, the sunlight reflected from the windows of the Luxembourg Palace, when he was surprised to find that the two rays alternately disappeared as the crystal was rotated through successive right angles.

To the alteration of the properties of light thus produced, Malus gave the name "polarisation," as on the emission theory which he held at the time he attributed it to a kind of polarity of the light corpuscles, and the term has been retained. The reflection plane itself—that is, the plane passing through the incident ray and the normal to the reflecting surface—was designated the "plane of polarisation."

[1] "Erasmus Bartholin: An accompt of Sundry Experiments made and communicated by that Learned Mathematician, Dr. Erasmus Bartholin, upon a Chrystal-like Body, sent to him out of Island," *Phil. Trans.* January 16, 1670/71, No. 67, p. 2039.

Malus, unhappily, was not able to continue the work so brilliantly begun. His health had been undermined during his military service with the Egyptian Expedition of Napoleon, and within four years he died of phthisis in Paris on February 23, 1812.

The next advance was made by Arago, who in 1811 discovered that when a ray of polarised light, after it had passed perpendicularly through a quartz plate cut in a direction at right angles to its axis, was analysed by a rhomb of Iceland spar, two rays coloured in complementary tints were produced in all positions of the rhomb. Arago noted that the appearances presented were exactly those which would be observed if the different coloured components of the incident white light emerging from the quartz plate were polarised in different planes.

The further development of this discovery, which singularly enough Arago did not follow up, we owe to Biot. He produced the polarised light separately from different parts of the spectrum, and found that the original plane of polarisation was rotated by the quartz through an angle proportional to the thickness of the plate, that this angle was different for each of the primary colours, and increased according to a definite law with the refrangibility of the light. He also made the further noteworthy observation that, when the quartz plates were cut from different varieties of crystals, some rotated the plane of polarisation to the right, others to a similar extent to the left.

Substances thus able to rotate the plane of polarisation of light are termed "optically active."

Biot passed from this to a still more fruitful discovery, that a number of natural organic substances, such as oil of turpentine in the liquid or gaseous condition, and solutions of sugar, camphor and tartaric acid, also rotate the plane of polarisation of light. This behaviour of gaseous, liquid and dissolved substances showed that optical activity was not necessarily connected with the crystalline state, but in certain cases, at least, resided in the individual molecules of which the bodies were composed.

Biot found that, as with solid quartz, the rotation produced in the plane of polarisation of light by passage through a liquid or dissolved active substance is dependent upon the

wave-length of the light used, and upon the temperature and the thickness of the layer of substance through which the light passes.

About the time of Biot's discovery (1815) which first brought the phenomena of polarisation under the consideration of chemists, Mitscherlich observed the close connection existing between crystalline form and chemical composition, a circumstance which naturally directed attention also to crystal measurements.

Haüy had early divided quartz crystals into two varieties showing hemihedral facets inclined to the right or the left when the crystals were orientated in a given manner. The observations of Haüy and Biot were brought into relationship by Herschel,[1] who showed that the two kinds of hemihedral crystals were respectively dextro- and lævo-rotatory, and concluded that the hemihedral facets were produced "by the same cause which determines the displacement of the plane of polarisation of a ray traversing the crystal parallel to its axis." Our knowledge of these obscure phenomena was a very little later advanced almost to the position in which it stands at the present day. About the beginning of the second quarter of the nineteenth century the interest of chemists was mainly centred on substances of organic origin, and one of the most interesting problems of the time was the relationship of the "acid from the Vosges," or racemic acid, to the long-known tartaric acid. A careful investigation by Berzelius had emphasised their close resemblance, the chief difference being that, whilst tartaric acid and its salts were dextro-rotatory, racemic acid and its salts were optically inactive.

A crystallographic examination of the double sodium ammonium salts of the two acids by Mitscherlich only served to render the difference less explicable, for he stated in a communication to the French Academy in 1844 that the two salts had the same chemical composition, the same crystalline form with identical angles, the same density and the same double refraction. With the exception of the presence or absence of optical activity the salts were as similar as seemed possible without being identical.

Mitscherlich concluded from this similarity of properties that the nature and the number of the atoms in the molecule,

[1] *Trans. Camb. Phil. Soc.* 1882, **1**, 43.

their arrangement and distances apart, were the same in the two salts.

Matters were in this condition when, in 1848, Pasteur, then a young man of twenty-six, made the first of his long series of brilliant discoveries. Having just completed his studies at the École Normale, he decided to re-examine crystallographically tartaric acid and its salts in order to perfect himself in crystal measurement, a subject which much attracted him. In this re-examination he noticed that hemihedral facets, although not very obvious, and consequently unrecognised by previous observers, were present on the crystals both of tartaric acid and of its salts.

Pasteur, being acquainted with Herschel's proof of the connection between the hemihedry and the optical activity of quartz, became convinced that a similar relationship must exist between the hemihedry of the tartrates and their optical activity. He reasoned that, although Mitscherlich had failed to observe hemihedral facets on crystals of ammonium sodium tartrate, he was probably correct in the statement that they did not exist on the crystals of the inactive ammonium sodium racemate.

To his surprise, however, he found hemihedral facets also on the crystals obtained by allowing a solution of ammonium sodium racemate to evaporate slowly at the ordinary temperature. On examining both the tartrate and racemate crystals more closely, however, he found that there was a difference between them, for, whilst in the tartrate the hemihedral facets were identically disposed, in the racemate the disposition was of two kinds, conventionally termed right-handed and left-handed respectively. Faced by this unexpected result, he carefully picked out the right-handed crystals from the left and examined a solution of each in the polariscope. He then found that the solution of the right-handed crystals was dextro-rotatory and that of the left-handed lævo-rotatory, whilst a solution of equal weights of each of the two kinds was inactive to polarised light in consequence of the compensating equal but opposite activities of the two forms.

From these active salts he prepared the two corresponding active tartaric acids. The lævo variety of the acid had up to that time been unknown. These varieties were not distinguishable chemically, and crystallised in similar forms with

identical angles, the only difference between them consisting in the right- and left-handed disposition of their hemihedral facets and in the right- and left-handed directions of their equal rotatory powers.

Pasteur at once grasped the full significance of his discovery, and concluded that in the molecules of these closely related acids the atomic arrangements must be so nearly identical that they differ only as an asymmetric [1] object and its image, and he put the question, "Are the atoms of the dextro-acid grouped in the form of a right-handed spiral or are they placed at the angles of a regular tetrahedron or disposed according to some other asymmetric non-superposable arrangement?"

It is a striking mark of Pasteur's genius that, before leaving the study of the phenomena of optical activity, he had carried it as far as the knowledge of the time regarding the structure of the chemical molecule allowed.

About that period the idea of molecular structure was only just beginning to take shape. This in its most complete form we owe to Kekulé, and even he for over twenty years appears to have looked upon formulæ as geometrical figures drawn upon paper, and hence never to have regarded the atoms as arranged in more than one plane.

Chemical theory, however, only advances when a need arises, and it was not till about 1870 that the inadequacy of plane formulæ began to be felt and the necessity for space formulæ recognised.

As we have seen, Pasteur clearly realised that optical activity, since it is shown by liquids and dissolved substances, must in such cases be due to lack of symmetry in the molecule itself and not to the way molecules are built up into crystals, and stated with precision that this unsymmetric molecular structure must be related in each pair of isomers as an "asymmetric" object is to its mirror image.

Adopting Kekulé's idea of the tetravalency of carbon, van't Hoff and Le Bel in 1874 independently defined for carbon compounds the conditions under which such an asymmetric structure could appear, namely, when a carbon atom was attached to four different groups.

[1] The term "asymmetric," used by Pasteur, and, following him, by most chemists up to the present time, is not strictly correct, inasmuch as enantiomorphous objects are known which are not totally devoid of symmetry.

Van't Hoff, in addition, adopted the tetrahedron as the formal representation of the carbon atom in this new aspect.

It ought, however, to be repeated that the state of knowledge at the time of Pasteur's work did not enable him to put forward anything like a complete theory, which could only come after ideas of molecular grouping had become much more precise.

The views of van't Hoff and Le Bel rapidly gained acceptance, for not only did these chemists state the basic principle, but they showed that it applied to most known active substances, and enabled the possible existence of others to be predicted.

The field of experimental inquiry thus opened was so vast that for many years the study of optical activity was almost entirely restricted to carbon compounds.

The possibility of other multivalent elements serving as the nucleus of asymmetric groupings must, however, have been present to the minds of all chemists approaching the problem, and the element nitrogen soon attracted attention in this connection. General considerations led to the conclusion that in trivalent nitrogen compounds the combined atoms or groups would, under the influence of molecular attraction, arrange themselves symmetrically round the nitrogen atom and thus would lie with it in one plane, which is therefore a plane of symmetry.

Consequently no trivalent nitrogen compound should show optical activity or be resolvable into optically active compounds. This conclusion has been established by experiment, all attempts to resolve such compounds having hitherto proved unsuccessful.

On the other hand, five different groups attached to a nitrogen atom might be disposed so as to form an enantiomorphous grouping, the pentavalent nitrogen atom thus acting as a centre of optical activity in the same way as a carbon atom.

The correctness of this deduction was first established by Le Bel, who showed that a solution of isobutyl propyl ethylammonium chloride, when subjected to the action of moulds, became lævo-rotatory, the dextro-rotatory isomer being preferentially destroyed. It was brilliantly confirmed by Pope and Peachey, who succeeded in isolating the two oppositely active

modifications of benzyl-phenyl-allyl-methyl-ammonium-iodide $(C_6H_5CH_2)(C_6H_5)(C_3H_5)(CH_3)NI$ by combining the base with Reychler's dextro-camphor sulphonic acid,[1] and fractionally crystallising the salt from acetone.

Almost immediately after it had been thus established that pentavalent nitrogen can replace carbon as a centre of optical activity, it was shown that sulphur and selenium can act similarly.

During the past decade what is without question the greatest advance in our knowledge of optically active compounds has been made by Dr. A. Werner, Professor of Chemistry in the University of Zürich, who has resolved into active components a number of complex derivatives of cobalt, chromium, iron and rhodium. This work, which has received only a very grudging acknowledgment in this country, is a natural development of his ideas upon the constitution of these compounds following upon a change of conception of that somewhat vague property of the atom which we call valency.

Valency is a term introduced in order to obtain a numerical expression for the attraction exercised by one atom upon another. When the atomic theory in its modern form was first put forward it was inevitable that atoms should be assumed to combine singly when forming their simplest compounds. Determinations of molecular weight, however, based upon a few simple assumptions, soon showed that this was not true, but that some atoms had the power of holding more than one other atom in combination, and in the case of many elements that the number of other atoms with which one atom of the element can combine depends on the nature of all of the combining atoms, and on the conditions (particularly those of temperature and pressure) under which combination takes place.

With the development of the idea of valency as a property

[1] This is a modification of a method which we also owe to the remarkable scientific insight of Pasteur. He reasoned that, although the two active tartaric acids, when combined with inactive bases, such as potash or soda, yielded salts of the same solubility, it was unlikely that this would be the case when they were combined with bases active in themselves, such as quinine, strychnine, or brucine, and that the solubilities of such salts might differ so widely as to render them separable by this means. This conclusion, which experiment confirmed, led to the most generally applicable method for the separation of optically active acids and bases.

of the atom, measured by the maximum number of certain atoms which the grouping atom can hold in stable union, arose the conception of ordered arrangement within the molecule. The discovery of isomerism associated such ordered structure with definite properties.

The idea of structure led to a system of schematic representation in which the molecules were depicted as frameworks in which valency was represented by lines or strokes proceeding from the atoms as though they were mechanical means of attachment.

Formulæ thus constituted were especially serviceable in organic chemistry, but they introduced a belief or tacit assumption that the attraction bringing about combination was divided up into a definite number of equal parts acting in definite directions from particular points upon the atom. Deviation from these fixed directions was only possible with loss of attractive force, strain and consequent looser attachment. Further, since such forces could not exist without some corresponding force to neutralise them, this, naturally, led to the conception of latent valencies as an explanation of the phenomena of variable valency. The forces unexerted upon other atoms were regarded as mutually satisfying each other.

In recent years, following Werner, the tendency among chemists has been to abandon these ideas and to revert to the older and simpler conception of affinity as a force exerted uniformly outwards from atoms and balanced by the similarly exerted attraction of other atoms in combination with them.

Any association of atoms may therefore possess affinity—that is, be capable of exerting attraction upon other atoms or groups. Consequently, there is no reason why a molecule should not replace an atom, or *vice versa.*

It follows naturally that there must be a space-limit to the number of atoms or molecules which any given atom can associate or hold together, and this must be connected with the actual volumes of the atoms or with the spaces over which they exert an effective influence, thus preventing other atoms or groups from entering.

Further, as the attractive force must diminish in some way as the distance separating the centres of the attracting atoms increases, the union can only be stable if this distance

does not exceed a certain limit. The number of atoms or groups which another atom can hold with a maximum force is therefore of necessity restricted, and whilst, by the resultant attractive force of this close aggregate, other atoms or molecules may be held, the union will be feeble and liable to be disturbed by the competing attraction of other molecules among which they may find themselves, *e.g.* if molecules are so attached they may be broken off when brought into solution, or atoms under similar circumstances may be ionised.

The maximum number of atoms or molecules which a grouping atom can hold in stable combination with itself is termed by Werner the "co-ordination number." This is found to be to a considerable extent independent of the nature of the central atom, and for many elements is six.

These six atoms, or groups, are so firmly held in a more or less complete enveloping sphere or shell enclosing the central nuclear atom, that they are incapable of separate ionisation. Other atoms or groups held by the residual affinity of the central group are in an outer or ionisable zone.

These theories have been chiefly developed in connection with the complicated addition compounds which certain metallic salts, notably those of trivalent cobalt and of chromium, form with other molecules, as for example:

$$CoCl_3{\cdot}6NH_3,\ CoCl_3 2NH_3{\cdot}2H_2O,\ Co(NO_2)_3{\cdot}3NH_3,$$
$$Co(NO_2)_3{\cdot}2NH_3{\cdot}KNO_2$$

According to the views of Werner, which have brought such apparently differently constituted compounds into an ordered system, they all contain a radicle which may be basic, neutral or acidic, consisting of a trivalent metallic atom such as cobalt or chromium associated with six other molecules, atoms or groups which may be ammonia or some similar basic substance (as ethylene diamine or pyridine), or water or halogen atoms, or groups such as NO_2, or partly one and partly others; *e.g.* the radicles in the compounds cited are

$$[Co(NH_3)_6]^{+++},\ [Co(NH_3)_2(H_2O)_2Cl_2]^{-},\ [Co(NH_3)_3(NO_2)_3]^{\circ},$$
$$[Co(NH_3)_2(NO)_2)_4]^{-}$$

The valency and chemical character of these radicles are found to depend not only upon the grouping metallic atom, but also upon the nature of the six molecules, atoms, or groups with

which it is associated. The maximum valency of such radicles, whether basic or acidic, is with a trivalent metal such as cobalt or chromium three, and this maximum valency is only exerted when all the associated groups are of the same character and it diminishes to zero as one to three groups of opposite sign enter the molecule. The compounds cited are therefore fully formulated thus:

$$[Co(NH_3)_6]Cl_3,\ [Co(NH_3)_2(H_2O)_2Cl_2]Cl,\ [Co(NH_3)_3(NO_2)_3],\ [Co(NH_3)_2(NO_2)_4]K$$

A very large number of substances of this nature are known in which ammonia is replaced by other groups or radicles; for example, pyridine can replace ammonia molecule for molecule, ethylene diamine and propylene diamine can each replace two molecules of ammonia.

The residues of dibasic acids, of oxalic (C_2O_4), of sulphuric (SO_4), of carbonic (CO_3) can in such radicles replace two monovalent negative groups or atoms such as NO_2 or Cl. There are besides, possible replacements of ammonia which leave the valency of the complex unaffected; one of these is of great importance—the replacement of ammonia by water molecule for molecule.

Such complexes may be represented as follows:

$$[MeA^{+}_{6}]^{+++} \ldots [MeA^{+}_{6-n}X^{-}_{n}]^{(3+)+(n-)} \ldots (MeX^{-}_{6}]^{---}$$

where Me is a metallic atom, A a molecule of ammonia, or its equivalent in water, pyridine or a diamine, and X a negative group or atom, such as NO_2 or Cl or its equivalent, the valency of the complex being represented by negative or positive signs, as shown. These complexes are held together by the metallic atom Me, the affinity of which is mainly exerted in holding together in firm attachment the six groups associated with it. The valency and chemical character of the complex is determined partly by the nature of the metallic atom, but very largely by the nature of the six groups, the replacement of a positive group such as ammonia by a negative group such as Cl diminishing the positive valency by one unit or increasing the negative valency similarly.

Werner assumes that in these complexes the six groups are symmetrically distributed round the metallic atom and that they effectively occupy all the available space within which

the attraction is sufficiently strong to hold them in stable non-ionisable union. The other ionisable groups attached to them he assumes to be outside this space held in a less firm union by the resultant of all the forces operating, not only those of the central atom but of the closely associated groups. The inner space which no other group or atom can enter without displacing some atom or molecule already there is sometimes spoken of as the inner sphere, or zone, or first sphere of attraction, in contradistinction to the outer sphere, or zone in which those atoms not forming part of the complex are assumed to find their place.

Granting that such complexes exist, the simplest assumption regarding their structure, if the associated groups or atoms are similar, is that they are symmetrically disposed upon the surface of a sphere at the centre of which the grouping atom is situated. This sphere would be somewhat deformed if the groups were different, but would in either case constitute a sort of shell surrounding the inner atom. Six similar particles symmetrically distributed upon the surface of a sphere are in the positions of the angular points numbered 1–6 of the regular octahedron of Fig. 1. This arrangement can be conveniently set up in type in the form of Fig. 2, which will be used throughout this paper for the various octahedral formulæ required.

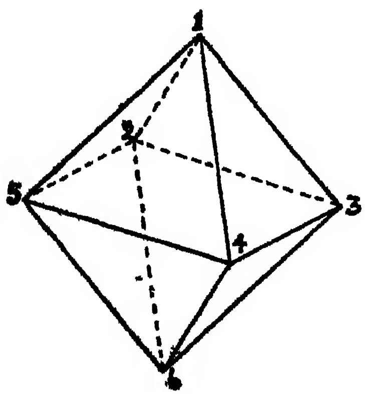


Fig. 1.

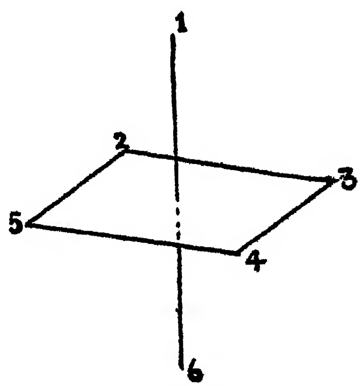


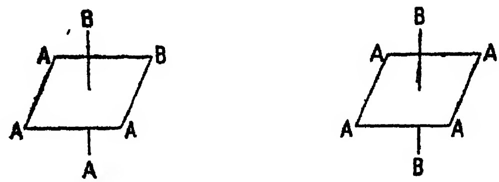
Fig. 2.

If the atoms, groups or molecules are not identical, the relative positions will be the same, and all the spatial relationships will be unaltered though the octahedron may be irregular.

To complete the formulation of such compounds it is

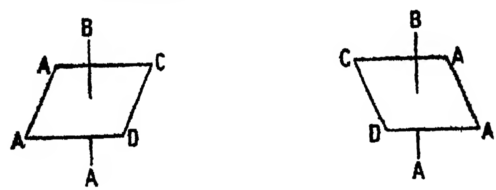
necessary to represent the ionisable atoms or groups as falling, when the compounds assume the solid state, into positions outside this complex, the particular positions taken up being determined by the residual affinity of the whole group—that is, by the resultant of all the chemical forces engaged. If it be granted that the six atoms or groups in the inner zone occupy the space round the central atom so fully that they are not able to exchange their positions without leaving the inner sphere of attraction, this octahedral representation leads to various conclusions relating to the possibility of isomerism.

For example, when the six groups are the same as in [MeA_6] or contain a single other group as in [MeA_5B] the resulting compounds should exist in one form only, whilst those having the composition [MeA_4B_2] should exist in cis- and trans-isomeric series, having configurations which may be represented thus:



This deduction has been confirmed in the case of compounds of the elements platinum, cobalt and chromium; *e.g.* the compounds [$(NO_2)_2Co(NH_3)_4$]X exist in two isomeric series.

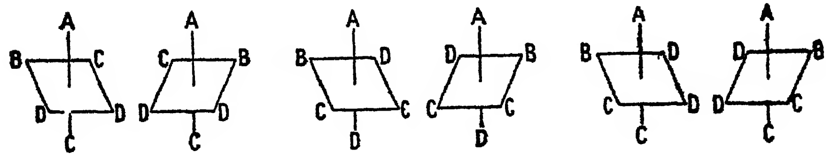
Again, in certain cases the theory predicts the existence of optical isomerism; for example, if the groups B C and D are situated relatively to the central atom in the positions of the angles of one face of the octahedron (for example, the corners 1, 3, 4 of Fig. 1) compounds containing complex radicles of the form [MeA_3BCD] should give pairs of optically opposed isomerides of the configuration



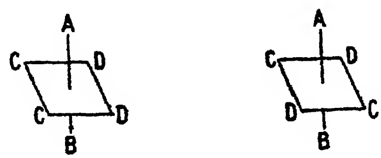
and again in compounds containing complexes of the composition

[$MeABC_2D_2$), [$MeABCD_2E$], and [MeABCDEF]

many structural and optical isomerides are predicted; for example, in the first we have the enantiomorphous isomerides



and also the " structural " isomerides (not enantiomorphous)



Bodies of such a complicated structure either have not hitherto been prepared or else their constitutions have not been sufficiently established for these conclusions to be put to the test. In more simple cases, however, also preceded by theory, it has been possible to test the conclusions reached, since it is not necessary for four or even three different groups to be present so long as at least two pairs are connected, inasmuch as only a different spatial orientation of the *connecting chains* is needed to cause molecular enantiomorphism.

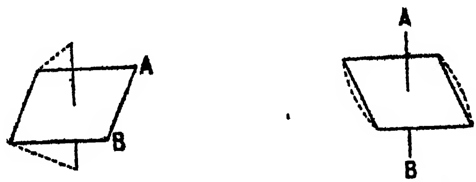
The possibility of such isomerism when only one kind of group is present is shown by the following figure and its mirror image where, using the above conventional representation, the connecting chains are represented by dotted lines [1]:

[1] The figures can be better appreciated if models are used. These may conveniently be made by cutting from large corks octahedra of about 2 cm. edge. Small pins with glass heads or ordinary pins with heads covered by sealing-wax variously coloured may be used to represent atoms or groups, while chains may be represented by thin strips of stiff paper carried by pins stuck into the corks at the angles.

and by the similar figure and its mirror image—

where only two such chains are present, A and B being other atoms, groups or molecules.

In the latter case two structurally different isomers are possible, in which the groups A and B are in cis- and trans-positions respectively.



Cis Trans

Optical activity is only possible in the cis-modification, since in the trans form the structure of the complex is identical with that of its mirror image.

Compounds of these types are well known, containing as the associating atom cobalt, chromium, rhodium or iron, and, as the connecting chains, ethylene diamine

$$(NH_2 \cdot CH_2 \cdot CH_2 \cdot NH_2)$$

or the oxalic acid residue

$$—O.CO.CO.O—$$

the former, ethylene diamine, being able to replace two ammonia molecules in compounds of the type $[(NH_3)_6Me]X_3$ or $[(NH_3)_4MeX_2]X$, where X is some negative atom or group such as a halogen atom or NO_2, and the latter, the oxalic residue, being able to replace two negative groups in compounds of the type $[X_6Me]M'_3$, where M′ is a monovalent metallic atom such as that of potassium.

Werner, to whom the development of the subject is almost wholly due, has resolved many of these compounds, employing for the purpose Pasteur's second method—that is, replacement of the ionisable X or M′ by some active acid or base, the separa-

tion of the oppositely active complexes being in every case possible owing to the widely differing solubility of the pairs of compounds thus produced.

Various acids and bases were employed, laborious investigations having to be undertaken in each case to decide upon the most suitable.

The simplest example of the first type is found in triethylene diamine cobaltic chloride:

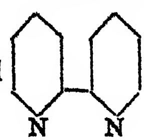
$$[\mathrm{Co(NH_2{\cdot}CH_2{\cdot}CH_2{\cdot}NH_2)_3]Cl_3} \text{ or } {}^{(1)}(\mathrm{Co\ en_3]Cl_3},$$

which is made by leading a stream of air for many hours through a solution of cobaltous chloride containing an excess of ethylene diamine. The resolution of this compound, which may be given as an example, was effected by adding to it two-thirds of the equivalent amount of silver tartrate. On allowing the chloride tartrate $\left[\mathrm{Co\ en_3}\right]^{\mathrm{Cl}}_{\mathrm{C_4H_4O_6}}$ thus formed to crystallise d-triethylene diamine cobaltic chloride tartrate separated. The lævo modification was obtained from the mother liquor. These active chloride tartrates were converted into the bromides of the active bases

$$\mathrm{d]Co\ en_3]\ Br_3 \text{ and } l[Co\ en_3]\ Br_3}$$

by treating them with a concentrated solution of hydrobromic acid.

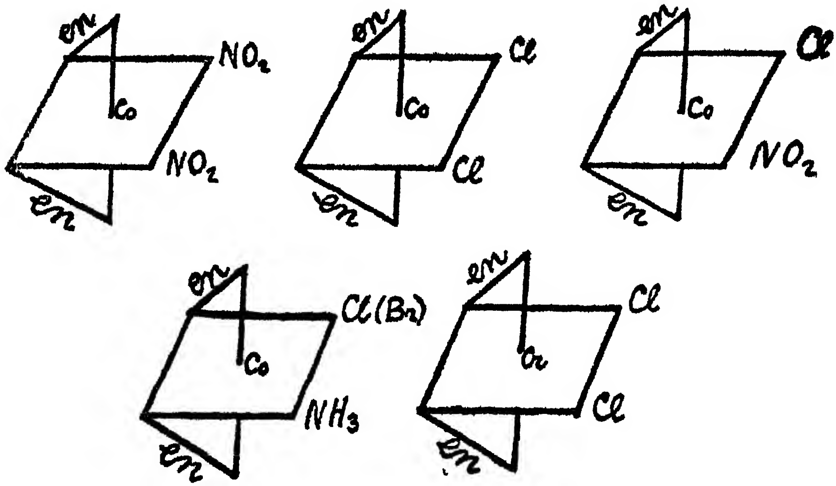
Triethylene diamine chromic chloride (Cr $\mathrm{en_3}$] $\mathrm{Cl_3}$ and triethylene diamine rhodium chloride (Rh $\mathrm{en_3]Cl_3}$ were similarly resolved, and recently a compound of platinum [Pt $\mathrm{en_3]X_4}$ has been added to the list.

The fact that the optical activity of these compounds depends neither upon the nature nor the valency of the central atom nor upon the ethylene diamine, but upon the enantiomorphous configuration of the molecule, was established by a similar resolution of the tri-α-α-dipyridyl ferro-compounds [Fe $\mathrm{Dipyr_3]X_3}$, in which the base $\alpha\alpha$ dipyridyl  is combined with ferrous iron.

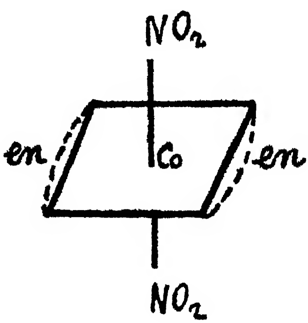
Many examples of the second type containing two molecules

[1] The symbol en = $\mathrm{NH_2 \cdot CH_2 \cdot CH_2 \cdot NH_2}$, is used to simplify the formulæ.

of ethylene diamine have also been resolved by Werner. These indeed were the compounds first examined by him, and conform strictly to the theory, the cis compounds being all resolvable, whilst the trans compounds are incapable of resolution. Among the cis compounds which have been resolved, generally by means of their camphor or d. bromo camphor sulphonates, may be mentioned the 3.4. dinitro, the 3.4 dichloro, and the 3 chloro, 4 nitro ethylene diamine cobaltic salts, the 3 bromo and 3 chloro 4 amine diethylene diamine cobaltic salts, and the 3.4 dichloro diethylene diamine chromic salts of the formulæ



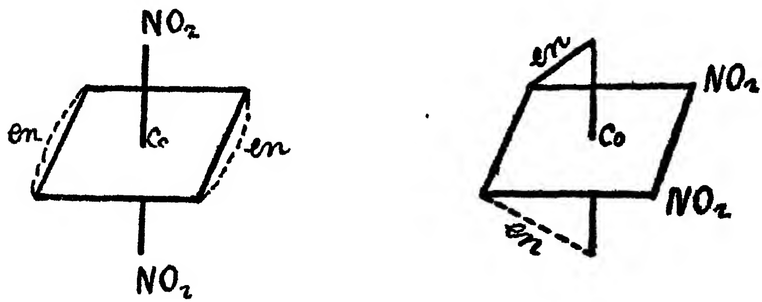
It may be noted that all attempts to resolve the trans 1–6 dinitro diethylene diamine cobaltic salt of the constitution



which, according to the theory, should be irresolvable, have failed, although a splendidly crystalline camphor sulphonate of the base can be obtained.

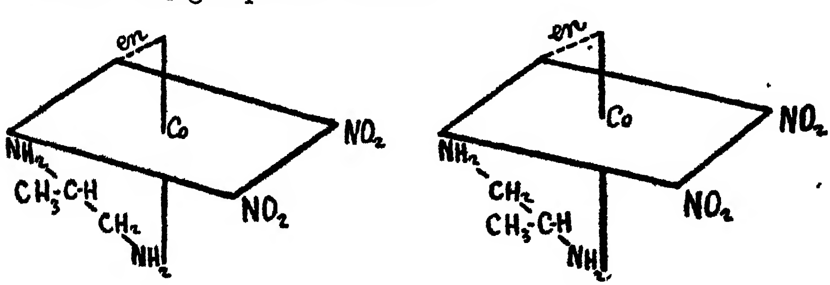
The conception of the octahedral structure of these complex compounds leads to many further conclusions, some of the most far-reaching of which have lately received at Werner's hands a most remarkable experimental verification.

For example, in the case of those cobaltic compounds of the constitution $[(NO_2)_2 Co\ en_2)X$ which have already been referred to two series are known :



Trans (Croceo-series). Cis (Flavo-series).

If one of the ethylene diamine molecules in these compounds could be replaced by a similar but unsymmetrically constituted diamine, as, for example, propylene diamine $NH_2 \cdot CH_2 \cdot CH\ (CH_3) \cdot NH_2 (= pn)$ trans and cis modifications should again be possible, whilst the latter should be capable of existing in two structurally different forms in which the methyl group is near to or further removed from the plane in which the two NO_2 groups are situated.



These predictions have been put to the test of experiment and confirmed; croceo and flavo salts have been obtained, and the latter have been found to exist in two forms, one termed the α or prismatic, crystallising in prisms, the second called the β or needle-shaped, crystallising in needles.

Again, as propylene diamine contains an asymmetric car-

bon atom and consequently can be obtained in optically active d and l modifications, if in the preparation of the compounds $\left[(NO_2)_2\,Co\,\begin{matrix}en\\pn\end{matrix}\right]X$ the oppositely active bases are employed, the theory predicts the possible existence of numerous optical isomerides.

For example, the trans isomeride (croceo salt) of the formula $\left[\begin{matrix}1. & NO_2\\6, & NO_2\end{matrix}\,Co\,\begin{matrix}en\\pn\end{matrix}\right]X$ should exist in three forms,

$$\left.\begin{matrix}\text{the d-trans compound}\\\text{the l-trans compound}\end{matrix}\right\}\text{the racemic trans compound,}$$

whilst eight optically active modifications of the two series of cis-isomerides (flavo salts) of the constitution $\left[\begin{matrix}3. & NO_2\\4. & NO_2\end{matrix}\,Co\,\begin{matrix}en\\pn\end{matrix}\right]X$ are theoretically possible due to different spatial positions of the methyl group in complexes containing the two connecting diamine chains differently orientated as previously described, namely:

In the *α*, or prismatic series:

$$\text{d. propylene diamine}\left\{\begin{matrix}\text{d. cobalt}\\\text{l. cobalt}\end{matrix}\right.$$

$$\text{l. propylene diamine}\left\{\begin{matrix}\text{d. cobalt}\\\text{l. cobalt}\end{matrix}\right.$$

In the *β*, or needle series:

$$\text{d. propylene diamine}\left\{\begin{matrix}\text{d. cobalt}\\\text{l. cobalt}\end{matrix}\right.$$

$$\text{l. propylene diamine}\left\{\begin{matrix}\text{d. cobalt}\\\text{l. cobalt}\end{matrix}\right.$$

These eight optically active compounds should be able to combine in different ways to give eight partially racemic and two completely racemic compounds which may be summarised as follows:

Four compounds which are racemic with respect to cobalt.

In the *α* or prismatic series:

1. { d. propylene diamine - d. cobalt + d. propylene diamine - l. cobalt.
   l. propylene diamine - d. cobalt + l. propylene diamine - l. cobalt.

And in the β, or needle series :

2. $\begin{cases} \text{d. propylene diamine - d. cobalt + d. propylene diamine - l. cobalt.} \\ \text{l. propylene diamine - d. cobalt + l. propylene diamine - l. cobalt.} \end{cases}$

Four compounds which are racemic to propylene diamine.

In the α, or prismatic series :

3. $\begin{cases} \text{d. cobalt - d. propylene diamine + d. cobalt - l. propylene diamine.} \\ \text{l. cobalt - d. propylene diamine + l. cobalt - l. propylene diamine.} \end{cases}$

And in the β, or needle series :

4. $\begin{cases} \text{d. cobalt - d. propylene diamine + d. cobalt - l. propylene diamine.} \\ \text{l. cobalt - d. propylene diamine + l. cobalt - l. propylene diamine.} \end{cases}$

Finally, two completely racemic and inactive compounds of the constitution

d. Pn - d. Co + d. Pn - l. Co + l. Pn - d. Co + l. Pn - l. Co

one in series α and one in series β, should be obtained by combining in equimolecular proportions the pairs of oppositely active partially racemic compounds included in brackets either under 1 or 3 or under 2 or 4.

All these predictions have been confirmed by actual experiment, a result which, since optical activity is unquestionably connected with molecular enantiomorphism, gives strong support to those theories of constitution by which Werner first brought into ordered arrangement one of the most complicated and perplexing groups of compounds known to chemists.

The new light which Werner's work has thrown on the problems of molecular structure may be well-illustrated by a brief consideration of the constitution of a well-known double salt such as potassium chromium oxalate $K_3Cr(C_2O_4)_3 \cdot 3H_2O$. Whilst satisfactory formulæ for the simple constituent oxalates are obtained by replacing the hydrogen in the formula of the acid by an equivalent quantity of the metal a difficulty arises in assigning a constitution to the double salt. How are the simple salts and the water of crystallisation severally combined ?

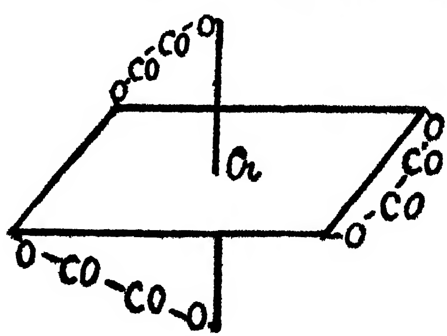
A few years ago no explanation was ever offered, the formula was written as above, or sometimes

$$3K_2C_2O_4 \cdot 2Cr_2(C_2O_4)_3 \cdot 6H_2O.$$

which, if it expresses anything beyond the ultimate composition, implies that the double salt is a loose aggregation of its three components. This, which may or may not be true with regard to the water of crystallisation, is certainly incorrect and misleading with regard to the other constituents, for in solution the salt does not show the characteristic behaviour either of a chromium salt or of an oxalate, whilst on the other hand it does give the characteristic reactions of a potassium salt.

With the advent of the electrolytic dissociation theory the formula acquired the ionic form, $K_3^{+++} \cdot Cr(C_2O_4)_3^{---}$ indicating in solution the presence of a negatively charged chromi-oxalate ion. This is a distinct advance, but it leaves untouched the question of the relationship of its parts.

Werner's views on the constitution of such complexes first suggested a structure, the consequences of which could be put to the test of experiment. If, as seemed likely from its behaviour, the whole and not one-half of the oxalate residues were united to chromium, the simplest arrangement would be octahedral; if octahedral, the molecular configuration would be enantiomorphous, if enantiomorphous two forms should be separable and oppositely active. Experiment established the correctness of these conclusions, and the constitution of the complex ion can be regarded as settled in favour of the following orientation in space of the oxalic chains :



This marks a substantial advance, although two parts of the problem still await solution, the constitutional as opposed to

the electrical relationship of the three atoms of potassium to the nucleus and the nature of the chemical forces which determine the inclusion of molecules of water in the crystals.

In this, as in many other instances, Werner has replaced vague and misleading ideas by clear and definite ones ; he has also opened up a new field of enquiry which seems likely to lead to a knowledge of the constitution of inorganic compounds comparable with our present-day knowledge of organic structure.

THE IMPACT TESTING OF METALS

By F. C. THOMPSON, D.Met., B.Sc.,
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The introduction of rapidly moving and severely stressed parts in engineering design, especially, perhaps, in motor-car and aeroplane contruction, led to a search on the part of engineers for a test in which rapidly applied stresses were involved which should supplement the ordinary tensile test where the rate of loading is relatively slow. The work of Eaton Hodgkinson (1847) and Wöhler (1859) paved the way for the so-called "fatigue" tests in which fracture of the sample is brought about by the rapid repetition of stresses often considerably below the elastic limit as determined in static tension. Many types of such test, which for some time enjoyed considerable popularity, have been devised, but in recent years, and especially during the war, in connection with the inspection of material for air-craft construction, the fashion has changed. The "impact" test, in which fracture of the test-piece is effected by a single sudden blow, has replaced the fatigue test, in some forms of which millions of repetitions of comparatively small stresses were required.

The literature of the subject is now somewhat voluminous, and an interesting bibliography is to be found in a paper by Dr. W. H. Hatfield (*Proc. Inst. Mech. Eng.*, 1919, p. 347).

The earlier work on impact testing revealed most exasperating discrepancies in the resistance of presumably the same material to sudden and severe stresses. For several years a controversy was carried on between those who claimed that a form of test which yielded such discordant results on samples which, so far as the usual methods of testing were concerned, were identical, was not worthy of credence, and those who saw in it a method so sensitive that variations shown by no other process were revealed. The latter view has been shown beyond doubt to be correct. Especially as a result of the work of Charpy and Cornu-Thenard (*Journ. Iron and Steel Inst.*, 1917, ii, p. 61) it has been demonstrated that, given really uniform material,

results agreeing within 1 or 2 per cent. can be obtained. It is, therefore, to variations in the steel under test that the apparent discrepancies observed are to be ascribed, this form of test now standing out as probably the most sensitive of all those used in the testing of materials of construction. The impact test has thus at last come into its own, and both as a method of research in determining the best treatment for any given steel, and as a method of routine inspection, has been widely adopted.

It has long been known that the special sensitiveness of this test was not displayed where plain bars of metal were employed; from some form of notched test-piece only was the full information obtainable to be procured. Many varieties of notch have been suggested and used, but the fairly general consensus of opinion is now that a fairly sharp notch is best. When comparing the V notch with the "Copenhagen" type, in which a straight cut ends in a cylindrical hole of relatively large diameter—2 mm.—the superior sensitiveness of the former in picking out brittle material is clearly shown. Since this constitutes the whole *raison d'être* of the test, the marked preference for the sharp notch is obviously justified. Although the radius at the base of the V should be as constant as possible (0·25 mm.), slight variations in this do not make sufficient difference to invalidate the test. The Mesnager notch, much used in France is 2 mm. deep, ending in a semicircular groove of 1 mm. radius. The work of Philpot (*Trans. Inst. Ant. Eng.*, April 1918) has shown that results of the same order as those obtained from impact tests are given by slow bending tests on similarly notched bars, and it would, therefore, appear that it is to the notched test-piece rather than the suddenness of the blow that the remarkable results are to be referred. Since, however, the notched bar test is more rapidly carried out in its impact form, this is the one which finds the chief application. To the subject of the notch repeated reference will be made below.

Concerning the types of machine designed for conducting impact tests, two distinct forms are in use, in which fracture is brought about by single and repeated blows respectively. The latter form of the test, of which the apparatus due to Dr. Stanton is the only one which need be referred to, occupies a position midway between the "fatigue" tests and the impact test proper. The test-piece, $\frac{1}{2}$ inch in diameter and $6\frac{1}{2}$ inches long, is placed on supports $4\frac{1}{2}$ inches apart. A groove, which in the form

supplied by the Cambridge Scientific Instrument Co. is 0·05 inch deep and 0·05 inch wide, is made round the centre of the bar, the corners of the notch being practically square. This bar receives blows from a hammer weighing 4·7 lb., the height of fall of which can be varied. The test-piece is rotated through 180° after each blow, about 100 of which are given per minute. The number of blows endured before fracture occurs at the groove is recorded. In the paper by Hatfield to which reference has already been made, results are given which reveal a very close connection between the resistance of steels to the Stanton test and their true elastic limits. Something much more than this is required if the impact test is to justify its existence, with the result that this modification of the test finds but limited application, the single-blow variety being far more commonly employed. The results obtained from these two types of impact tests are often widely discordant, materials which, according to one test, show marked resistance to impact making a poor show when tested by the other.[1]

In all the machines in common use for conducting impact tests a bend test is employed. Other types have been suggested, but have found little or no support. Stanton and Bairstow (*Proc. Inst. Mech. Eng.*, 1908, p. 889) have described and figured an apparatus designed for carrying out impact tensile tests. Four forms of impact bend test are in fairly common use. The two best known of these, the Izod and Charpy types, both employ a falling pendulum to strike the blow. The Frémont machine has a vertically-falling weight, while in the Guillery apparatus a rapidly revolving fly-wheel is used to impart the blow. The essentials of each form are as follows:

In the Izod test the piece, 10 mm. square with a V notch 2 mm. deep at an angle of 45°, is clamped vertically with the notch on a level with the face of the die and facing the pendulum. This is released by a spring attachment from a given height, and at the lowest point of its swing a knife edge which it carries strikes and breaks the test-piece. The residual energy is measured by a pointer which is moved over a graduated scale as the pendulum rises. The height of fall of the centre of mass of the pendulum is 2·5 feet, the striking velocity 13·6 foot-seconds.

[1] A considerable amount of information concerning the relationship of the Stanton to other tests will be found in the *Journ. Iron and Steel Inst.*, *Carnegie*, vol. vi, p. 94.

The Charpy test differs from the foregoing chiefly in that the test-piece is loosely supported at both ends, the distance between the supports being 40 mm., and that the notch is placed on the opposite side of the test-piece to that on which the blow is received. Although, of course, a notch identical with that used in the Izod test can be and often is used, the specified form consists of a cut 5 mm. deep, terminating in a cylindrical hole 1⅓ mm. in diameter. The knife edge which comes in contact with the test-piece is here placed vertically in the pendulum; in the Izod machine it is horizontal. In the machine of the usual size the weight of the tup is 22·5 kilograms, the striking energy being 30 kilogram-metres.

In the Frémont test the blow is received from a tup which has fallen vertically through a height of several metres, the striking velocity being higher than in either the Izod or Charpy forms. The test-piece, 8 mm. by 10 mm., rests horizontally on supports 21 mm. apart, with a square notch 1 mm. deep and 1 mm. wide on the longer lower side.

The standardisation of the impact test is, as a result of the work of Charpy and Cornu-Thenard, now fairly complete. Despite the differences in the types of apparatus described, the results obtained from similar samples with the same form of notch on all of the machines are practically the same. Not only is the type of machine without influence, but so also is the energy of the blow, provided that this is ample to produce fracture. The work done in breaking the test-piece is, therefore, a definite characteristic of the material.

The results of the test are commonly expressed in terms of the difference in energy of the tup before and after fracturing the test-piece. As Mr. H. Brearley has pointed out, the work done upon the material is expended in the first place in starting the crack at the root of the notch, secondly in extending the crack across the bar, and thirdly in deforming the material in the neighbourhood of the crack. In view of this complexity, it is not surprising that no satisfactory general connection has so far been discovered between the dimensions of the bar and the work done in fracturing it in impact. All that it is at present possible to do is to record the work expended in breaking a clearly specified but quite arbitrary type of test-piece.

The unfortunately fairly common practice of expressing these results in such terms as kilogram-metres per square centimetre

is most unsatisfactory. The energy used up in breaking, say, an Izod test-piece 0·8 sq. cm. in cross-section at the notch, when multiplied by 1·25, does not give the energy required to fracture a test-piece whose dimensions are 1 cm. × 1 cm. at the base of the V. It has been suggested by Mimey (*Revue d'Artillerie*, July 1911) that if T_1 and T_2 are the energies absorbed in fracturing two geometrically similar bars of the same material, then $\frac{T_1}{T_2}$ will be proportional to the cube of the homologous dimension, provided that

$$\frac{M_1}{M_2} = \frac{\sqrt{H_1}}{\sqrt{H_2}} = \frac{D_1}{D_2} = \frac{\sqrt[3]{T_1}}{\sqrt[3]{T_2}}$$

where M_1 and M_2 are the masses of the weights, H_1 and H_2 the distance fallen, and D_1 and D_2 the distances apart of the supports for the test-piece. The result is not, however, in agreement with experiments, except where deformation does not proceed as far as fracture and in the case of unnotched bars. For purposes of reducing the results obtained in ordinary impact tests on notched bars, it is useless. In the case of a bar of side x and notched with a standard groove, the total work done in producing fracture would require a formula of three terms of the form $\alpha x + \beta x^2 + \gamma x^3$. The first term corresponds to the energy expended in starting the crack along the line of the notch, the second to that done in carrying it across the section of the test-piece, and the third to that used up in deforming adjacent material. Since it is often comparatively easy to continue the crack when once it is formed, the importance of the first time—neglected by Charpy and Cornu-Thenard—is evident. The relative part which each of these factors plays will vary considerably in different materials. In, for instance, a hardened steel the resistance to deformation is very great. A crack, therefore, once started extends with ease and without appreciably deforming the surrounding material. A low impact value will thus be recorded. In the case, however, of a metal or alloy with a fairly low yield-point, it may not be possible either to start or to propagate the crack without considerable distortion of the surrounding material. In this distortion considerable energy is used up, with the result that a high impact figure will be given. It is not, therefore, possible to draw any hard-and-fast line, and say that above such and

such a value a material is tough, and below that value brittle. For a steel with a maximum stress of 100 tons per square inch, an impact value of 30 foot-pounds would be extraordinarily good; if the same figure were associated with a tenacity of 60 tons per square inch it would be moderate, while for a 20 or 30 ton steel it would be distinctly poor.

In a crystalline aggregate, as all metallic test-pieces are, both the crystalline portion and the intercrystalline surfaces make their influence felt. As pointed out by Mr. H. Brearley, brittle materials, which, like the hardened steel, yield low impact figures, break between the adjacent crystals, which are hardly affected at all. The fractured surface of a brittle test-piece is strikingly crystalline. In the case, however, of a tough sample in which the intercrystalline strength exceeds that of the crystalline portion the reverse of the foregoing conditions obtains. The crack must pass through the crystals themselves; in so doing, considerable work is done on the surrounding parts, and a fibrous or silky fracture is shown. The transition of a hardened steel from the brittle to the tough condition on tempering is in part at least due to the softening of the crystalline material, rather than to the increased strength of the intercrystalline areas; and in determining at what temperature any given steel should be tempered to yield the toughest product, the impact test gives the highest results when tempering is carried out 50–100° C. below the carbon change-point. To the impact test, therefore, a hard material composed of large crystals is typically brittle, a more finely crystalline and softer one tough.

In the case of a forged material, such as steel, the resistance to impact is closely bound up with the direction in which the test-piece itself is cut A sample cut longitudinally will yield far better results than one cut transversely to the direction of forging. The following results (*Journ. Iron and Steel Inst.*, 1918, ii, p. 24) will illustrate this influence:

Angle of Notch with Direction of Rolling.	Work absorbed on Rupture (Kilogram-metres).
0° [1]	1·3
20°	1·5
45°	3·4
90°	13·5

[1] The notch being perpendicular to the test-piece, this sample is cut transversely to the direction of rolling.

The explanation of these differences lies in the alinement of brittle impurities in the steel as a result of the rolling. Sulphur and phosphorus, present as manganese sulphide and as a solid solution of iron phosphide respectively, are present in long-drawn-out threads elongated in the direction of the forging.

The presence of slag in the steel, which will also be drawn out in the direction of the rolling, deleterious as it is from nearly every point of view, may yet exert a considerable influence in raising the resistance of the material to the impact test, provided that the test-piece be cut in a longitudinal direction. As the crack formed reaches a slag inclusion, which is perpendicular to the direction in which it is growing, a tendency exists for a change of direction, the crack travelling along the line of the slag. Oriented in this way, slag fibres give an increased resistance to impact. If, however, as in a transverse test-piece, the slag lies parallel to the crack, it merely greatly facilitates its passage through the material, very low impact values being obtained. This point is very clearly dealt with by Mr. H. Brearley (*Journ. Manchester Association of Engineers*, 1917, pp. 492 *et seq.*).

The greatest triumph of the impact test has been the revelation of a new and quite unexpected type of brittleness to which certain alloy steels, especially those containing nickel and chromium, were liable. Tempered at temperatures around 600–650°C., and slowly cooled, the resistance of the steel to impact may be almost non-existent, while when rapidly cooled from the tempering temperature, the resistance is excellent. This phenomenon, usually known as "temper-brittleness," is frequently not revealed at all by the tensile test, as the following figures, due to Mr. Brearley (*loc. cit.*), clearly show:

		A	B
Yield-point	*tons per sq. in.*	45·0	45·6
Maximum stress	" " "	53·7	52·4
Elongation	*per cent.*	21·0	21·0
Reduction of area	" "	55·8	59·3
Impact value	*ft.-lbs.*	2·5	76·5

The two sets of tests were carried out on the same material similarly treated, except that sample A was slowly cooled in the furnace after tempering, B being quenched in water. Although the tensile test is to all intents and purposes unaffected, the

resistance to impact of A is only 3 per cent. of that of B. Steels of certain compositions show the brittleness fully ; the rate of cooling from the tempering temperature is the essential point ; a steel which has become tough or brittle may by appropriate re-treatment be rendered the reverse; no structural change has so far been observed between the brittle and tough states; and finally certain casts of steel can never be rendered tough or others brittle. No explanation fully covering the facts has yet been given.

THE CAPILLARY CIRCULATION

By W. M. BAYLISS, F.R.S., M.A., D.Sc.,
Professor of General Physiology, University College, London

Although the part played by the small arteries, arterioles as they are usually called, in regulating the height of the blood-pressure and the supply of blood to the tissues of the various organs is fairly well known, that of the capillaries and the veins remains more obscure. The fact that the arterioles possess a muscular coat of relatively considerable thickness, while the capillaries consist merely of a single layer of epithelial cells, renders the mechanism of contraction and dilatation of the former easy to realise. Their nervous supply has also been worked out in some detail, and the facts of their active changes in calibre under the stimulation of various nerves and under the influence of different kinds of drugs and other chemical substances are generally admitted.

The veins also possess a muscular coat, but of much less thickness than that of the arterioles. Some evidence exists that they are capable of changes in calibre; but the object of the present article is to consider rather the facts that have come to light in recent times with regard to the properties of the capillaries, together with the consequences, theoretical and practical, that follow from them.

The difficulties in supposing that the capillary blood-vessels have the power of altering their calibre independently of the pressure of the blood entering them appear to be largely due to the absence of a muscular coat. On the other hand, a fairly copious supply of nerve fibres to them has been described, and we know that cells other than muscle cells can change their form when stimulated. The pigment cells of the skin in fish and frogs may be mentioned, and the spherical forms assumed by the amœba and the leucocytes are familiar. Thus the absence of a muscular coat in the case of the capillaries does not warrant a denial of the possibility of active changes in calibre in the latter.

If the reader will examine, under a low power of the microscope, the circulation in the web of the frog's foot—and there is probably no more interesting thing in the whole of physiology—he will notice how great is the volume of the capillaries compared with that of the veins and arteries. He will see that the rate of the flow of blood in the arterioles is, accordingly, very much greater than that in the capillaries; and, remembering that the friction is proportional to the velocity, he will realise the great effect that changes in diameter of the arterioles have on the pressure on the arterial side of the system. But he will also appreciate how large a volume of blood can be contained in the capillary region, and that a comparatively small increase in the diameter of the capillaries, if present in a large part of the body, will suck up, as it were, a great proportion of the blood present in the circulation. The importance of this fact is that the amount flowing into the heart from the great veins will be less, and therefore also that driven out into the arteries on contraction of the ventricles. Hence there will be a fall in blood-pressure and deficient supply to the tissues generally. Thus, although the capillaries may be wider, the current through them will be diminished by the decrease in driving pressure, in addition to the lower rate of flow involved in their increased width. Under ordinary circumstances, as described by Lister and by Langley, some of the capillaries are more or less empty of blood, and become filled only when dilatation occurs. This is an additional way in which blood is accumulated in capillary networks. This fact has recently been further investigated by Krogh, who shows that, in resting muscle, only a small number of the capillaries are filled with blood. In activity, a greater or less proportion of the remainder becomes dilated and conveys a current of blood. According to his experiments, it requires a fairly high pressure to open up the collapsed capillaries; so that, if an increased supply of blood is needed by an organ, it is necessary that an active dilatation of the capillaries should occur, as well as one of the arterioles. It appears that, if the latter alone were to take place, a comparatively small increase in blood-supply would be provided. If the capillaries alone dilate, there is no increase in total supply, or very little, and the blood which continues to flow will be greatly reduced in rate with a tendency to stagnation, with rapid loss of its oxygen. This con-

dition will be exaggerated if the arterioles simultaneously constrict, as happens under the action of histamine.

That the capillaries are not merely passively distended or depleted according to the diameter of the arterioles supplying them is indicated by the common experience of two different effects of external cold on the skin. The colour of the skin in white races is almost entirely due to the blood in the capillaries. When the blood-supply is cut off, the skin becomes white and cold. This may sometimes happen as the result of the constricting effect of cold on the arterioles; but there are also two familiar states produced by cold which are associated with increased depth of colour, and therefore with a greater content in blood. In one of these, the skin is red and warm; in the other, on the contrary, it is blue and cold. It is clear that the warmth in the former case must be due to an increase in the current of warm blood flowing through the capillaries, and this can only be brought about by dilatation of the arterioles and capillaries.' The capillaries are thus filled with a rapidly renewed current of fresh warm blood. In this way, the skin is protected from the deleterious effects of cold. A compromise must be effected between the increased loss of heat from the body and the protection of the skin. It will be remembered that one of the means of getting rid of excessive heat is by dilatation of the vessels in the skin. What is the state of affairs when the skin is blue? Bearing in mind the fact that the colour of the blood in the veins appears to be blue when looked at through the skin, we realise that this is the colour assumed when oxygen has been lost. The blue colour of the blood in the capillaries must be due, then, to its having lost more oxygen than it does when the skin is red. Although the capillaries are dilated, the current through them must be decreased, and this state can only be explained by an expansion of the capillaries independent of that of the arterioles, which are probably constricted. The coldness of the skin is readily explained on the basis of the slow circulation through it. An exaggeration of this blue state of the skin may be observed in certain pathological conditions, and appears to be easily brought about by exposure to cold in such cases.

With regard to more definite experimental evidence of active change in the capillaries, we may note the difficulty of

excluding passive effects due to changes in the arterioles. A dilatation in these vessels would raise the pressure at the place where they become capillaries, and naturally distend these to some extent. Lister in 1858 described dilatation of the capillaries in the frog's web under the action of chloroform and other agents. Roy and Graham Brown in 1880 confirmed this observation, and believed that they had excluded the effect of the simultaneous dilatation of arterioles by the fact that if reflex stopping of the heart was brought about, so that the arterial pressure fell to zero, the dilated capillaries did not empty. This, however, is not completely convincing, because the elastic reaction of the capillaries might not be sufficient to empty them after they had been stretched. If the passive stretching had caused an elastic reaction, it is difficult to see why the capillaries did not empty themselves back into the arterioles where the pressure was zero. Better evidence of independent action on the part of the capillaries is afforded by the observation of these investigators that the diameter of different capillaries is not in direct proportion to the arterial pressure. Two capillaries lying side by side may require very different external pressure to obliterate them, and, after a pause, that one which previously collapsed under the lower pressure may now require the higher one.

In 1893, Worm-Müller had shown that large quantities of blood could be injected into dogs without much rise of blood-pressure. The blood remained in the circulation, and post-mortem observations did not reveal a distension of the arteries and veins of sufficient degree to accommodate it. The conclusion was drawn that it was accumulated in the capillaries of the body generally. The adjustment is doubtless brought about by a nervous reflex. It may be that a sufficient explanation lies in dilatation of the arterioles and the resulting passive distension of the capillaries. If this be not accepted, and Krogh's results cast doubt on the acceptability of the explanation, we must assume a direct nervous control of the capillaries by vaso-dilator nerves.

Severini described experiments on the empty capillaries in excised tissues in which oxygen caused narrowing, carbon dioxide widening; but Roy and Graham Brown failed to confirm the observations.

Other observations on capillary dilatation might be quoted,

but it will be seen that, so far, satisfactory experimental evidence of an independent activity on the part of the capillaries is wanting. This was not afforded until the experiments of Dale and Richards in 1918. Dale and Laidlaw in 1910 were struck by the puzzling action on the blood-pressure of a base, histamine, obtained by removal of carbon dioxide from the amino-acid, histidine, a constituent of most proteins. In the dog, cat, and monkey, a marked fall of blood-pressure is produced by very small doses. Now, in the case of certain other drugs having this effect, depression of the heart being excluded, it was known that dilatation of the arterioles was produced, and the explanation of the fall of blood-pressure was easily explained by decrease of peripheral resistance. But tests of the action of histamine on various kinds of smooth muscle, including that of the arteries, showed that it caused *contraction*, and therefore narrowing of the arterioles, an effect which by itself alone would produce *rise* of blood-pressure. By a number of ingenious experiments, the details of which cannot be given here, Dale and Richards showed that histamine causes a widespread dilatation of the capillaries, independent of its effect on the arterioles, which are constricted. It is evident, therefore, that the capillary effect is great enough to overpower that of the arterioles. How is the fall of blood-pressure produced? It seems improbable that a dilatation in such a wide bed as the capillary district, even in normal conditions, should have any marked effect in decreasing the peripheral resistance, especially when the arterial constriction has reduced the blood flow to a low degree of magnitude. The result must be due to removal of blood from actual circulation, an interpretation confirmed by the examination of the heart, which is seen to be empty of blood. In fact, the blood, soaked up by the capillaries as by a sponge, takes no more part in the general supply of the organs with fresh oxygenated blood than if lost by hæmorrhage. In this work, the fall of blood-pressure caused by very small doses of adrenaline in certain conditions was shown to be due to its dilator action on the capillaries.

The practical interest of these experiments lies in the fact that histamine is one representative of a group of substances obtained by disintegration of the protein molecule. In the state of "shock" brought on by extensive injury to tissues,

especially to muscle, whether by shell wounds or by surgical operations, observations made at the casualty clearing stations in the late war showed that a state of the circulation similar to that produced by histamine was present. The obvious connexion between severe shock and massive injury suggested experimental test. Col. Cannon, of Harvard University, and the present writer found that injury of the thigh muscles in anæsthetised cats and dogs brought on a condition like that of wound shock, and we were able to show that it was not due to irritation of nerves, but to the passage into the blood of some chemical product of the injured tissue. The fact that the state, whether accompanied by hæmorrhage or not, owes its dangerous character to the want of blood in circulation, pointed to the treatment which alone was found successful—namely, the filling up of the circulation by transfusion of blood or of some appropriate artificial solution, such as gum arabic in saline.

In regard to the nervous supply of the capillaries, some recent observations by Krogh are of importance. He shows that it is possible, by touching with a glass needle over the situation of a closed capillary in the frog's tongue, to make this vessel dilate, and that the extent to which the dilatation spreads depends on the strength of the stimulus. A similar local effect can be obtained on an arteriole. Degeneration of the nerves to the tongue or the application of cocaine very nearly abolishes the effect, and what remains is sharply localised to the point stimulated. The conclusion is drawn that the spreading of the effect is due to an axon-reflex in sensory fibres, analogous to that suggested by myself (1901, p. 196) for the case of the vaso-dilatation produced by stimulation of the peripheral ends of sensory nerve fibres.

There remains for brief discussion another aspect of the capillary circulation which has important physiological and pathological relations. In the shock produced by histamine, and also in that of wounds in man, it was found that not only was the blood in effective circulation diminished, but what was in circulation had become more or less concentrated by loss of plasma. To understand the cause of this phenomenon, we must consider the evidence as to the permeability of the capillary wall. It is a familiar fact that, when blood is lost, fluid is taken up from that in tissue spaces in order to restore

the volume of the blood as far as possible. What is the nature of this fluid? It was shown by Scott that, although the lymph outside the blood-vessels contains proteins, that which enters the blood is devoid of them, consisting only of a solution of the salts and diffusible substances of small molecular size, such as glucose and amino-acids. In other words, the normal capillary wall is impermeable to proteins. If, then the proteins of the blood possess an osmotic pressure, this will be manifest in a tendency to absorb water from the outside. Starling showed that they have such an osmotic pressure, and that it reaches the value of about 35 mm. of mercury. This may be regarded as the force tending to draw water into the blood. But on the arterial side of the circulation, the pressure inside the blood-vessels is over 150 mm. of mercury. This is opposed to the osmotic pressure, and the excess pressure causes the filtration of fluid (= lymph) outwards. Following the blood-pressure as it falls in its course from the arterioles to the capillaries, we find that the pressure inside the latter only amounts to some 10 mm. of mercury. This is less than the osmotic pressure of the proteins, and water is absorbed from the outside. The two processes, however, do not completely compensate one another, and a certain quantity of the lymph is drained off by the lymphatic vessels, being ultimately returned to the blood by the thoracic duct. When we inject into the veins a solution containing salts or glucose only, we decrease the osmotic pressure of the proteins by dilution, more pressure is available for filtration, while the place where reabsorption begins is pushed farther towards the veins, so that a diminished area is in activity. The result is that the fluid injected leaves the circulation rapidly, and the blood returns to its diminished volume. This agrees with clinical experience. If, however, we add to our injection fluid a colloid possessing an osmotic pressure of the correct value, there is no tendency to loss by increased filtration, and if the permeability of the capillaries is normal the increased blood volume is permanent. The present writer has shown that the addition of 6 or 7 per cent. of gum arabic to the 0·9 per cent. sodium chloride serves this purpose, and such a solution was in extensive use during the later period of the war.

Incidentally, it may be remarked that the filtration of protein-free liquid from the blood-vessels owing to the pres-

sure therein is the view most generally accepted as the first stage of the production of urine.

But there is another way in which increased exudation from capillaries may be brought about. Suppose that the walls become permeable, not only to salts, but also to colloids. There is then no osmotic force to oppose filtration, because the osmotic pressure of the proteins cannot be effective. Now, such an effect is produced by histamine and other products of tissue destruction. If it has reached a high value by prolonged action of the toxic substance, it cannot be restored to normal. Accordingly, in late and severe stages of wound shock, neither blood nor intravenous gum-saline is effective. The liquid escapes from the blood-vessels in a comparatively short time and the blood-pressure falls to its former low level. The occurrence of this condition is shown by the coincidence of a low blood-pressure with a high hæmoglobin content of the blood, indicating loss of fluid, whereas normally the low pressure would be associated with dilution. This latter was, in fact, recognised as a favourable sign. If the change in permeability has not become too great, the introduction of blood or gum-saline may restore the normal state (see N. M. Keith).

A similar action to that of histamine on the permeability of the capillaries appears to be exerted by prolonged deficiency of blood-supply, probably owing to want of oxygen. The low pressure of shock in itself exaggerates the direct effect of the tissue toxins.

In certain regions of the body, such as the liver, and to some extent the intestines, the capillaries are normally more permeable to proteins than in other regions, and lymph is accordingly more abundantly formed in such regions. It also contains a certain amount of protein. But, of course, the pressure in the portal vein is much lower than that in the arteries, and the lymph produced also ultimately finds its way back into the blood.

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CORRESPONDENCE

TO THE EDITOR OF "SCIENCE PROGRESS"

A NATIONAL SCIENCE LIBRARY

FROM W. R. B. PRIDEAUX, B.A.

DEAR SIR,—I should like to call Dr. Slade's attention to the work for students which is actually being done by the Library Association, and further work which is in contemplation, and dependent only on the amount of support it receives from research workers of all kinds.

The Library Association issues an Index to Periodicals which covers over 500 journals, and which could be expanded still further if adequate support were forthcoming. Subscribers to the 1915 and 1916 volumes have had the right, for a small fee, to borrow any article notice of which appears in the Index, and thus the opportunity was given to workers in any part of the country to keep themselves abreast of their specialty in their own homes at a trifling cost. These facilities for the loan of articles have hitherto remained a dead letter—most probably because they have not been sufficiently widely known. Difficulties of storage may make it impossible in the future to lend the journals (though the loan scheme is still in operation), but in connection with the forthcoming issue of the Index (to cover the years 1917, 1918, and the first half of 1919) the Association hopes to establish an information bureau from which inquirers could obtain, for a small fee, lists of references on any topic, right down to the date of the inquiry. The scheme is perfectly practicable, and only needs sufficient support.

The Library Association would always be glad of helpful criticisms and suggestions, which may be addressed to me. I may add that the Index is not run for a profit, and is a co-operative work. At present the heavy expenses of printing

and publishing are not covered, so that more subscribers are urgently required if the work is to be continued and developed.

Yours faithfully,

W. R. B. Prideaux, B.A.,

Librarian of the Reform Club, London;

General Editor of the "Index to Periodicals."

THE INTELLIGENT RAT

From Enoch Karrer

Department of Commerce, Bureau of Standards, Washington

Dear Sir,—In the last issue of Science Progress there appears an article on "The Mathematical Amœba," bearing upon animal intelligence. Will the following observations be of sufficient interest to you for publication?

Observation on the behaviour of the rat, indicating that the rat may learn by experience.

The incident here recorded occurred in the chemical room of the Physical Laboratory of the United Gas Improvement of Philadelphia, in the fall of 1915. The first record made of it was on October 24, 1915.

There is reason to believe that only one rat is concerned here; for, after one rat was trapped, there were no further evidences of the presence of rats.

This particular rat had been carrying away the half-bars of "Ivory Soap" to the shelves in an adjoining instrument-room and devouring them there, previous to the discovery that it was carrying the soap into the compartments of a table built against the north side of the same room, which contained the sink with the soap against the west wall. The table was 82 cm. high, 100 cm. wide, and of the length of the room.

In the closed compartment below the table were kept the stock bottles of acids. The rat had entrance into these compartments from the rear of the end about 6 m. from the sink. The sink was 83 cm. above the floor.

The habit of the rat was to carry the bar of soap from the sink to this acid compartment (7 m.), where it devoured the soap and gnawed off the paper labels marking the acid containers.

A test with litmus showed these labels to be slightly acid,

and the soap to be slightly alkaline. Samples of fæces near the bottles were neutral.

The acids whose labels were eaten or gnawed are nitric, hydrochloric, and sulphuric.

The evidence seems to show that the rat had learned by experience or accident, under its normal conditions, that the food elements in the alkaline soap were slightly more palatable when taken with a sip of neutralising acid.

The older the rat the wiser he may become!

Yours very truly,
ENOCH KARRER.

THE ICE-AGE QUESTION

I.—FROM MAJOR R. A. MARRIOTT, D.S.O.

DEAR SIR,—The remarks made by Mr. H. Spencer Jones under this heading, in the July number of SCIENCE PROGRESS, appear to be an attempt to shelve rather than to solve this very important question. The evidence in favour of Drayson has now reached a stage which makes it less than ever "not necessary" for astronomers "to enter into the reasons for the unanimous rejection" of Drayson's theory.

Unfortunately, it is just these reasons that geologists and laymen wish to know. But, if astronomers would answer this *one* question satisfactorily, it would meet the requirements of the case. If Drayson's reason for the continuous decrease of the obliquity is rejected, what takes its place?

Is it due to the action of the planets which, until the claims of Drayson were put forward lately, was considered to be wholly negligible?[1] Or to what? If the curve followed is now better known than when it was "for small angles a circle, but for larger angles not a circle,"[2] let us have it.

The caution given against basing geological conclusions on the theory is wholly unnecessary, since all the main and essential

[1] If due to the action of the planets, further questions under this head should be able to be answered—viz., What is the different axis, and how long the different period? (See SCIENCE PROGRESS, April 1919, p. 601.)

[2] Statements made by astronomer-correspondents of Drayson's time, and recently.

facts were ascertained in absolute disregard of astronomy, and in ignorance of Drayson's contention.

To quote from the correspondence, "Even if such conclusions were in accordance with purely geological evidence, it would not necessarily follow that Drayson's theory was correct." Perhaps not; but when Drayson's astronomy solves a crucial and fundamental question, to which astronomers have never given a satisfactory answer, and which they are now unable apparently to answer at all,[1] and at the same time outlines certain cosmical conditions which should have prevailed on the earth during 30,000 years, and which geologists independently, in different parts of the world, find to have existed and to have had a duration in strict accordance with the theory, it becomes an act of unreasoning bigotry to refuse investigation.

Yours faithfully,

R. A. Marriott.

Exeter,
August 1919.

II.—From W. H. Hatfield, D.Met., The Brown, Firth, Research Laboratory, Sheffield

Dear Sir,—In Science Progress of April this year we read an extremely interesting article by Major R. A. Marriott, D.S.O., in which the Draysonian theory relative to the Ice Age was discussed. We were extremely interested, and looked forward to the July Number in the hope that more matter on the same subject would appear, even if only in the nature of a discussion upon Major Marriott's paper. We were, therefore, extremely disappointed that we only found a short letter by Mr. H. Spencer Jones. That writer simply considers it desirable to point out that astronomers generally do not accept Drayson's theory, but does not attempt to give the necessary data which your readers, as scientific people, have a right to expect when anyone controverts a theory which had been carefully explained. After a well-reasoned and very interesting article such as that which Major Marriott had prepared, I am of the opinion that it is hardly courteous, and certainly not truly scientific, for anyone to dismiss the matter so lightly as Mr. Jones has done, and I

[1] A question as to the cause of the continuously decreasing obliquity was addressed to the Royal Observatory, and though a stamp was enclosed, no reply was received.

think that your readers have a right to expect such contributors not only to say that they disagree, but also to give the reasons for their point of view. I take it that all your readers are deeply immersed in some field or other of scientific work, and that SCIENCE PROGRESS usefully and successfully enables us all to obtain a reasonably broad knowledge of the general trend of scientific work. As regards the Ice Age, it is clear that there we have a subject of very considerable interest to all of us, and my friends and myself look forward with much interest to the matter being further discussed.

Yours faithfully,
W. H. HATFIELD.

NOTES

Lord Rayleigh (1842-1919) (Sir R. T. Glazebrook, C.B., F.R.S.)

John William Strutt, third Baron Rayleigh, was born on November 12, 1842, and succeeded his father in the title in 1873. He took his B.A. degree at Cambridge in 1865, graduating as Senior Wrangler. The same year he was first Smith's prizeman, and in 1866 he became a Fellow of Trinity College. This position he held until the time of his marriage in 1871 to Evelyn, daughter of Mr. James Maitland Balfour of Wittingehame, and sister of Mr. A. J. Balfour, the Foreign Secretary. Two of their sons survive him—Robert, his successor in the title, now Professor of Physics in the Imperial College, South Kensington; and Arthur, who through the War has served with the Grand Fleet.

He lived at Terling until 1879, when, on Maxwell's death, he accepted the invitation to become Cavendish Professor of Physics at Cambridge, a post which he held until 1884. This same year he took part as President, at Montreal, in the first Meeting of the British Association outside these islands.

In 1887 he became Secretary of the Royal Society, an office he retained until 1896, while he was Professor of Natural Philosophy in the Royal Institution from 1887 to 1905. He was President of the Royal Society from 1905 to 1908. In this latter year he became Chancellor of the University of Cambridge in succession to the late (eighth) Duke of Devonshire. He was closely connected with the National Physical Laboratory from the time of its foundation, first as Chairman of the Treasury Committee appointed to report on the desirability of establishing the Laboratory, then as Chairman of the Executive Committee, an office which he held almost up to the time of his death.

He acted as adviser to the Government on a number of Scientific matters such as Electrical Units, Explosives, Aeronautics, Acoustical questions, and many others.

In 1905 he became a Privy Councillor; he was also one of the first members of the Order of Merit.

In 1904 he was awarded the Nobel Prize ; the Royal Society gave him the Copley, the Royal, and the Rumford medals ; he was an Officer of the Legion of Honour, Foreign Member of the Institute of France, and of many British and Foreign Scientific Societies, while he received degrees from numerous Universities at home and abroad.

His papers from about 1870 to 1910 are contained in the five volumes of Collected Works, and to the 349 there printed others have been added since, which must bring the total well over 400. They cover the whole range of Mathematical Physics, and, to quote Sir J. J. Thomson's appreciative article in *Nature* for July 10 last, " Not one of these is commonplace, there is not one which does not raise the level of our knowledge of the subject." Besides these there is the great book on Sound, which has done more than any other work on the subject to put the fundamental laws of acoustics on a firm basis.

A number of the earlier papers deal with optical questions, and these continued to have a special interest for Lord Rayleigh till the end. The last time the writer saw him at Terling, he was working at the colours of beetles' wings, and was much interested in the difficulties of the problem. The papers deal with the scattering of light by small particles, and by showing that the intensity of the scattered light is inversely proportional to the fourth power of the wave length, give a rational account of the blue of the sky ; the molecules of air are small enough to scatter some of the incident rays and deflect the blue rays much more copiously than the red. About this time, too, the theory of double refraction attracted him, and he investigated the question whether this could be explained by an ælotropic distribution of inertia rather than of elasticity, as had been assumed by older writers. It was shown that, if the ether be treated as incompressible, the assumption led to a form of wave other than that of Fresnel. This was, of course, before the days of Maxwell's Electromagnetic Theory.

During the same period the Theory of Sound appeared ; the first volume was published in 1877. It deals mainly with vibrating systems in general, and well bears out the claim made by its author in his preface for some novelty of treatment and results. The second volume appeared the following year.

Then came the Cambridge period 1879–1884. The Cavendish Laboratory had been opened in 1871 with Maxwell as the first

Professor. It was the gift to the University of the Chancellor, the seventh Duke of Devonshire. Laboratory training in Physics was almost unknown. Carey-Foster had established classes in University College, and Clifton was in charge of the Clarendon Laboratory at Oxford. Maxwell gathered round him a few senior students, among them Chrystal, W. D. Niven, Garnett, and Donald Macalister; but the Laboratory, though equipped with a large collection of lecture-room and similar apparatus, was not laid out for the training of the ordinary undergraduate.

Rayleigh, with the help of his demonstrators, set himself to alter that. Classes in Practical Physics were organised, and the senior men, encouraged by the example of the Professor, began to undertake physical research work. For himself he commenced that great series of Electrical Measurements which have formed the real starting-point of the system of Electrical Units to which the progress of Electrical Engineering is so greatly due. The unit of electrical resistance, the "ohm" had been first realised in concrete form by the work of Maxwell at King's College, but further investigation on the Continent had thrown some doubt on his result, and Rayleigh and Schuster undertook to solve the uncertainty.

In his later work he had the assistance of his sister-in-law, Mrs. Sidgwick, and with her carried out his classical measurements on the Silver Voltameter and the Latimer Clark Cell, thus establishing definitely the units of resistance, current, and electromotive force.

These researches illustrated in a marked degree Lord Rayleigh's genius as an experimental physicist. The apparatus employed, except when high precision was required, was of the simplest, that which came nearest to hand was utilised; and, whether at the Cavendish Laboratory or at his private laboratory at Terling, visitors were astonished at the means by which results of surprising accuracy, accuracy which has only been further substantiated by more elaborate research, were reached.

During this same time there appeared also some remarkable short papers on optical questions, dealing with the resolving power of optical instruments and cognate matter. To him we owe much of the fundamental work connecting the aperture of an optical instrument with its power to distinguish the small

details of an object and the wave-length of the light by which that object is seen. The article " Optics " in the ninth edition of the *Encyclopædia Britannica,* which appeared in the year 1875, is by him, and in it will be found much of his optical work.

The Cambridge period came to an end in 1884, and Lord Rayleigh returned to Terling, but previously to this, in an address in 1882 as President of the Mathematical and Physical Section of the British Association, he had indicated his next line of work.

He wrote : " The time has perhaps come when a redetermination of the densities of the principal gases may be desirable—an undertaking for which I have made some preparations." The discovery of Argon was heralded in 1892 by a letter to *Nature* which begins : " I am much puzzled by some recent results as to the density of Nitrogen "—he had published in 1887 and 1889 papers on the densities of hydrogen and oxygen, and the composition of water—" and shall be much obliged if any of your chemical readers can offer suggestions as to the cause. According to two methods of preparation I obtain quite distinct values. The relative difference, amounting to about one-thousandth part, is small in itself "—the difference ultimately found was one-two-hundredth—" but it lies entirely outside the errors of experiment, and can only be attributed to a variation in the character of the gas."

" Is it possible," he concludes, " that the difference is independent of impurity, the nitrogen being in a different (dissociated) state ? "

Next year his paper on the densities of the principal gases was communicated to the Royal Society, in which it was conclusively shown that chemically prepared nitrogen was lighter than that obtained from the atmosphere. Atmospheric nitrogen contained some unknown impurity, and Lord Rayleigh undertook to separate out the nitrogen from the air, and determine the nature of this unknown substance. This he did by effecting the combination of the nitrogen by the electric discharge, while Sir Wm. Ramsay, who at this stage had become associated with him in the work, accomplished the same result by passing air over heated magnesium, with the result that at the Oxford Meeting of the British Association in 1896 they were able to announce that the atmosphere contains about one-half per cent. of a new gas—Argon. Rayleigh had started

the hare, as Sir J. J. Thomson, in the article already referred to, says, "not by a happy accident or by the application of new and more powerful methods than those at the disposal of his predecessors, but by that of the oldest of chemical methods, the use of the balance."

About the same time, 1896, he became closely connected with another important undertaking already alluded to, the organisation of an Institution which would do for England work similar to that accomplished for Germany by the Reichsanstalt. It had been frequently discussed, and was brought into prominence by Sir Douglas Galton in his address to the British Association at Ipswich in 1895, and again at the Liverpool meeting in 1896. Prof. Lodge had at the same time thrown himself very heartily into the proposals, with the result that a Committee was appointed by Lord Salisbury, then Prime Minister, to consider and report on the matter. Lord Rayleigh was chairman, and the Committee reported in 1898 in favour of establishing the Laboratory "as a public institution for standardising and verifying instruments, for testing materials, and for the verification of physical constants."

The National Physical Laboratory was the result. Lord Rayleigh became chairman of the Committee, and under his care the Laboratory has grown in less than twenty years from an establishment with a staff of fifteen or twenty, and an income of perhaps £5,000, to one with a staff of nearly 600 and an expenditure in 1917–18 of about £100,000.

With all this growth he has been closely connected, and his wise guidance has shown the way to surmount many difficulties.

Some ten years later he became prominently connected with another most important work. In a paper read before the Literary and Philosophical Society of Manchester he had discussed very completely the fundamental principles involved in flight, pointing out in particular the need for experimental work and the conditions under which model research could prove successful; thus when in 1908, at the instigation of Lord Haldane, then Secretary of State for War, the Prime Minister (Mr. Asquith) appointed the Advisory Committee for Aeronautics "for the superintendence of the investigations at the National Physical Laboratory, and for general advice on the Scientific problems arising in connection with the work of the

Professor Barkla

Admiralty and War Office in Aerial Construction and Navigation," Lord Rayleigh naturally became its first President, an office he continued to hold until a few weeks of his death.

His work as President has been of the utmost value to Aeronautics ; perhaps his greatest service has been his insistence on the principle of similarity in defining the condition under which model results obtained in an air-channel are applicable to full scale work, and thus enabling results to be obtained which have given to British Aircraft the superiority they now enjoy.

His death at this moment, when the question of future Research and Education in Aeronautics is still unsettled, is no small disaster.

This list of investigations could, if space permitted, be almost indefinitely extended, some naturally more important than others, but all marked by the same characteristics—a clear grasp of principles, a fearless courage in attacking difficulties, and a firm determination to reach the truth.

For some fifty years he worked. For many years past he has been recognised and revered as the leader of English Physical Science. The public knew him but little ; we who were proud to be his pupils and his helpers realise only too sadly the greatness of our loss.

Professor Barkla (J. Nicol, B.A., B.Sc., Northern Polytechnic Institute)

PROFESSOR BARKLA, the recipient of the Nobel Physics Prize for 1917, is one of the most brilliant of the many distinguished physicists who have come from the Cavendish Laboratory. While there he investigated the influence of the nature of wires on the speed of electric waves propagated along them, but since then the whole of his work has been concerned with X-rays, mainly with the various kinds of secondary rays which they excite. So much did he dominate the subject, that up till the application of diffraction methods to the problem by Laue, Bragg, and Moseley, the history of X-ray discovery was practically a history of the discoveries of himself and his pupils.

Leaving the Cavendish Laboratory in 1902, he returned to his old University, Liverpool, as Oliver Lodge Student, and remained there as demonstrator and lecturer until 1909, when

he became Wheatstone Professor of Physics at King's College, London. This he resigned in 1913 to take up his present position as Professor of Natural Philosophy at Edinburgh. During the whole of the time, wherever he has been, there has issued a steady stream of X-ray research from him, working alone or in conjunction with his students. Some account of his work can best be given by a short description of the properties of the various kinds of secondary radiation excited by X-rays.

This is of three types—scattered, characteristic or fluorescent, and corpuscular. The scattered radiation is the main product formed when X-rays fall on substances of low atomic weight (up to sulphur). In quality it resembles very closely the primary radiation, and its quantity is proportional to the mass of the radiator and independent of its nature. On the ether pulse theory of X-rays it is due to radiation from the accelerated electrons set in motion by the passage of the primary pulse over them. Accepting this view, Barkla calculated the number of radiating sources (electrons) per atom in the radiator (1904). Owing to the inaccuracy of the values of the charge and mass of an electron then accepted, the numbers obtained were too high; but using modern values for e and e/m, the original X-ray measurements show that the number of electrons per atom is about half the atomic weight, except in the case of hydrogen, where it is equal to the atomic weight (1911).

By investigating the amount of the scattering in various directions in the plane perpendicular to the primary ray, Barkla was able to show (1904) that the X-rays from a cathode tube were partially polarised, and later (1906) that the scattered rays from carbon were much more nearly completely polarised. This was of great importance in confirming the ether pulse theory of X-rays and showing their similarity to light. The question of the nature of the rays was, however, by no means settled, for many of the relations between the energy of the original cathode ray, the X-ray produced by it, and the corpuscular ray to which it in turn gives rise, are most easily explained by assuming that the X-rays are corpuscular in nature. Bragg suggested (1907) a special form of particle theory which was able to explain the polarisation of the rays, but Barkla proved that, if care is taken to work with purely

scattered rays, their distribution in the plane containing the primary ray is (except in directions nearly coinciding with that of the primary ray) exactly that required by the pulse theory and not in accord with the particle theory. Later, diffraction experiments by other observers have placed beyond all doubt the identity of X-rays and light, but the difficulty of the energy relations still remains, and exists also in the case of light, where a satisfactory harmonising of the undulatory and quantum theories is one of the outstanding problems of physics.

The fluorescent X-rays are one of the most important of Professor Barkla's discoveries. They form the main bulk of the secondary X-rays from substances with atomic weights from 50 to 100, and differ from the scattered rays in being unpolarised and equally distributed in all directions even when the primary ray is polarised. They have a definite absorption coefficient depending on the nature of the secondary radiator, and not at all on the hardness of the primary beam. Thus successive similar aluminium sheets absorb the same proportion of the rays falling on them, while in the case of the scattered rays successive sheets absorb less and less as the softer constituents of the rays are filtered out. The fluorescent rays are analogous to monochromatic light, the copper rays being distinguished by a definite absorption coefficient in aluminium, just as sodium light has a definite refractive index in glass; but the phenomena in X-rays are much simpler than in light, for the hardness of the rays steadily increases as the atomic weight of the substance producing them rises. At first it was thought that the hardness was a periodic function of the atomic weight, increasing from chromium to tin, then decreasing, to increase again from tungsten to bismuth; but Barkla soon realised (1909) that in the case of the latter elements the rays belong to a different softer series (the L series), so that it would be possible for one metal to give off two different sets of fluorescent rays, just as hydrogen gives off three different monochromatic radiations in the visible spectrum. That this was actually the case was experimentally verified (1910), the hard K and soft L rays being obtained from silver, antimony, and tin. Lately (1916) Barkla has proved the existence of a third, still harder J series of rays from aluminium, oxygen, and carbon. The term "fluorescent" is applied to all

these rays, because they are only excited by rays harder (*i.e.*, of shorter wave-length) than themselves.

The third form of secondary rays, the corpuscular, differ from the preceding in that their hardness (in this case a measure of speed, not of wave-length) depends solely on the primary rays, and not at all on the nature of the radiator.

In the 1916 Bakerian Lecture, Professor Barkla, by considering the energy of the various secondary radiations, arrived at a picture of the processes accompanying their formation. For each quantum of fluorescent radiation formed one corpuscle of associated corpuscular radiation is ejected, carrying with it one quantum of energy of size corresponding to the frequency of the incident rays. Thus, when these are of just greater frequency than that of the fluorescent radiation, half the absorbed energy is emitted as corpuscular rays and half as fluorescent rays. As the hardness of the exciting rays increases, the value of the quantum for them and for the corpuscular rays increases, while that for the fluorescent rays does not alter, so that a larger and larger proportion of the absorbed energy goes into the corpuscular rays. This agrees well with the experimental results. The atom is regarded as consisting of a nucleus with several rings of electrons revolving round it. The ejected corpuscle is supposed to have come from one of these rings, the central one if it is associated with the emission of J radiation, from one farther out if associated with K, L, etc., rays. The place thus left vacant is filled by another corpuscle falling in from outside, and it is the lost potential energy of this, emitted as one quantum, which forms the characteristic fluorescent radiation.

This theory has been based mainly on observations on one element, and needs further investigation; but Professor Barkla's views on the nature of X-rays have been so uniformly substantiated by later experiments that we may expect that these also will be confirmed, and will in his hands lead to further important discoveries.

Science in the War

Lord Moulton's Rede Lecture for 1919 has recently been published in the form of a little book[1] which will interest many readers, and should be useful for

[1] "Science and War," by the Right Honourable Lord Moulton, K.C.B., F.R.S. Cambridge: University Press. Price 2*s.* 6*d.*

science classes in schools. He reviews briefly the discoveries and inventions which have had the most important results in the recent war—explosives, internal combustion engines, secondary batteries, wireless telegraphy, gyroscopes, sound-ranging, chemical warfare, and even medical and surgical advances. It is certainly not generally known that the Germans did not declare war until their great factories for obtaining nitrates (for explosives) from the air were actually at work, thus enabling them to dispense with imports of nitrates from Chili—nor that the Allies were obliged to continue this expensive and dangerous importation owing to their lack of similar foresight. The author says that as soon as the German 70-mile range guns came into play, the whole details of them were at once worked out by us, but that we were too magnanimous to use such weapons. But we have been told that the Parisians suffered not at all inconsiderably from the Big Berthas; and if we had had similar guns we might have done much to control this particular barbarity of the enemy by retaliation on their cities. There is an unfortunate passage on the second and third pages of the book, in which it is suggested that the war "owes its very possibility to science," and that, "but for the stupendous advances that science has made in times within the memory of many here present, no catastrophe at once so wide-spreading and so deep-reaching could have happened." Many of the numerous enemies of science in this country have been trying to saddle it with the war by pretending that the German attack was due to their scientific attainments, just as their weapons were perfected by these attainments; but we doubt whether Lord Moulton desires to belong to this camp. Probably scarcely one of the German war-makers knew any more of science than our own politicians did—whose neglect of every common-sense precaution gave the war-makers their great opportunity. The world is not governed by men of science, but by various types of adventurers; and it was the latter among the Germans who used the former, just as a murderer may employ the pharmacopœia when it suits him to do so. To blame science for the war is like blaming the pharmacopœia for all the criminal poisonings which have occurred by help of it. If man misuses his servant science, that is not the fault of the latter. Nor do we even believe that the horrors of war have been aggravated by the employment of scientific weapons; for the old hand-to-hand fighting must have been very terrible, and we read that almost the whole of a defeated army was sometimes destroyed within a few hours. After all, the total casualties in the recent war seem to have amounted to less than a third or a quarter of the strength, and to much less than the annual toll to disease.

From the commencement of the struggle it was obvious that victory was likely to go to the more inventive side. The Germans had prepared everything in advance, and our men of science were obliged to improvise at a moment's notice. The more honour to them in the result! General Ludendorff is reported to have attributed his defeat to tanks and the blockade—that is, we hear, to the defeat of the submarine largely by our sound-ranging devices. Probably the men to whom victory was chiefly due were these very men of science and inventors—whose names are almost unknown to the public, who are given no part in our numerous triumphal processions, and who, in many cases, have not even been paid for their services! Similarly, when a fine feat in aviation is performed, everyone praises the gallant airman, but no one remembers the greater men who designed the machine. Apparently, in the state of semi-civilisation in which we are really living, the greater the work of an individual, the less does he receive in payment for it. No, the war was not due to science, but to the fact that science has not yet advanced far enough to construct a rational mankind out of the present raw material.

The Recompense of Medical Scientific Workers

We are very glad to hear that the Science Committee of the British Medical Association has elected a Sub-committee to confer with the British Science Guild and other bodies "in the matter of the inadequate recognition and recompense by the Government and other bodies of medical workers in the field of science." We are also glad that the Science Guild is nominating some of its members to confer with this Sub-committee of the British Medical Association. The members are as follows: For the British Medical Association, Sir Clifford Allbutt, K.C.B., F.R.S., Dr. R. T. Leiper, Prof. Benjamin Moore, F.R.S., Mr. E. B. Turner, F.R.C.S., Prof. J. S. Haldane, F.R.S.; and for the British Science Guild, Prof. Bayliss, F.R.S., and Dr. Sommerville (Chairman and Secretary of the Guild's Health Committee), Sir Alfred Keogh, G.C.B., and Sir Ronald Ross.

We have called attention to this matter in SCIENCE PROGRESS over and over again, without any definite result hitherto. There is unlimited talk just now about the encouragement of science, but the vital point is almost always omitted. This point is that, unless you make it worth their while for men of great abilities to investigate nature, they will in many cases not be able to do so even though they have the strongest inclination in that direction. We are now spending large sums of money for scientific work, but most of it goes in providing laboratory facilities and small salaries to junior men for "pot-boiler work." This is certainly essential, and we lodge no objection to such expenditure; but, in addition, we must pay adequately for the best possible brains. There is only one way to do so—by paying for discoveries which have already been made. There is really no other way of detecting the best possible brain when it exists The proof of the pudding is in the eating, and, of the best brain, in the result obtained by it. We therefore think that the world should organise a system of pensions, not only for medical, but for all work which has been of great value to the public at large without being remunerative to the worker. Such a thing is only common sense, common justice, and common morality.

The case of the medical scientific worker is the strongest of all. Few people recognise that medical science brings in almost no payment even when it results in discoveries which are really revolutionising civilisation. The fact is that, of all great events in history, perhaps none exceed in importance the discoveries made during the last century regarding the nature of human diseases and their prevention and cure. Yet the people who have made these discoveries have generally lived, we might almost say, in extreme poverty. We believe that the salaries of pathological professors amount generally to only a few hundreds a year, and seldom, if ever, exceed one thousand pounds a year. Even these posts appear to be seldom given to men who have themselves made leading medical discoveries. Some people seem to think that such men are remunerated by medical practice; but that is far from the case, and anyway it is a poor kind of remuneration which is given only by means of additional work. For example, Jenner, the great discoverer of vaccination, found that his reputation in this line actually ruined his medical practice; and it was partly for this reason that early last century the British Parliament (which was then a rational and virile body) gave him £30,000 as a reward. The reason for this is that everyone considers a famous discoverer to be only a faddist or a charlatan! Of course many other pursuits which are invaluable to civilisation are in precisely the same boat—other branches of science, music, literature, and sometimes even painting, travel, etc. Our proposal is that every nation should keep a pension fund for really great work in these

lines. We do not suppose that the British Empire would have to pay more than, say, £30,000 annually for such pensions, as against many millions of pounds which it now gives as a subvention for loafing, incompetence, and unemployment. The reader who is interested in this theme might see SCIENCE PROGRESS, Nos. 29, 30, 31, 32 (especially), 33, 34, and 38. Of course the war checked these efforts; but they can now be resumed.

How to Pay One's Debts

Some time ago the Calcutta paper *Capital* suggested that the Viceroy and the Governor of Bengal should bring the claims of Sir Leonard Rogers, the eminent medical scientist in India, before the Nobel Committee for a Nobel prize. Commenting on the suggestion in the last April number, SCIENCE PROGRESS pointed out (1) that Sir Leonard Rogers had already been recommended for the prize, but (2) that it would be well if those high officials were to try to think out the matter of giving him also a reward from India. *Capital* of June 13 now declares that "anything more churlish and inconsequent than the comment of SCIENCE PROGRESS it would be impossible to conceive." Apparently our contemporary finds it difficult to understand English. What is there churlish and inconsequent in suggesting that a scientist should be paid by the country which has been benefited by him? We have every hope that Sir Leonard will receive a Nobel prize; and, in fact, the Editor of this Quarterly recommended him for one years ago, long before *Capital* ever seems to have thought of such a thing. But that is no reason why India should not reward him in addition—as it ought to do. SCIENCE PROGRESS argued that such rewards should be a part of the national policy for the support of science in every country. It is only proper that every nation should pay professionai men who perform public professional services for their nation, just as a private patient pays a professional fee for private services. The minds of many people appear to be impenetrable to such a simple notion, and we gather from the remarks of *Capital* that the Indian Government and people are extremely disturbed at the mere suggestion that they should provide a little extra money for Sir Leonard Rogers, though they are quite willing to ask Sweden to do so. But we are grateful for small services, and, anyway, wish to commend *Capital* for suggesting that a medical scientist should be paid at all, a thing which scarcely any other newspaper in the whole British Empire has ever yet dared to do.

Sweden can scarcely be expected to continue to reward (that is, in many cases, to feed) the world's benefactors for ever, single-handed. During the last eighteen years she has probably distributed over half a million pounds in her four annual prizes. Other nations should give their share. For a century the rich, generous, and enlightened British Empire has not given one penny in the same way. But, probably, we know more about the infamous story than *Capital* does.

Sir David Bruce

We regret to learn that Major-General Sir David Bruce, K.C.B., F.R.S., has been superannuated under military rules. While a limit of age is obviously necessary for the army, we maintain that distinguished men of science ought never to be retired in civil work so long as their faculties remain unimpaired, because, in science, knowledge and judgment grow indefinitely with years; and we therefore hope that some means will be found to enable him to continue his medical

researches without pecuniary loss to himself. The State has recently been giving large sums for medical investigation, and we think that a fraction of them should be devoted to this purpose. Either by himself or as leader of several scientific commissions, he has found the causes and modes of transmission of no less than three important diseases—namely, Mediterranean Fever, Sleeping Sickness, and the Tsetse-Fly Disease of cattle—thus having conferred an enormous boon on the world, without any adequate pecuniary gain to himself. Genuine scientific capacity is very rare; not every young man who looks through a microscope possesses it—as some recent, lamentable, and costly failures help to prove; and Sir David Bruce's great ability should not be lost to the world.

Our "Recent Advances"

The Literary Supplement of the *Times* complains that some of our "Recent Advances" are little more than "meagrely annotated lists of papers." We are very glad to receive suggestions, but may point out that this matter is one simply of available space. Some subjects, which have few papers during the quarter, can be more freely annotated than other subjects such as mathematics, for example, of which the list of works is always enormous. The section of Recent Advances is designed to help the man of science and the amateur who wish to keep in touch with branches of science other than their own special branch, but who do not subscribe to bibliographies or the literature of the former. Nearly half our space (including reviews) is employed to meet this want; but if each good paper is to be fully annotated, the whole number would be required for the purpose.

We may take the opportunity to state that articles are put into small print, not because they are supposed to be of lighter or slighter quality, but because their subjects are not purely scientific. We have also to consider exigencies of space; and small type is more suitable for mathematical articles.

The School Science Review (D. O. W.)

This quarterly journal, edited by Mr. Adlam, of the City of London School, and published by Mr. John Murray at 2*s*. net, forms the latest enterprise of that very energetic body, the Science Masters' Association. It should satisfy the long-felt need for a periodical in which science masters can discuss the problems which abound and multiply so freely in science teaching. The appearance of the *Review* is more especially opportune because, very largely as a result of the efforts of the Association, science is becoming a compulsory part of the school curriculum. The methods and syllabus of teaching at present generally used are not such as will fill the masses of the younger generation with enthusiam or even respect for science, and to achieve this result is surely the most important task facing the teacher to-day. His responsibilities are very heavy. He has not only to convince the examining authorities that a change in their syllabuses is necessary, he has also to devise for himself methods of teaching on the lines of *Science for All*, which, while retaining the full savour of the romance of science, must yet contain something of the rigidity and logic of scientific method. This change will not be an easy one for the average teacher; the severe specialisation of the honours course does not lead to the acquirement of the knowledge that is needed, and the routine of everyday work leaves all too little time for reading and thought. Thus it would seem to the writer that it is in the discussion of the problems of

teaching both old and new that the *School Science Review* will find, for a time at least, its most useful function.

The first number contains some most excellent articles; but only one is on the lines we have indicated above—namely, that entitled "Notes on Apparatus and Experiments," and edited by Mr. Beesley, of St. Olave's School. This gives descriptions of convenient forms of laboratory apparatus contributed by teachers at various schools. It is to become a permanent feature of the *Review*, and when illustrated by diagrams, should be of very considerable value to its readers. The article entitled "Recent Advances in Chemistry," by J. Hart Smith, seems to the reviewer less commendable. Only one portion of it, which describes a laboratory method for preparing chloroform, is strictly germane to the purpose of the *Review*. The greater part of the article deals with the structure of the atom, and the information given by the author must surely have been already familiar to most of his readers. An excellent paper by Mr. Durrant, of Marlborough College, on "Ions in Solution," is not only well written in itself, but describes some suggestive experiments for the school laboratory, and hints for the teaching of this fascinating subject.

Three further articles remain for consideration. First, a very readable account of the formation and work of the S.M.A., an account which quite properly opens the first number of the *Review*. Next an article by Sir William Tilden, which presents in delightful style the ideals of the new outlook on school science; and finally, a scheme for the initiation of research in schools, by Mr. Hough, of Oundle. Here we enter on very debatable ground. Sir William Tilden writes:

"There seems to be no reason why young people should not be initiated in the methods of research, though, by reason of their inexperience and want of technical skill, the results of such work may have little or no value beyond the influence it may have on their own processes of thought . . . they might well be taught to inquire into the validity of many popular notions—*e.g.*, the idea that sunlight puts out fire," etc.

Mr. Hough goes a good deal further when he pleads for "genuine research into the unknown." Even if we grant that it would "provide the stimulus required by capable boys with alert brains," it must be remembered that such boys would almost certainly be taking up science as a career, and they would be much better employed at their University or during the waiting period in the performance of more advanced experimental work designed to make them familiar with laboratory manipulation. The writer has rarely met with a boy fresh from school who could make a T-piece with glass tubing, while soldering is an almost, if not equally, unknown art. The science master can give instruction in such work. He cannot reasonably be expected to direct genuine research; his main interests should lie in a different direction, and the problems confronting him in his proper sphere are of sufficient complexity to engage all his time and thought if he be really enthusiastic.

In conclusion, we must repeat our keen appreciation of the work the S.M.A. is carrying on, and our hope that this their latest venture may meet the full success it deserves.

An Experiment in Preparatory School Education

In all the recent controversy about education little has been heard of the preparatory schools. It would be well, perhaps, if attention were focussed on these for a while. Is the instruction given in these places adapted to the nature of their small inhabitants? Often, unfortunately, it is not. Too much time is

given to the imparting of knowledge of a formal kind : too great a strain is put on the memory, too little attention is paid to the development of reasoning power, and to the discovery of latent talent through the awakening of interest.

For many years a fight has been waged against the preponderance of formal training in the public schools. The teaching of mathematics and of science is far more humane than it used to be. Even modern history and the classical studies have now some claim to be grouped among the humanities ! It is easy to point to examples of these changes ; to the Perse School, where the attention of boys is directed towards a classical language itself before being brought to bear on its grammar—the material before the analysis of it ; to the Royal Naval College at Osborne, where from the start the cadets are taught to speak the language they learn, and where the study of history begins with biography, and not with a cram-book of dates ; to Oundle School, where the history of science is read backwards, and the boys know something of the steam-engine before they bother their heads about the properties of steam, and where mathematics is taught, not as an abstract science, but as a handmaid of the various activities of life.

Little of this sort has entered, as yet, into the preparatory schools. There are more reasons than one for this, but the chief is that the most famous public schools, in their entrance scholarship examinations, give preponderating weight to a formal knowledge of the classical languages. The preparatory schools are tempted by the kudos which follows success in these examinations to organise to attain it, and the natural avenues of development of the many pupils are closed in order that the few may reach their objective along the other hard path. A new preparatory school, "St. Piran's," in which the instruction will be along the lines indicated above, has now been started near Maidenhead. Major V. Seymour Bryant, who has left Wellington College to undertake this work, is well known, among those interested in the progress of natural science, as a recent chairman of the Science Masters' Association, and as the present secretary of the Neglect of Science Committee. We wish him success, and feel sure that science will profit from his determination to base his training of the small boys under his charge on their natural inquisitiveness about the phenomena which surround them in their daily lives.

Notes and News

The "Birthday" Honours list contained only one name which need be mentioned here. Professor Boyd Dawkins, the well-known geologist, of Manchester, received a Knighthood.

Mme. Curie has received the Great Cross of the Civilian Order of Alfonso XIII, from the King of Spain, and has been elected Professor of Radiology at Warsaw.

Prof. Soddy has been elected a foreign member of the Swedish Academy of Science in succession to the late Sir William Crookes, and Sir Norman Lockyer has been made an Associate of the Académie Royale des Sciences, des Lettres et des Beaux-Arts de Belgique.

Sir Oliver Lodge's pioneer work on wireless telegraphy has at last been specifically recognised ! The Royal Society of Arts has awarded him its Albert Medal for that reason.

Prof. Eddington has been granted the Pontecoulant Prize for Astronomy by the Paris Academy of Sciences.

Sir James Dewar has been awarded a Franklin Medal by the Franklin Institute of Philadelphia.

Vilhjalmur Stefansson has been awarded the La Roquette Gold Medal of the Geographical Society of Paris, in recognition of the discoveries made by the Canadian Arctic Expedition under his command during the years 1913–18.

The Baly Medal of the Royal College of Physicians has been awarded this year to Dr. Leonard Hill.

The Henry Draper Gold Medal of the National Academy of Sciences, U.S.A., has this year been given to Charles Fabry, Professor of Physics at the University of Marseilles, and the Agassiz Medal to Prince Albert of Monaco.

Prof. W. H. Davis has received the Patron's Medal of the Royal Geographical Society for his contributions to the development of Physical Geography.

Old students and colleagues of Dr. E. S. Goodrich, F.R.S., will note with pleasure that he has been elected Corresponding Member of the "Société de Biologie" of Paris, and an Associate of the "Académie Royale des Sciences de Belgique." Dr. Goodrich is probably the best known British morphologist, his work dealing with a wide range of subjects. He is one of the few English zoologists who have been elected members of these societies.

The Lord President of the Council of the National Physical Laboratory has appointed Prof. Joseph Ernest Petavel, D.Sc., F.R.S., M.I.Mech.E., etc., as Director in succession to Sir Richard Glazebrook, C.B., F.R.S., who retires on reaching the age limit on September 18 next.

Prof. Petavel is Professor of Engineering and Director of the Whitworth Laboratory in the University of Manchester. He is a member of the Advisory Committee for Aeronautics of the Air Ministry. He was educated at University College, London, and undertook scientific research at the Royal Institution and at the Davy Faraday Laboratory until 1898. He was elected John Harling Fellow of the Owens College, Manchester, in 1900, and was Scientific Manager of the Low Temperature Exhibit of the British Royal Commission for the St. Louis Exhibition, 1904.

Among those whose decease has been announced during the last quarter, we note the following: J. Barrell, Professor of Structural Geology at Yale University; Prof. Adrian J. Brown, the well-known specialist in Biological Chemistry; Sir John T. Brunner; Walter G. Davis, Director of the Argentine Meteorological Service; Dr. W. G. Farlow, Professor of Cryptogamic Botany at Harvard; Prof. Emil Fischer, For. Mem. R.S., the eminent chemist; A. P. N. Franchimont, Emeritus Professor of Organic Chemistry in the University of Leyden; Dr. Ernst Haeckel, biologist and anglophobe; Father Walter Sidgreaves, S.J., Director of Stonyhurst College Observatory; Lord Rayleigh (on June 30, 76 years of age); Prof. C. B. Richards, of Yale University, the inventor of the Richards Steam-Engine Indicator; Prof. Gustav Retzius, the Swedish Anatomist.

A Committee has been appointed by the Government to advise the Treasury on grants for University Education in the United Kingdom. It consists of Sir W. McCormick (Chairman), Prof. Bateson, Mr. Dugald Clerk, Sir J. J. Dobbie, Sir F. G. Kenyon, Sir Stanley Leathes, Sir W. Osler, Sir J. J. Thomson, and Miss S. M. Fry. It is stated that grants are to be increased, and that a method of distribution is to be adopted "which would give the individuality of each institution free play, and would safeguard the legitimate interests of University autonomy."

The Zoology Department of King's College, London, has received the splendid gift of £1,000 from Mrs. Row, in memory of her son, Harold Row, who recently

died. The income of this donation is to be devoted to the foundation of a "Harold Row Scholarship" in zoology.

By some six votes, the proposal that Greek should be made an optional subject at Oxford was defeated last June. The narrow margin of defeat may be taken by the proposers as encouraging evidence that next time they will not fail. The result of this vote makes one realise that it is high time the method of government of Oxford University was reorganised. It seems preposterous that a majority of pass degree teachers and parsons should thus be able to hinder the advancement of the University, by out-voting the better informed minority of Oxford professors and science men.

On July 11, Sir William Osler, Regius Professor of Medicine at Oxford, was presented with a book containing a collection of essays written by pupils and colleagues in England and in America. The presentation was made by Sir Clifford Allbutt, of Cambridge. The volumes are just about to be issued to subscribers, and the English publishers are Messrs. H. K. Lewis & Co., of Gower Place, W.C.

According to reports published in the daily Press, American Prohibition Agents, headed by a gentleman called Mr. "Pussyfoot" Johnson, are now in England preparing a campaign aimed at making this country "dry." Apart from the fact that we regard missionary interference in this matter as unnecessary, we must say that, during such trying times as the present, any further tendency to unrest must be discouraged. Elsewhere in this journal we have reviewed a book by an American, in which it is stated that one out of every six men in America is defective in some way, while out of 20,000,000 children, 14,000,000 are handicapped by some kind of physical defect. We are also aware that American cities are not without vice; we conclude that Mr. Johnson might be better employed in America. Such a great authority as Sir William Osler has stated that the present is not the time to launch an anti-drink campaign in this country, however necessary later on.

We understand that Prof. E. B. Wilson, of Columbia University, is at present working on a new book on Cytology.

Prof. Graham Kerr's part in the *Textbook of Embryology* has just come out. It is certainly a fine work, and we must congratulate Prof. Kerr and ourselves on the advent of such an excellent book in the English language.

Two members of Manchester University Zoological Staff have left Manchester to take up work at the Rothamstead Experimental Station, Harpenden. They are Dr. A. D. Imms and Dr. Ward Cutler. We notice a very interesting paper by the latter on Ditrichomonas, in the latest number of the *Quarterly Journal of Microscopical Science*. Dr. Ward Cutler has used the very latest technique, with fruitful results.

The British Scientific Products Exhibition was held at the Central Hall, Westminster, during the period July 3—August 5, the King and Queen honouring it by their presence on July 22. At the time of writing no details were available as to the measure of success attained; when we visited it the attendance was by no means large. It is probable that visitors not possessing considerable scientific knowledge would find it distinctly unintelligible. The demonstrators in charge of the exhibits, with few exceptions, did not seem over-anxious to explain their apparatus, and in many instances no one appeared to be available for this purpose. Science will not be popularised by inviting the public to pay to inspect a museum of mysteries, even though they do constitute the latest and most amazing achievements of mankind. Space does not permit of a detailed account of the various exhibits. That of the Air Ministry was, in the absence of the necessary

explanations, by far the most interesting, while Messrs. Hadfield's and Barr & Stroud's deserve special mention for the attractive arrangement of their stands.

It has been decided to establish a Wild Life Forest Experimental Station at the New York State College of Forestry (Syracuse University) as a memorial to Theodore Roosevelt. Plans for such a station were drawn up in December 1916, with the co-operation of Roosevelt himself, and it is felt that no more suitable and characteristic memorial could possibly be obtained. The work to be carried out includes investigations, experiments, and research in relation to the habits, life histories, and methods of propagation of all kinds of forest wild life, including lake and river fish. The station will be supported partly by grants from the New York Legislature, and partly by private subscription.

The existence of hereditary tendencies or predispositions to cancer in man has been for years a much debated question. Murray, Loeb, and Lathrop have proved that hereditary factors play an extremely important part in determining the incidence of cancer in mice, and Dr. Little, of the Cancer Commission of Harvard, considers that, even with the present imperfect method of preparing data, it seems strongly indicated that there are hereditary tendencies to cancer in man.

Science (May 2, 1919) contains an interesting article on the use of poisonous gases in warfare. It appears that the first-recorded attempt to use such methods dates from the siege of Platæa and Belium by the Spartans in the Athenian-Spartan Wars (B.C. 431–404). Wood, saturated with pitch and sulphur, was burnt under the walls of these cities for the purpose of choking the defenders. From the Middle Ages come tales of poison gases projected from squirts or bottles, and the famous plan of Lord Dundonald for the reduction of Sebastopol, as revealed in the Panmure Papers, consisted in the use of the vapour from burning sulphur. Dundonald estimated that 500 tons of sulphur, together with some 4,000 tons of coke and coal for its combustion, would have been sufficient to reduce the fortress.

A remarkable discovery has been made by two British bacteriologists—Dr. Cramer and Dr. Bullock. They find that bacteria of gas gangrene and of tetanus, when completely freed from their toxins, either by washing or heating, do not produce the specific disease when injected into a mouse or a guinea-pig. But if a small dose of a soluble ionisable calcium salt is injected together with such treated bacteria or their spores, the specific disease is elicited in a very virulent form. The chlorides of sodium, potassium, ammonium, strontium, and magnesium, when injected with the gas gangrene "bug," are not capable of producing the disease. At present these facts ascertained by Bullock and Cramer stand alone, and it remains to be seen how far any development of this work will touch on medical bacteriology.

In late numbers of *Nature* will be found some correspondence between Prof. MacBride and Prof. Bateson with regard to the testimony of Dr. Kammerer, of the Vienna Versuchsanstalt, on the Inheritance of Acquired Characters in the Frog Alytes. From what Prof. Bateson says, it appears that Dr. Kammerer is a most untrustworthy person; dishonesty in Science is worse than any other sort of dishonesty. We are glad to see, however, that Prof. MacBride is trying to have Kammerer's experiments repeated by observers in this country, and the question may be considered to be unsettled at present.

We note the appearance in the *Journal of Anatomy* of a paper by Prof. Arthur Thomson on the Early Development and Maturation of the Human Ovum. This subject is one of great interest to both medical and lay biologists, and it is with pleasure that we congratulate Prof. Thomson on succeeding, after many years' search, in finding some of the missing stages in human embryology. It is a pity,

however, that Prof. Thomson's microphotographs have come out so badly in the paper, and we have no hesitation in saying that the value of his interesting study is much curtailed by unsatisfactory text figures. We hope that Prof. Arthur Thomson will republish his work with hand-drawn illustrations.

The recent eruption of the Kalut volcano in Java cost 40,000 native lives, and caused millions of dollars worth of damage by inundations of hot mud and by falling ashes. The eruption was, nevertheless, on a much smaller scale than that of Krakatoa in 1883, the smaller death-roll in that catastrophe (estimated at 35,000 lives) being due to the isolation of the volcano. The area and population of Java are almost identical with those of England; the distribution of the inhabitants is, however, much more uniform than in this country. Hence it follows that an eruption of any one of the many volcanoes in the island involves a disastrous loss of life.

The details of our method of sound-ranging, which has been in active use since the early months of 1915, have not yet been released for publication (so far as the writer is aware); but Prof. Trowbridge, who had control of the U.S. Signals Service, has made known something of the American system (*Science*, May 30), and the *Scientific American* (May 17) has given photographs of the apparatus employed, though without any adequate description. A sound-ranging section consisted of about 70 men commanded by a lieutenant. These had charge of a central instrument which recorded photographically the time of arrival of the sound of the enemy guns at a series of instruments at surveyed positions near the front line, and covering a length of about five miles. The central instrument delivered automatically developed and fixed photographic records in less than one minute after the sound of the enemy gun reached the front line, and this record could be interpreted by the use of quick graphical methods, so that the position of the gun could be telephoned to the friendly artillery in about one minute more. A survey after the advances of the American army in September and November last "showed that the estimates of accuracy made by the sound-rangers in reporting a location had been very conservative; a location reported not accurate to within fifty yards was often accurate to within twenty-five yards. In general, the average of half a dozen locations of the same gun taken on different days under different weather conditions was of a very high order of accuracy; often a matter of but five or ten yards." The method used by the U.S. army was apparently the same as that used by ourselves. The original device must be credited to the French; the sound-receivers placed near the front line were invented by an English physicist.

Quite lately a new book of practical exercises on mammalian physiology has appeared from the pen of Prof. C. S. Sherrington. This is the greatest contribution to the teaching of physiology published within recent times, and it is especially arranged in order to give the medical student of the better type an insight into the physiology of the mammal from the point of view of experimental medicine. We have no doubt that this book will cause a complete remodelling of the teaching of mammalian physiology in this country and elsewhere. The illustrations are surprisingly good, and are not over-burdened by explanations.

Prof. Arthur Dendy has almost finished his work on certain aspects of wheat and grain preservation. This work has been very successfully carried out by Dr. Dendy, but we shall be glad to see his return to pure zoology. The results of the work have been in the discovery of several important facts with regard to the *modus vivendi* of the destructive wheat weevil. These discoveries should lead to a modification of the modern methods of storing and packing

wheat, barley, and other grains. Prof. Dendy believes in the institution of national food stores, as a preparation for the difficult situations brought about by war and famine.

An interesting periodical is the *Annals of the Durban Museum*, edited by Dr. E. C. Chubb. The latest number contains several papers of interest, ranging from one on the skeleton of the Dodo to the subject of African elephants. In connexion with these we note, with regret, that it has become necessary to destroy the herd in the Addo Bush Forest Reserve near Port Elizabeth. They number about 200, and represent the last survivals of a species which once ranged over all South Africa. The elephants sally out of their reserve in quest of food and water. They break down the fences on the farms which now surround the reserve, stampede cattle, and destroy the crops. Moreover, they assume that the irrigation canals recently constructed for the farmers have been designed for their especial benefit, and, by bathing therein, break down the banks and ruin the dams. To enclose them completely would require a substantial fence 13 miles long, and, as the cost of this would not be less than £20,000, the Provincial Council of the Province of the Cape of Good Hope has decreed the destruction of the herd. It is nevertheless to be hoped that a few of the elephants will still be preserved in a smaller enclosure.

The report of the Inter-Departmental Committee on Power-Alcohol, published on June 30, shows that the consumption of petrol in this country has increased from 1,200 million gallons in 1914 to 2,680 million in 1918, and that it is likely considerably to exceed 3,000 million gallons this year. The necessity for obtaining some artificial substitute, such as alcohol, is thus becoming of essential importance. The yield of alcohol from potatoes is only 20 gallons of 95% alcohol per ton, so that, even with potatoes at their pre-war price, power-alcohol could hardly be produced from them on a commercial basis. Similar objections apply to its manufacture from artichokes, sugar-beet, and mangolds. The most promising process for the production of large quantities of alcohol in this country is by the extraction of ethylene from coal and coke gas ovens. Investigations of the method have not yet proceeded far enough for definite figures as to quantity and price to be given. In our overseas dominions there are two sources which promise exceedingly well. The sun-dried flowers of the mahua-tree (*Bassia latifolia*), which grows in the Central Provinces of India and in Hyderabad, yield no less than 90 gallons of 95% alcohol per ton, and can be delivered to a factory in their own zone at 30*s.* per ton. Further, maize and other cereals have admitted possibilities as raw material for the production of power-alcohol. A satisfactory denaturant also has still to be found. The cost of the method used at present may be as great as sixpence per gallon, and the Committee is of opinion that every effort should be made to provide a denaturant which will be effective in the smallest possible quantities at lowest possible cost. It is further recommended that power-alcohol should share with home-produced benzol and shale motor-spirit the privilege of exemption from the motor-spirit tax, and that it should even be imported from the Colonies free of duty.

On March 28 last, Prof. C. H. Lees presided over a meeting of the Physical Society held for the discussion of the applications of methods of exact measurements to industrial work. Sir R. T. Glazebrook, Mr. Wm. Taylor (of Messrs. Taylor, Taylor, and Hobson, of Leicester), Dr. P. E. Shaw, Prof. W. Ripper, Sir Henry Fowler, Mr. F. M. Ryan, Mr. B. P. Dudding, and others, took part in or contributed papers to the Discussion, which has been published as a separate report by the Society. We owe the application of metrology to industry in this

country to Sir Joseph Whitworth, who demonstrated very clearly the advantages of the use of gauges for accurate mechanical work. By means of a micrometer or some other form of gauge, a workman could fashion his work to the desired degree of accuracy. The next step was the introduction of limit gauges. The workman is given two gauges of fixed dimensions—one a little bigger than is required for the finished work, and the other a little smaller, and all he has to do is to turn his work until the larger gauge will go over it, making sure that he has not ground it so far that the smaller gauge will go over. The difference between the sizes of the two gauges is known as the tolerance. It should be as great as the nature of the work will permit, and varies from a ten-thousandth of an inch in watchmaking to the "sloppy fit" of a perambulator wheel. As a matter of fact, limit gauges are at present only made for the finer classes of work, and it is extremely desirable that their use should be extended to cases in which quite coarse tolerances are permissible. Limit-gauging applies specially to, and is, indeed, essential for, the production of strictly interchangeable parts. Previous to the war the gauges used in this country were in a very bad state; they were inaccurate in themselves and differed enormously from one another. Indeed, when the demand arose in 1915 for interchangeable parts to be made in different shops for the production of munitions, it was found that the best make of English gauges had errors many times larger than the tolerances called for, and that the average gauge made in the tool-rooms of English firms had errors twenty to thirty times these tolerances. Although experience led to the tolerance being trebled, a great deal of improvement was obviously needed to get quantities of gauges of the required accuracy. This work was carried out by the National Physical Laboratory, which designed many ingenious testing-machines capable of accurate and speedy use in the workshops—notably a projection apparatus for examining screw-threads. The importance of work of this character may be judged from the fact that, just before the armistice, 10,000 inspection gauges were being produced per week, and of an accuracy at least equal to those manufactured in other parts of Europe.

In spite of their war experience, it seems that many manufacturers are now going back to their old methods, while others (*e.g.*, the Engineer's Small Tools Manufacturers' Association of Sheffield) were never converted. It seems clear, therefore, that it is only by educating the younger school of engineers that out-of-date, uneconomical methods of production can be finally abolished. This work has already been taken in hand by Dr. Shaw at Nottingham, and by Prof. Ripper and Mr. Bolton at Sheffield. It is to be hoped that their efforts to found departments of Metrology may meet with continued success, and not dwindle to nothing through the indifference of the manufacturers who are so acutely concerned.

ESSAYS

PURE SCIENCE IN RELATION TO THE NATIONAL LIFE.

Being a Lecture delivered on February 15 by ARTHUR SCHUSTER, D.Sc., F.R.S., in a Course on **Science and the Nation,** arranged for Science Teachers by the London County Council Education Committee.

IN discriminating Pure Science from Applied Science, we draw a distinction between two branches of learning without defining how they are related. They might be separate avenues leading independently to different goals, or they might be connected like the branches of a tree to its stem. If we put our trust in the plain meaning of words, the terms used seem to support the second interpretation, as we perceive if we replace the word "science" by its primitive equivalent "knowledge." Before knowledge can be applied, it must exist; and knowledge by itself alone is not worth having unless it can be applied. If this reasoning were correct and contained the whole truth, I might here conclude my discourse, for I should have nothing to add to what previous speakers have told you in this series of lectures. We are all agreed that science lies at the foundation of all great industrial developments; and these have already been sufficiently pressed on your attention. But something more should be said by those who believe that science has a function beyond that which culminates in the workshop or factory, and who maintain that pure science renders an important service, related to an independent part of what is commonly called "national life."

A nation, like an individual, must first of all look after its existence. It must be fed, and is therefore obliged to foster its commercial and industrial activity. But the material comfort which may thus be obtained is worthless unless the opportunities it offers be worthily used. To this end the national life must supply something more than the means for the continuation of its existence, and provide for mental as well as bodily health. As I have expressed it on another occasion [1]: "the duty to work, the right to live, and the leisure to think, are the three prime necessities of our existence, and when one of them fails we only live an incomplete life."

It is a truism to say that no knowledge is worth having which cannot be applied; if it cannot be used, it must be useless. But knowledge can fructify in various ways: it may be applied to increase the wealth of the nation, it may add to the pleasure of life, or it may serve to strengthen our sense of moral responsibility. Instead of taking Pure Science to be merely the foundation of its industrial applications, I look upon it rather as ministering to the mental health of the nation, just as Applied Science ministers to its material prosperity. In this it fulfils the same functions as literary or artistic pursuits. But it has besides a special value: it teaches us something of the nature that surrounds us; it helps us to understand the conditions of life, and to base our judgment on reasoning rather than instinct. Looking to the end rather than to the means, we must draw the separating line

[1] Presidential Address, British Association, 1915.

between Pure and Applied Science, not by the subjects it treats, nor by the degree to which a particular research may lead to industrial applications, but rather by the mental attitude of the worker. "Applied Science" is a vague term; it includes the unselfish and unremunerative researches of a Pasteur as well as the activity of an inventor intent merely to exploit a patent, and between the two extremes we have all the higher forms of "industrial science," which, while ultimately benefiting the community as a whole, are primarily intended to increase the wealth of individuals. To improve one's personal and thereby the national prosperity is as worthy an object as the ambition to gain standing and reputation by intellectual pursuits. I am not at present concerned with such considerations, nor with the exact definition of "Applied" Science. For the purpose of this lecture it is sufficient to define the prosecution of Pure Science as the search for knowledge for its own sake.

The utilitarian and idealistic motives are, however, frequently coexistent, and a sharp distinction can seldom be drawn. A laboratory attached to a chemical works may be full of men charged with the duty to conduct researches. These may often lead to results of theoretical rather than practical value; yet such work—to my mind—falls within the category of industrial science, if its scientific significance has been an incidental consequence rather than a primary motive. On the other hand, men engaged in practical pursuits, having entered their profession as men of business merely, are often carried away—sometimes temporarily, sometimes permanently—by the fascination of research, and produce work which, whatever its ultimate industrial value may be, partakes of the spirit and virtue of pure science. The life of Joule is instructive in this respect. He began with the purely utilitarian object of making an electro-magnetic machine that would work economically. "I hardly doubt," he writes as a boy of nineteen, "that electro-magnetism will ultimately be substituted for steam to propel machinery." He was then led on to consider what we should now call the "equivalence" of different forms of energy, and, adopting the mechanical theory of heat, started on his classical series of experiments. As the work proceeded, he fell more and more under the spell of the intellectual pleasure of research for its own sake. Those who knew him in his later years will agree that his interests were concentrated on the advance of pure knowledge. "If I were a young man," he once said near the close of his life, "I should tackle atomic weights." There was no idea here of industrial applications.

There is—in my opinion—good ground for pressing the claims of pure science at the present moment, because there is some danger that the immediate necessity of promoting industrial prosperity may drive pure science into the background. Some have tried to allay that fear by pointing out that the greater the industrial need, the greater must also be the demand for research, and that Pure Science stands to be the gainer rather than the loser by the increased attention given to its industrial side. The reassurance rests, I think, on a wrong interpretation of what is implied in the meaning of "Pure Science," which loses the essential part of its specific virtue by being pursued with a special view to its applications. That the danger is a real one might be illustrated by the history of the German Universities during the last twenty years. Science pursued merely for the sake of its commercial value, whether immediate or ultimate, loses its claim to be called "pure."

The student who devotes himself to the pursuit of knowledge for its own sake is sometimes regarded as an academic freak ministering selfishly to his own intellectual appetite. We have all heard the story of the distinguished mathe-

matician, who boasted that none of his work would ever find a practical application. That this anecdote is quoted so frequently—being connected with different individuals, without substantial evidence for its genuineness—proves that it characterises only an exceptional case. Individual propensities, such as shyness or fear of public criticism, will affect the influence which a person is capable of exerting, but there can only be few men who would not take a pride in finding that the efforts of their brain have proved of value to their country. The incidents in the life of Archimedes which are most frequently mentioned leave us with the impression that he was mainly an intellectual glutton, happy only in working out his mathematical problems, and this, no doubt, was the natural tendency of his mind. His dying remark is quoted in confirmation: "Noli tangere circulos meos"—"Do not touch my circles," he exclaimed when on the point of being transfixed by a Roman sword. We are told that he set no value on his mechanical inventions, considering them beneath the dignity of science. Yet, during the siege of Syracuse, he placed his knowledge freely at the service of his fellow-citizens, and his engines of war delayed the capture of Syracuse for three years. Can such a man be said to have stood aloof from public interests? It is unbelievable that he did not find satisfaction in the working of that beautiful spiral tube used for raising water. Archimedes was a man of science, the purest of science, but he was also a man of action, and could be carried away by his imagination, and by the possibilities of his discoveries, as when he exclaimed, "Give me firm ground to stand upon, and I will move the earth!"

British science can furnish many examples of great men who served their country not solely in virtue of their scientific achievements. Newton took a leading part in defending the Universities against the interference of the Crown, and, as Master of the Mint, improved the coinage of the country. John Wallis, one of the foremost mathematicians of his time, was frequently mixed up in political affairs, and his facility in deciphering secret codes was made use of by the Puritan party, and brought him into great trouble. Christopher Wren, the great architect, was also distinguished as a pure mathematician, and had, independently of Newton, suggested a universal attraction of matter as the cause of gravitation and the motion of the planets. John Robison accompanied General Wolfe on his expedition to Quebec, and took part in the war. For a time he held a post in a Military Academy at Petrograd, but ultimately settled down as Professor of Natural Philosophy at Edinburgh. Even Henry Cavendish, whose chief object in life is said to have been to avoid the attention of his fellow-men, and whose ambitions were confined to the four walls of his laboratory, did not refuse his help, when it was required, to investigate the efficiency of different forms of lightning-conductors. But this is a minor issue, brought forward only to remove the misconception that those who devote themselves to academic studies keep aloof from the general interests of their country. Intimate contact with nature should render us, on the contrary, more accessible to human sympathies.

How did the study of science originate? It is easy to make a guess. The revolution of the stars in the heavens round an apparently fixed pole assisted the traveller at night to keep his direction, and helped to form an idea of time; the name "geometry" indicates that it arose out of the measurement and parcelling out of land; heavy weights had to be lifted, and hence the discovery of the lever; crops required rain, and the labourer on the land looked anxiously to the clouds for a sign of its arrival. Astronomy, mathematics, mechanics, and meteorology may have begun in this way. This is a plausible but, I believe, insufficient explanation. Men were no doubt forced to observe and invent, but that alone

does not constitute science. You want the connecting-links and generalisations which were supplied by the old philosophers, who discussed the elemental notions which brought the facts into relationship with each other.

The desire to understand nature is as old as the wish to turn its manifestations to practical use. Pure Science and Applied Science have grown up separately, if not independently, and occasionally they came into conflict on account of the fundamental difference of their outlook. We must agree with Plato that it was not the men who observed the rising and setting of the stars who should be classed as astronomers, but those who investigated the spheres of the heavens and the great harmony of the universe. This, according to him, is the only matter worthy of a humanity imbued with the spirit of the gods. The Greek philosophers observed nature, though, unfortunately, they confined themselves to those facts which came under their daily notice. They observed, but did not attempt to experiment; not, at any rate, in a systematic manner. The sense of harmony was for ever so strongly present in their minds that they were led astray by the untrained instinct of their inner consciousness. Nevertheless, if pure science be defined by its guiding motive, its founders were the Greek philosophers, and not the nomad tribes who used the polar star to show them the way at night. The period of Greek science includes the teaching of Thales, Pythagoras, and Anaxagoras, the originator of the atomic theory. Democritus (460–370 B.C.) was the first to teach the corpuscular theory of light; but the preponderating influence of Greek philosophy on the science of the Middle Ages centres in the teaching of Aristotle, who more than anyone else took a comprehensive view of the whole range of human thought.

Science, according to Huxley, is organised common sense; it would be more correct to say that science is the organiser of common sense. To Aristotle it seemed common sense that a body unacted on by a force must be at rest. If it be set in motion and continue to move, he found himself called upon to explain the continuance of its motion as well as its initiation. Our better-trained minds distinguish between a motion in a straight line with unchanging velocity, and one in which either the direction or the velocity alters. The former is identical with the state of rest, which can only be relative to other bodies. The interval of nineteen hundred years between Aristotle and Galileo marks the progress in the common-sense view of what is implied by absence of force. Nevertheless, the guiding motive of Pure Science was always the same. Much has been written about the evil influence which the later followers of Aristotle had on the progress of science; but we must not blame the master because during a particularly barren period the pupils stuck to his errors and disregarded the spirit that pervaded his teaching. His position in the history of science rests on the clear view he had of the problems that had to be solved. What he cared for was Pure Science as he understood it, and as we ought to understand it. If anyone doubt that he had an influence on the life of the nation, let him be referred to the testimony of Alexander the Great. "I honour Aristotle," he said, "as I honour my father, because while one has given me life, the other has made it worth living."

Aristotle was followed by Euclid (330–275 B.C.), Archimedes (287–212 B.C.), and after a considerable interval Ptolemæus (A.D. 70–147). While Rome was busy enforcing its will on the world by military conquests, the centre of culture shifted to Alexandria. But for a time it still remained Greek culture. By the murder of Hypatia in the year 414 the Greek period finally closed, and for a thousand years science had to take refuge under the protection of the Arabs. The search for truth can only flourish in an atmosphere which tolerates differences of opinion;

and the Christianity of this period claimed that its sacred writings contained all knowledge permitted to man. Thus, St. Augustine asserts that there can be no inhabitants on the opposite side of the earth, because no such race is recorded in Scripture. In secular matters the Arabs were more tolerant, and claimed adhesion only to the essential tenets of their religion. They were saved by the innate leaning of Oriental races towards metaphysical speculation, and by the great attention which the climate forced them to pay to sanitary regulations. It was, in the first instance, the medical profession which, as in other periods of history, saved science, but it was primarily a utilitarian science that was fostered, and that soon degenerated. Astronomy became Astrology because men sought divine approbation for their worldly ambitions; chemistry flourished under the influence of the desire to convert base metal into gold; and the laws of physics were turned to impress the public mind by the magical powers of the learned. Occasionally, no doubt, a man arose with nobler desires. Such was Mohammed Ben Musa, and above all the astronomer Alhazen (987–1038). It was through the Spanish Moors that science returned to Europe; but it was alchemy, astrology and magic that mainly found entrance; and the explosive mixture employed by the Arabs in the display of fireworks was rediscovered, perfected as gunpowder, and used for the destruction of human lives. Whatever there remained of pure science was confined to the teaching of Aristotle.

A temporary and sporadic revival of learning showed itself in Europe during the thirteenth century, and was coincident with the renascence of art in Italy under Giotto; but it was short-lived. Roger Bacon's voice remained unheard, though occasionally higher motives prevailed, as when the monk Vitello, one of Bacon's contemporaries, struck by the beauty of the rainbow, investigated the refraction of light. The period, taken as a whole, is singularly depressing.

It is a relief to turn to the dawn of better days, and once more to note the simultaneous revival of science and art. No better example could be given than that of Leonardo da Vinci (1452–1519), known to the world as a great painter, but also a great man of science. Pure and Applied Science can both claim him as an apostle. Mechanics, according to Leonardo, is the paradise of mathematics, because it brings it to fruition. He was an observer and an experimenter. He recognised that the optical arrangement in the eye was that of the camera obscura; he investigated the laws of bodies sliding down an inclined plane, but the gem of his experiments, to my mind, is contained in a manuscript notebook preserved in the library of the Louvre. In it we find the drawing of a long rope stretched out, and the same rope coiled together into spherical shape, with one end protruding a little so that it could be pulled along. A written statement first asserts that the coiled-up rope is easier to move than the one stretched out, because the surface of contact is smaller. But directly underneath this it is contradicted, no doubt as the result of an experiment, and it is explained that the pressure is increased in the same proportion as the contact is diminished. This implies, though it is not implicitly stated, that the retarding force is the same in both cases. An important principle in the laws of friction was thus foreshadowed, if not actually enunciated.

We look to Pure Science for those discoveries or new departures of thought which change the whole mental outlook of the community. Science then becomes international without losing its national influence. Galileo and Darwin supply us with two instances where the search for pure knowledge has led to far-reaching intellectual revolutions. During the interval between the two mental upheavals, great advance in general education had taken place; the two countries in which

they respectively originated differ considerably in national characteristics; the personal temperaments of the two discoverers are widely asunder; and yet they possess many features in common.

A new scientific theory never disposes at once of all difficulties, and serious objections can generally be raised. Addressed, as it generally would be, to scientific men, and expressed in technical language, it invites criticism in detail, and it may be years before some crucial experiment or conclusive argument is found. When confined to the discussion of specialists, its progress is necessarily slow. But great generalisations always contain an element that appeals to a wider public, only that public must be reached. Where there is a conflict, the scale may be turned by the literary power of an exponent, or some striking new fact that stirs the imagination. Galileo was able to supply both.

Three stages may be recognised in the progress of our ideas on the constitution of the universe. In the first, the earth is considered to be the centre round which everything revolves; in the second, the sun becomes the centre of the planetary system and of the universe; in the third, the sun itself is only one member of a vast stellar system. The passage from the second to the third stage was easy. The great struggle came in establishing the second. Yet the idea that the earth revolves round itself and round the sun is an old one. It is said to have been held by Pythagoras, and it was certainly taught by a number of Greek and Indian philosophers, but Aristotle pronounced against it. We may here recognise a fundamental difference in the manner of judging the merits of a theory. We may consider it to be merely an embodiment of a collection of facts, or we may require something more of it. It used to be said that the theory ought to explain the facts; and though no one would now express it in this form, we nevertheless expect a theory to possess some guiding idea which binds the facts together. Otherwise a catalogue of detached statements would do equally well. To the majority of the Greek philosophers this connecting-link was the important matter, and Aristotle believed that he had disposed of the heliocentric doctrine by pointing to the behaviour of heavy bodies on the surface of the earth. They all tend towards its centre, and the earth itself must, therefore, tend towards its own centre. The idea that there may be a similar and more general tendency of all bodies towards each other did not occur to him.

Copernicus (1473-1573) detached himself entirely from any claim to furnish an explanation. Though this has been looked upon as a merit, it also proved to be his weakness, for he abandoned all possibility of influencing public opinion. He showed that, considered merely as a kinematic problem, it was simpler to place the sun in the centre of the planetary system. But he insisted so much on the purely formal side of his fundamental hypothesis that one is led to believe him to have been influenced more by the fear of stirring up religious opposition than by philosophical insight. The heliocentric doctrine thus remained for more than half a century longer a purely academic subject of discussion.

Galileo and his contemporary Gilbert were the first scientific investigators in our present meaning of the term, combining philosophical insight with a craving for the extension of knowledge by direct experiment. The history of the mental development of such pioneers is of entrancing interest. Galileo's father was distinguished as a musician, whose writings show him to have been well acquainted with Greek and Roman literature. The son was sent to a classical school, but it was intended that he should earn his livelihood in commerce. The future mapped out for him could not satisfy his ambitions, and when he showed extraordinary ability in mastering the ancient languages, logic, and dialectics, it was decided to

train him up for the medical profession. It is recorded that after leaving school he accidentally went to listen to a lecture on mathematics, and was so much attracted that he devoted his leisure time to a private study of the subject. Watching the swinging of a lamp suspended by a long rope in the dome of Pisa, he compared the time of its oscillation with the beat of his pulse. He was then a medical student, and possibly he only intended to find a scientific method of measuring changes in the rate of beating of the pulse. Be this as it may, he discovered that the time of swing was the same whether the lamp swung through a large or small arc, and had sufficient insight to recognise that this was a fundamental discovery. He was then nineteen years old, and this event turned his mind to the study of dynamics. At the age of twenty-five he obtained a Professorship of Mathematics at a salary of £12 a year. He came into contact with the scholastic philosophers, because he proved by experiment that, contrary to the orthodox views, heavy and light bodies acquired the same velocity in falling.

Galileo was a fighter by temperament, and fearless—up to a certain point. He made enemies in quantity, but he also had the power of gaining staunch friends, who remained true to him through life. I cannot here enter into the astonishingly successful progress of his dynamical work, but pass on at once to the turning-point which led to the crisis in his life, and marks the beginning of modern science.

The faith in the old Aristotelian doctrine, which Roger Bacon did not succeed in weakening by powerful reasoning, was profoundly shaken by a few nights' visual observation, and henceforth survived only in a few strongholds of reactionary prejudice.

Galileo, applying his newly-constructed telescope to Jupiter, had discovered four moons revolving round that body. The appeal was to imagination rather than to logic: the earth has a moon; Jupiter is a planet and has moons; therefore the earth is a planet. The argument put in this way does not satisfy the strict rules of correct reasoning; but it touches the chord of human sympathy. If it be accepted, the planetary system is transformed into a harmonious whole.

Galileo's discoveries were numerous and brilliant, and he placed dynamics on a solid foundation; but he rendered a greater service to mankind when he taught the worker that science was not the monopoly of the philosopher, the monk, or the impostor, but belonged to the man of intellect in every rank of life. Herein lies his claim to be called the founder of our present science. Latin was then the international language; Galileo preferred to write in Italian. It was not that he was unacquainted with classical literature, but he deliberately chose the language of his own people because he preferred to address them rather than the learned of other nations. His powers of exposition were as great as his powers of reasoning, and there is little doubt that the great influence he had on his generation was largely due to the vigorous manner in which he used his native tongue.

I have mentioned Darwin's work as a second example of an investigation in pure science that has led to a great and lasting effect on the intellectual progress of the world. Those whose memory can carry them back to the time of its publication will not have forgotten the stir created by the appearance of the *Origin of Species*. As in Galileo's case, its effect was enhanced by being placed before the public in a form which allowed its general drift to be appreciated by all educated men. The Copernican theory introduced harmony into the planetary system, and the idea of natural selection united in a similar manner the different branches of the organic world. Both with Galileo and Darwin the

religious animosity that was raised helped to attract notice, and the ultimate effect on the national thought was enhanced. When the changes in the outlook, brought about by a discovery, are more subtle, they become more difficult to trace; but I believe that such doctrines as the atomic theory, the conservation of energy, and the electric theory of matter, have had an influence on the public mind reaching well beyond the limits of scientific specialists.

This is true to a still greater extent of the investigations which have led to recent advances in medical science. The researches of Pasteur, Lister, and their followers, are triumphs of science applied directly to the benefit of mankind; but I fancy that their hold on our imagination is mainly due to the new vista opened out on the nature of disease, the marvellous workings of the lower forms of life, and the almost human attributes of blood corpuscles, which have been disclosed.

The effect on a community is only the summation of the effect on individuals, and if we judge by individuals there can be little doubt that, except under the stress of abnormal circumstances, pure knowledge has as great a hold upon the public mind as the story of its applications. I learned that early in life, when a series of public lectures was organised by the University of Manchester. The Professors were asked to submit titles of lectures they were willing to give, and these were submitted to Working Men's Clubs in Lancashire, who selected those they desired to be delivered to them. Thinking that they would be more interested in matters which affected their daily life, I drew up a list accordingly; but for my own satisfaction I included one on an astronomical subject: and that was the only one ever chosen. Astronomy has a particular fascination, for the reason that it is, perhaps, furthest removed from our ordinary concerns. During the most critical period of the American Civil War, Abraham Lincoln used to seek refuge from his worries and anxieties by spending his nights in the Observatory at Washington watching the heavens. On a previous occasion I have already told the story of the American politician who, during a Presidential Election in which he was keenly interested, visited a friend who possessed a telescope. After an hour spent in contemplation of the heavens, and having received answers to some of his questions, he was silent for awhile, and then left the room, saying: "It does not matter after all whether Taft or Brian gets in."

Not long ago I overheard—in a London club—a conversation between three literary celebrities, who were discussing the relative merits of different studies. One of them blamed scientific men, on the ground that they could not distinguish between "evidence" and "proof." I confess I was startled, because I could not imagine anyone to whom the distinction is more constantly brought home in his daily work than the student of science. We speak of proving a statement when we can show by correct reasoning that certain conclusions follow from certain premises. It is a qualified proof, because the conclusions depend on the truth of the premises. We have, in fact, only shown that the conclusion is contained in the premises. It is the mathematician, and not the logician, who points out that the propositions of Euclid need not be true, because we can imagine a space in which the axioms on which they are based are not true. As regards the truth of scientific theories, we do not pretend ever to reach an absolute proof. We can accumulate evidence until it becomes so overpowering that all *known* alternatives are excluded; but I have never come across the scientific man who would not admit that new alternatives may arise at some future time. My reason for mentioning the incident is that I know how difficult it is for a sane man to make any statement whatsoever that does not contain a grain of truth, and this grain is often worth picking out. I find it in this case in a subsidiary effect possibly pro-

duced in the mind of a beginner by the manner in which the elements of science are taught and have necessarily to be taught. The teacher has to point out that the conclusions of science rest on experience, and not on authority; yet he will be unable to do more than give illustrations of the physical truths he teaches. He must explain theories before the evidence in their support can be fully understood by the pupil; he must formulate laws before he can discuss all that is implied in them. He must, in fact, frequently appeal to authority; but it is not the authority of the old scholastic tradition relying on the wisdom of one man strengthened by imitative echoes of a number of mediocrities. It is the authority of independent intellects who have spent a lifetime in testing their conclusions. For every man who tries to establish a law there will be a number of others eager to find flaws, and the verdict of the time is the combined result of a concourse of opposing tendencies. The difference is the same as that between the judgment of an autocratic ruler who knows no law but that of his own will, and the pronouncement of a judge who, having heard both sides, decides according to the evidence that has been put before him.

Science, like all other branches of knowledge, has its dogmas, but they are admitted to be temporary only. Though they are based on experience, fresh evidence may qualify or alter our interpretation of that experience. Such alterations mark an advance, but do not shake the structure; a scientific revolution is a constitutional progress, and not the violent upheaval of a fundamental faith. The atomic theory remains true in spite of the discovery that atoms may be split up, just as a house built of bricks will remain a house built of bricks, though each brick itself is made up of molecules. All I have said on this subject is only a collection of truisms to the trained man of science. He knows that the interpretation of the facts is subject to change; but to the beginner theories must often appear to be immovable dogmas, and in the early stages the teacher cannot help being dogmatic. Nevertheless, there may be room for improvement. Students are often shown how to compress a volume of air, and to find the relation between the pressure and the compression. I am afraid they are then sometimes led to believe that they have proved Boyle's law. They have done nothing of the kind; for they have only performed one or two experiments, the results of which are not inconsistent with the law. The experiments dealt with only one gas, and the measurements did not possess any high degree of accuracy. A proof is, indeed, impossible, because the law is not strictly true. The same remarks apply to the experiments often found described in elementary books on laboratory work, in which Ohm's law is said to be proved when it has only been illustrated. Teachers ought to be careful to put such experiments in their proper light, because a wrong impression in the early stages of scientific instruction may lead to that kind of arrogance which belongs to the intelligent youngster in any subject, but is, perhaps with some justification, laid more particularly to the charge of the half-fledged learner of science. I have had a sufficiently long experience as a teacher of elementary science to speak out frankly on the subject, because I know its great difficulties. It is well that these should be clearly understood.

At the present time we are urging the importance of extending the scientific instruction which is being given in schools. Some base the claims of science on its utilitarian value; but if this were the main object, the teaching should be principally addressed to those who will ultimately require a knowledge of science in their profession. If we adopt this view, the teaching would be simple enough. The future student of science is drawn to his subject by an instinctive feeling of predilection, properly classified by Karl Pearson as an æsthetic feeling; this

carries him easily over the initial difficulties; he is not frightened by the drudgery which accompanies the early stages of every serious study. To a boy of considerable scientific talent, it probably makes little difference whether he be taught well or badly. But sound teaching is of paramount importance to the great number whose only chance of a little science comes during their school life. There is no doubt in my mind that the teaching ought principally to be addressed to them. Those who believe that science can convey an inspiring message to everyone will agree that the infusion of a scientific spirit is of greater importance than the knowledge of scientific facts.

But we must insist that all science depends on strict and clear reasoning. Leaving proof out of the question, the value of scientific evidence centres in the agreement between the conclusion drawn from certain premises and the observed facts. That agreement to be perfect has to be quantitative as well as qualitative, and measurement therefore lies at its root. To express it in different words, mechanics, as Leonardo da Vinci has pointed out, is the foundation of all science. Mechanics implies a certain amount of mathematics, with a moderate facility in the manipulation of numbers, and here we meet with the difficulty that the study of mathematics is unsympathetic to a considerable section of the community. Are we, then, to draw a line even at an early stage of the teaching, separating those who have an aptitude for mathematics from those who have not? It seems that we must either exclude a number of scholars from scientific teaching, or force a distasteful subject on them. I have suffered too much myself from the latter alternative to advocate it without qualification, but I should not assume that what may be only a superficial aversion has any deep-seated cause in the constitution of a boy's brain. Maxwell, in his own trenchant manner, once said to me: "All men are mathematicians, only some know it and some don't." It is here that the skilful teacher has his chance, and may stir the imagination of the pupil to a point at which the aversion is overcome. A member of my own family furnishes an example which may serve to illustrate my meaning. As a boy he made no progress in his mathematical lessons; he disliked the subject so much that he went to the headmaster of the school, declared his inability to master its elements, and asked to be excused from further attendance, declaring himself willing to spend the time on any other subject that might be assigned to him. The headmaster, being a reasonable man, talked to him sympathetically (he was probably no mathematician himself). "Try another six months," he said, "and if at the end of that time you come to me with the same request, I will agree to it." Whether it was the feeling of freedom from compulsion, or whether he was put under another teacher who succeeded in appealing to his imagination in the right way, these six months altered his whole outlook. He stuck to his mathematics, went to the University, and ultimately became a mathematician of some repute. He had to lead the life of an invalid, and I remember him an old man lying on his couch, finding his chief consolation in mathematical work, which leading journals found sufficiently important to publish.

Exciting the imagination is one of the most powerful methods of mastering difficulties by overcoming the distaste for the initial drudgery of work which only becomes interesting when these difficulties have been surmounted. It is specially important in illustrating numerical relationships, and allowing the mind to grasp the meaning of large figures.

Astronomy will furnish a number of suitable examples. The distance from the sun to the earth is about 92,000,000 miles, a number which perhaps in itself does not convey much, but if we use an illustration due to the late Professor Young

of Princetown, we may obtain a better idea of what it means. The sensation of pain travels along a nerve with a definite speed. If you put your finger on a hot surface, the pain will be felt some little time after the moment of contact. We do not realise this, as the interval is so small, the rate of propagation being roughly 100 feet a second. By a calculation which can easily be verified, we find that if a child were born with an arm long enough to reach the sun, it would die of old age long before it could feel that its fingers had been burnt. The distance to the nearest star can be brought home by a slight variant of an illustration used by the late Sir Robert Ball. If we could travel to the sun or the nearest star for the same fare as we travel on a railway—i.e., one penny per mile—the journey to the sun would cost a little less than £400,000, but the journey to the nearest star would require a sum sufficient to pay for the cost of the late war two or three times over. (The exact amount would be £72,000,000,000.)

Lord Kelvin used to speak scornfully about persons who spoke of such magnitudes as the distances of the stars as "inconceivably large." To him everything that could be expressed in figures was conceivable, and indeed one's powers of conception are greatly a matter of practice, and of the choice of suitable units or the selection of intermediate standards. Some men may be able to form an image of a thousand men grouped together, but would fail if the number were extended to a million, while this might be easy to the commander of an army. Most of us, especially those who have travelled, can form some idea of the size of the earth. A sphere of 4,000 miles radius is certainly not inconceivably large; using the earth as an intermediate standard, some assistance is given to a plausible representation of large figures. The number of molecules in a cubic inch of gas at the freezing-point and under atmospheric pressure is 46 followed by 19 zeros, and we may well at first sight look upon such a number as inconceivably large. But let us spread those molecules uniformly over the whole earth, and we shall find only 14 on each square inch. One further example may be given of a quantity which may be made to look either very small or very large according to the manner of expression. Assuming the average annual rainfall over the world to be 50 inches, a quantity which we can easily picture to ourselves, it would mean that in each second of time the average rainfall produces a layer of water over the earth of a thickness a little more than one 1,500,000th of an inch; an inconceivably small figure, some may say. But working out the total weight, we find that over 30,000,000 tons of water falls on the earth in each second of time. Calculate from this the weight of the *annual* rainfall over the world, and you get an enormous figure to represent the same fact which previously gave us what seemed an inconceivably small number.

Well-chosen illustrations are frequently helpful in bringing scientific teaching into relationship with every-day life. Nature seldom presents to us problems in their simplest form, and one of the minor worries of a teacher anxious to spread the knowledge of science is that he is confronted with intelligent questions which he is unable to answer, because the questioner does not possess the preliminary knowledge necessary to understand the answer. What is the cause of the blue colour of the sky? or the red of the sunset? Why does the high-water in tides occur every twelve hours instead of only once a day? Why does the barometer fall at the approach of stormy weather? These are very intelligent questions, but what answer can we give without assuming a considerable previous knowledge of science?

To obtain a knowledge of the forces of nature in their simplest form we must have recourse to specially selected experiments, which generally seem to have

no relation to the experience of our daily lives. We teach a student elementary principles of measurement when he wants to know the working of a telephone, we explain to him all about specific and latent heat when he thinks that he knows already something of how a locomotive works, and does not see why he should have to go through all this drudgery. I do not wish to alter the present modes of instruction. Teaching has a disciplinary value apart from the facts that are taught. The concentration of thought on a definite problem; the training in accuracy and neatness in work; the importance of mechanics as a foundation of science—all these are necessary if the value of science teaching is not to be impaired. But, simultaneously with this instruction and independently of it, I think some time might be spent in stirring the imagination, improving the observational faculties, and bringing the different branches of science into proper relationship with each other. While physics and chemistry must always receive the first place in laying a firm foundation, observational sciences such as astronomy, geology, and biology will furnish large opportunities to supply the supplementary teaching with instructive examples.

More intimately connected with the main object of my lecture is the moral lesson that can be drawn from all science teaching. Nature works by fixed rules. Constant changes are occurring in the position and the illumination of the objects that surround you, or in the state of our inner consciousness. Each of these changes is both preceded and followed by other changes which are not independent, but connected with it, like the oscillation of the piston of an engine with the rotation of the wheel it drives. Every one of your acts and every one of your thoughts have traces which persist throughout time, and reach to the ends of the universe. You may accidentally—but it never is accidentally—drop a sheet of paper; you think nothing of it and lift it up again, and there, perhaps, to your mind, the matter ends. But while the paper fell, the whole earth moved towards it, and your casual act has affected every person that stands on it; your carelessness has increased the length of the day by an amount which you ought to be able to calculate. By replacing the paper in its previous position, you can never quite undo all that you have done; some effect of your action will persist for ever. Or while out on a walk on a clear day, you may be taken with a desire to smoke. You light a match, which the wind blows out immediately. During the second the match was burning it sent out waves of light. The waves will travel outwards, pass through the atmosphere, and reach the sun in 8⅓ minutes of time; but to the right and the left of the sun they will pass onwards. In sixty years they will reach stars whose distances may still be measured by direct means. But other methods of estimating distance are now available, and we are able to point to small telescopic bodies, and say that twenty thousand years hence the explosive compound contained in your match will supply a minute fraction of its energy to their surface. The consequences of your action will endure for ever. If this be true of your most casual and irresponsible acts, it also holds for the thoughts and motives which guide you in the serious tasks of your life. The consequences may be more vital, but we cannot trace them so accurately in the intellectual domain as we can in the material universe. Theology and philosophy may attempt to do so, but scientific knowledge alone can enforce the lesson with unrelenting persistency

THE NUTRITIVE VALUE OF FEEDING-STUFFS (J. Alan Murray, University College, Reading).

It was formerly believed that the nutritive value of a feeding-stuff was determined entirely by its composition and digestibility. The sum of the percentages of digestible protein, carbohydrates and fat multiplied by 2·3 was called the "total digestible nutrients," and the ratio of the sum of the last two to the first is still called the "nutritive ratio." This view obtained until it was shown by Kellner that the digestible nutrients of hay and straw did not produce so much body fat as equal amounts of the same digestible nutrients in a pure state or in various cakes and meals. To account for this he advanced the hypothesis that more energy is expended on the mastication and digestion of coarse fodders, and he attributed this to the presence of the hard lignified fibre they contain. The fact that the productive value of wheat straw is increased appreciably when the straw is finely ground, and to a much greater extent when it is reduced to pulp, before being consumed, was regarded as confirmatory evidence.

He therefore proposed to deduct from the total digestible nutrients a sum equal to about three-fifths (0·58) of the percentage of crude fibre. His experiments showed that this adjustment was satisfactory for hay and straw, but for grasses and green fodders the factor (0·58) had to be reduced by 0·05 for each 2 per cent. of fibre below 16 per cent. as follows[1]:

Crude fibre *per cent.*	16	14	12	10	8	6	4
Factor . . .	·58	·53	·48	·43	·38	·34	·29

A certain correction was found to be necessary also in the case of cakes and meals, but it could not be made in this way. He therefore determined the relative productive value of the nutrients in these foods, and expressed the results by means of a number called the "value" of the food. This may be concisely expressed by the formula—

$$v = 100f/F,$$

where f is the amount of body fat produced by the digested nutrients in any quantity of the feeding-stuff, F is the amount produced by equal weights of the same nutrients (digested) in a pure state, and v is the value number.

The data obtained in the course of these investigations enabled him to revise the coefficients of equivalence for fat-production of the several nutrients, and he attempted to express the nutritive values of the various kinds of feeding-stuff in terms of an equivalent amount of starch. If we accept his view[2] that, without serious error, 1 part of fat may be reckoned equal to 2·2 parts of starch in all cases, this is concisely expressed by the formula—

$$S.E = (2{\cdot}2F + C + 0{\cdot}94P) \times v/100$$

where F, C, and P are the percentages of digestible fat, carbohydrates (including fibre) and protein respectively, v is the value number, and $S.E$ is the starch equivalent of 100 parts of the feeding-stuff.

In the case of hays and straws:

$$S.E = (2{\cdot}2F + C + 0{\cdot}94P) - 0{\cdot}58p,$$

where p is the percentage of total crude fibre. For green fodders the calculation is made in the same way, but the factor 0·58 is varied as described above.

[1] *Scientific Feeding of Animals*, p. 356.
[2] *Ibid.*, p. 357.

Kellner's hypothesis that the difference in productive value of the digestible nutrients in various kinds of food is attributable to expenditure of energy in mastication and digestion of the lignified fibre is not easily reconciled with the necessity for this difference in the method of calculation or with certain other facts. On his own showing,[1] the "value" of linseed meal, which contains 9 per cent. of crude fibre, is 96, whereas that of palmnut meal, which contains 25 per cent., is 100, and that of beet molasses, which contains none, is only 87. He also showed that digested pure fat does not produce its own weight of body fat, but only from 50 per cent. to 60 per cent. of that amount. In other words, from 40 per cent. to 50 per cent. of the energy of the digested fat disappears, and is, presumably, expended on mastication and digestion of the substance. The same is true of starch and other ingredients. These phenomena are not due to the presence of fibre, and the hypothesis appears to be untenable. As a matter of fact, it is erroneous. The apparent loss of energy in the digestion of wheat straw is not diminished when the straw is pulped; the increased productive value arises merely from the larger amount digested.[2]

The loss of energy observed by Kellner, however, is real—whatever the cause may be—and that the productive value of the nutrients is not the same in all feeding-stuffs is a fact. When this was realised, the starch equivalent system was readily accepted. There was no alternative; and it cannot be denied that it affords a more reliable indication of the nutritive (productive) value than does the "total digestible nutrients." For the purpose of calculating rations, however, it is of little significance unless the maintenance requirements can be stated in the same terms. Kellner arrived at his conclusions in regard to this by the practical method of adjusting rations to the maintenance requirements, and calculating the amount of starch equivalent to the same. The mean result[3] of several experiments was $4\frac{3}{4}$ lb. per day for oxen of 1000 lb. live weight, but he recommends 6 lb. as the standard to be used in practice. More recent investigations show that the latter is nearer the truth.

The amount of pure starch required for maintenance of ruminants cannot be determined by direct experiment, because these animals cannot subsist normally on such concentrated food. Neither can it be inferred from the normal heat-emission, for that depends not only on the size and condition of the animals, but also, to a large extent, on the quantity and quality of the food consumed. The whole of the metabolisable (thermic) energy of the maintenance ration is ultimately given out as heat from the body, and the heat-emission of an animal consuming the quantities of food equivalent to 6 lb. of starch would, therefore, be as follows:

Kind of Food.	Starch Equivalent per lb.	Amount of Food required.	Thermic Energy per lb.	Total Thermic Energy of Food	
		lb.	*kt.*	*kt.*	*Cal.*
Pure starch .	1·00	6·0	3·76	22·6	10,260
Maize meal .	·78	7·7	3·00	23·1	10,487
Meadow hay .	·31	19·4	1·82	35·3	16,026
Wheat straw .	·11	54·5	1·27	69·2	31,417

N.B.—A kilo pound unit (*kt*) of energy is the amount given out from 1000 lb. of water when cooled 1°C., ∴ $kt \times 454 = Cal.$ Also *kt* per lb. = *Cal.* per gram = Therms per kilogram.

[1] *Scientific Feeding of Animals*, tables pp. 360 *et seq.*
[2] Armsby, *Jour. Agric. Resources*, 1915, p. 485.
[3] *Scientific Feeding of Animals*, p. 244.

For equal amounts of starch equivalent, meadow hay gives out 50 per cent. more heat than pure starch, and wheat straw more than three times as much. In other words, 12½ lb. of hay and 17¾ lb. of straw respectively would furnish as much heat to the animal as 6 lb. of starch. If the function of the food were merely to maintain the body temperature, these quantities would suffice for that purpose; but in that case the starch equivalent system would be destroyed, for, clearly, 6 lb. of starch, or any other definite quantity, could not be accepted as the standard for maintenance. It would be necessary to have a different standard for each kind of food.

It is apparently for this reason that Wood[1] has attempted to draw a distinction between the starch equivalent for production and that for maintenance. Using the same notation as before, the difference between the two may be concisely indicated by the formulæ—

$$P.S.E. = (2{\cdot}2F + C + 0{\cdot}94P) \times v/100$$
$$M.S.E. = (2{\cdot}3F + C + 1{\cdot}25P + 0{\cdot}6A)$$

It will be seen that *P.S.E.* is Kellner's unit, and that *M.S.E.* differs from it mainly in that the value number is not used, and no deduction is made on account of the fibre. It is, in effect, the discredited "total digestible nutrients," and not starch equivalent at all. The inference that 17¾ lb. of wheat straw is a sufficient maintenance ration for oxen of 1000 lb. live weight is in conflict with experimental evidence.

The chief function of maintenance rations is not to produce heat, but to furnish the energy required for internal work. This energy differs from that expended on external work only in that it is transformed into heat within the animal's body. It is, therefore, available to maintain the body temperature, and it is, normally, sufficient for that purpose. The amount of heat emitted by an animal when it consumes no food at all, called the basal katabolism, is, therefore, the true measure of its maintenance requirements. For ruminants this cannot be determined directly, but Armsby[2] has shown that it may be inferred from the difference in heat-production due to large and small amounts of the same ration. Ingestion of solids increases heat-production. The energy so transformed is not available for internal or external work, or for production of milk or meat, and, normally, little if any of it is required to maintain the body temperature. It is, in fact, the non-productive energy observed by Kellner, and ascribed by him to expenditure on mastication and digestion of fibre. For most of the common feeding-stuffs it varies from about 1·0 to 1·2 *kt* (500 *Cal.*) per lb. of dry matter ingested, and this amount must be deducted from the metabolisable (thermic) energy. It is only the balance, called by Armsby net available energy, that can be utilised for production or for maintenance.

The metabolisable energy of feeding-stuffs may be calculated from the percentages of digestible nutrients by means of the usual factors or, according to Armsby,[3] from the percentage of digestible organic matter; the factors (*kt*) to be used are—for hay and straw 3·5, for grains 3·9, and for oil-cakes 4·4 to 4·8. The net energy is then found by deducting the heat-production (*kt* per 100 lb.) due to ingestion of the food.

In recent investigations[4] on the net energy of starch, Armsby has obtained results substantially different from those of Kellner, as follows:

[1] *Composition and Nutritive Value of Feeding-Stuffs* (Cambridge University Press).

[2] *Jour. Agric. Resources*, vol. xi, No. 10.

[3] *Bulletin* 142, Pa. State Colony Agricultural Experimental Station.

[4] *Jour. Agric. Resources*, vol. xv, No. 5, 1918.

	Total Energy per lb.	Metabolisable Energy per lb.	Heat Increment per lb.	Net Energy per lb.	Organic Matter digested.	Energy per lb. of Organic Matter digested.	
						Metabol.	Net.
	kt.	*kt.*	*kt.*	*kt.*	*per cent.*	*kt.*	*kt.*
Armsby .	4·105	3·328	1·692	1·636	93·6	3·563	1·75
Kellner .	4·152	3·051	1·248	1·803	84·6	3·603	2·25

Comparison of the data shows that the loss in fæces observed by Kellner was more than three times as much as in Armsby's experiments, but the loss as methane was about 50 per cent. less, and the resultant difference in metabolisable energy was largely obliterated by the difference in heat-production. The difference in net energy per lb. of starch digested cannot, however, be lightly disregarded. Allowing for the difference attributed to the protein, Kellner's starch equivalents multiplied by 2·25 give products nearly equal to the net energy values (*kt*) of the foods; and for the practical purpose of calculating rations it makes but little difference which of the two systems is employed.

From a theoretic standpoint, however, the system of net energy values has much to recommend it. It is simpler, independent of any meretricious hypotheses, and illuminates many points that are obscured by the starch equivalent system. It leads directly to a simple law of maintenance and a law of production which are applicable to all animals alike. These may require some modification, but, subject to a large probable error, they may be tentatively stated as follows:

(1) The maintenance requirements (net energy) are equal to the basal katabolism of the animal which varies as the two-thirds power of the live weight in fat-free condition, and increases faster than the live weight as condition improves.

(2) The requirements (net energy) in excess of maintenance, as defined above, are equal to the total energy of the milk or meat produced.

The following equation, deduced by the author,[1] embodies the quantitative expression of the law of maintenance for ruminants:

$$\eta = \frac{9{\cdot}187}{903^{\frac{2}{3}}} \times \left\{\frac{M(100-F)}{100}\right\}^{\frac{2}{3}} \frac{100}{100 - 1{\cdot}247F}$$

or $$\log \eta = \tfrac{2}{3}\{\log M + \log(100 - F)\} - \{\log(100 - 1{\cdot}247F) + 0{\cdot}3407\}$$

M is the observed live weight (lb.), F the percentage of body fat inferred from the condition, and η is the basal katabolism.

For horses and pigs the constants are not necessarily the same as those for cattle and sheep, but the difference, if any, is probably small. It does not follow that the same kinds of food are suitable for all. The diminishing returns for food consumed by fattening animals may be ascribed to three contributory causes—viz., (1) increased basal katabolism; (2) slower rate of production; and (3) in the case of animals not fully grown, the larger amounts of potential energy in each successive pound of increase.

Contrary to what is commonly believed, the suitability of a food for any particular purpose is not determined either by the net energy or by the starch equivalent. Both of these units refer to the concentration, and afford little or no information regarding the quality. A complete picture of the properties of a food may be obtained if the energy values are plotted on co-ordinate axes. In fig. 1 the distance along ox = the total energy, and AB = the metabolisable energy. The

[1] *Jour. Agric. Sci.*, vol. ix, No. 2.

upper part of AB is the net available portion, and the lower is non-available. Then joining OA and OB—

$$y = x \tan \alpha \quad \text{and} \quad y' = x \tan \beta.$$

Foods of similar character must give figures of similar shape, though not necessarily of like dimensions. Conversely, those which give similar figures may be regarded as alike in character, though they may differ in concentration. For comparison, several foods have been plotted on the same axes in fig. 2. For clearness the lines OA and OB have not been drawn in each case, but the two directive lines corresponding to angles of 15° and 25° respectively in the upper part of the diagram, and one corresponding to an angle of 15° in the lower, should be sufficient for the purpose.

The net energy of the oil-seeds was reckoned, like that of the other foods, by Armsby's method; but as it is doubtful whether that method is applicable to

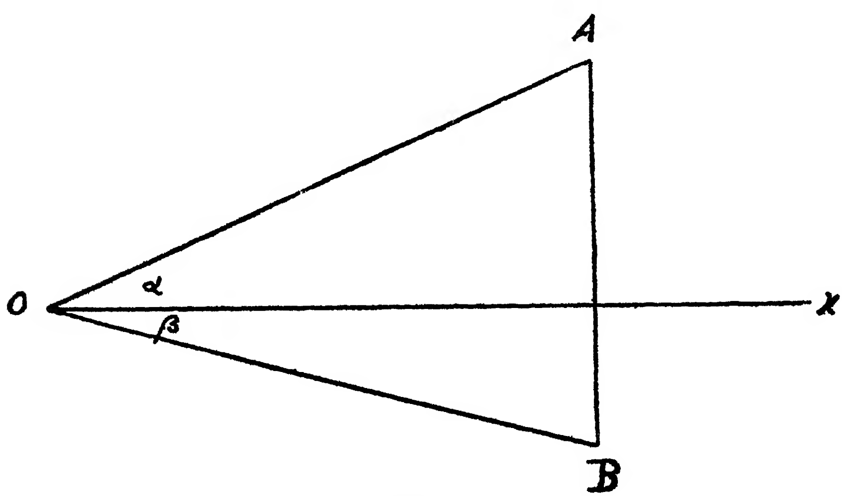

FIG. 1.

products which contain 40 or 50 per cent. of fat, it was recalculated by means of the usual coefficients. This gives considerably higher results, and the difference is indicated by the broken lines above the firm ones.

It will be seen that for foods rich in oil the angle β is less than 15°, while for Italian rye grass, meat meal, and pure starch, it is greater. The precise significance of the fact is not apparent. It is $\tan \alpha$, the ratio of net available to total energy, that determines the character of the food. As this cannot, unfortunately, be called the nutritive ratio, it may be referred to as the productive index. Swedes and potatoes give very similar triangles, in which $\tan \alpha$ is practically equal to that of decorticated cotton cake, greater than that of pure starch or oil, but less than that of molasses, maize meal, earthnut cake, or linseed cake. Roots are not infrequently described as coarse fodder, apparently for no other reason than because they are cheap, whereas, judged by the method here described, they are among the finest. Hay, if made without loss, should be very similar in character to the fresh material from which it is made. In the diagram, fresh red clover and very good red clover hay have the same productive index. Italian rye grass, timothy

grass, brewer's grains and undecorticated cotton cake are very similar, but the productive index of average meadow hay and of oat straw is lower.

Consideration of the starch equivalent per lb. of dry matter leads to much the same conclusions, as may be seen from the following examples:

	Dry Matter. per cent.	Starch Equivalent. per 100 lb.	per lb. of dry matter.
Maize meal	85·0	78·0	·919
Earthnut cake	90·6	75·7	·835
Linseed cake	88·8	74·0	·833
Decorticated cotton cake	90·2	70·7	·783
Potatoes	23·8	17·8	·748
Swedes	11·5	7·3	·635
Molasses	78·1	47·8	·621
Brewer's grains	32·4	18·4	·564
Red clover	19·0	10·2	·537
Meadow hay (very good)	84·0	40·4	·481
Undecorticated cotton cake	87·9	40·0	·455
Italian rye grass	31·5	13·7	·435
Red clover hay	83·5	35·7	·427
Timothy grass	33·1	14·0	·423
Meadow hay (poor)	85·7	18·6	·217
Oat straw	86·0	17·0	·196
Wheat straw	86·0	10·8	·125

These data serve to explain the well-known fact that roots may advantageously form a large part of rations for production, whereas straw may not, though the starch equivalent of the latter is double that of the former. Moreover, there is reason to believe that animals consume more total dry matter when the rations consist largely of roots. It is certain that the amount any given animal can consume is limited. In a recent experiment the specific capacity for food of a fat ox (1400 lb. live weight) was found to be 24·2 lb. of dry matter per 1000 lb. fat-free live weight, but there is nothing to show whether it varies with the size, age, condition, and the kind of animal.

The basal katabolism of an ox of 1000 lb. live weight in store condition is about 10·5 *kt* (4,900 *Cal.*), and as the net energy of wheat straw is only 0·17 *kt* per lb., about 62 lb. of that substance would be required for maintenance; but if the animal can consume only 25 or 26 lb. of the fodder, it must ultimately die of starvation. This result would accrue not, as has been said, because the animal expends more energy on the food than it gets out of it, but simply because it cannot eat enough to satisfy its requirements. Sawdust contains about 15 per cent. of digestible organic matter, equal to about 0·5 *kt* of metabolisable energy per lb.; but, as ingestion of the material increases metabolism by nearly twice that amount, the nutritive effect is negative. In other words, sawdust does not retard, but accelerates, the process of starvation.

It is estimated that, if the basal katabolism of an ox were 10·5 *kt* in store condition, it would be about 15 *kt* when the animal became fat. As pasture grass contains about 22 per cent. of dry matter, 100 lb. per day would be about the limit of the animal's capacity for this fodder. If the net energy of the grass were 0·15 *kt* per lb., the animal might eventually attain the fat condition; but if it were only 0·12 *kt*, or less, as in some of the poorer pastures, the animal could not improve beyond the half-fat condition, however large an area it was allowed to graze; the whole of the net energy of the largest amount of food the animal could consume would be required for maintenance at that stage, and there would be no surplus

for further production. The improvement in the quality of pastures effected by phosphatic manures is probably attributable to increased digestibility, and consequent higher productive index of the produce.

In a recent paper[1] the author gave reasons for the conclusion that 60 per cent.

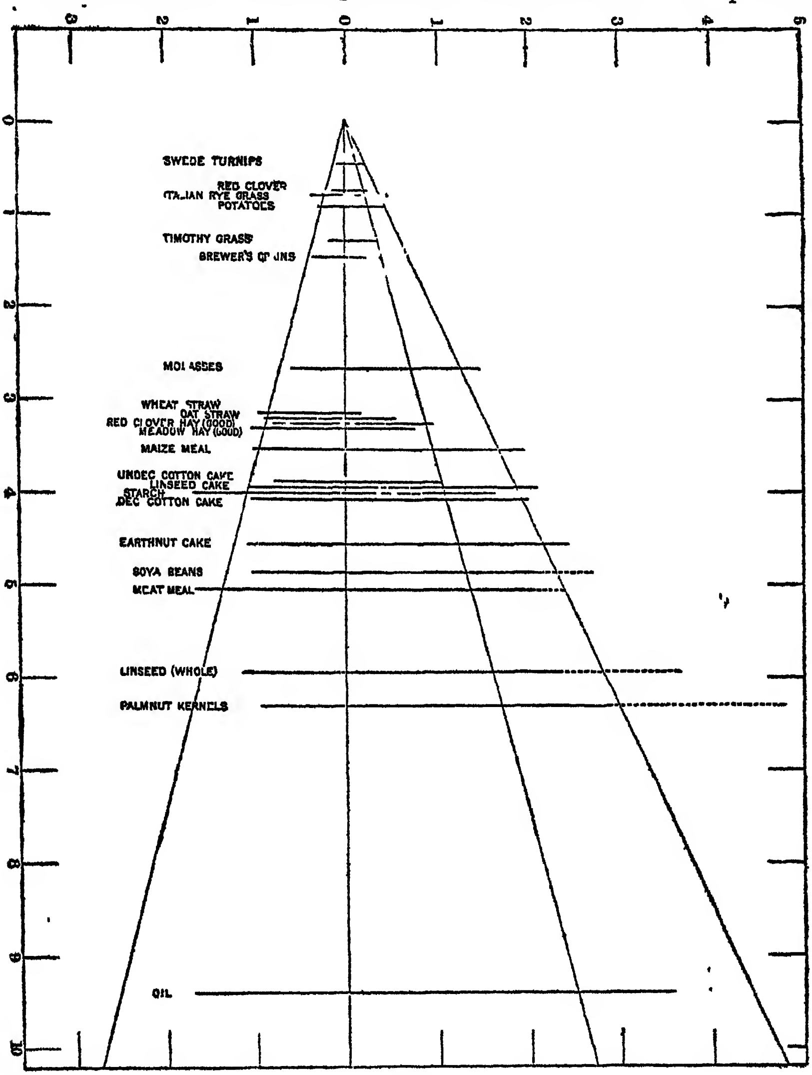

FIG. 2

was about the largest amount of body fat an ox could put on. It now appears that, if the productive index of the rations had been lower, the limit attainable would have been less. On the other hand, pigs, which can subsist normally on rations composed entirely of foods which have a very high productive index—e.g.,

[1] *Jour. Agric. Sci.*, vol. ix, No. 2.

peas, maize meal, barley meal, etc.—may be able to attain to a higher degree of fatness than ruminants, which require a certain amount of hay and straw. A lower capacity for food would, of course, counteract this condition.

DETOXICATED VACCINES (David Thomson, O.B.E., M.B., D.P.H., Lecturer and Researcher on Venereal Pathology to the London Lock Hospital, Dean Street, Soho).

OUR knowledge regarding immunity towards pathogenic bacteria is slowly but surely progressing. It would appear, indeed, that the time is not far distant when, by means of vaccines, we will be able to master perhaps the majority of the infectious diseases which cause so much misery and so much loss of life amongst mankind.

It has now been definitely proved that, when a number of dead bacteria are injected under the skin, a temporary inflammation is produced in the tissues, and thereby these tissues manufacture a certain amount of specific antisubstances towards the germs which were thus artificially introduced. These so-called antisubstances, which consist of "agglutinins," "precipitins," and "bacteriolysins," remain in the system for a considerable time, and by circulating in the blood they render the individual less susceptible to attack by that particular germ which was injected.

All germs are more or less toxic. In some cases the toxin or poison is excreted by the bacterium, but in the majority it apparently resides enclosed within the germ stroma, and is known as endotoxin. Most workers maintain that antisubstances cannot be successfully developed against endotoxins, so it would seem logical that we should try to rid vaccines of these poisons, since they are harmful and do not aid in immunisation.

Emulsions of dead germs are called vaccines, and we can make a vaccine of any micro-organism which we can isolate and grow artificially in pure culture. Since we can now grow pure cultures of the majority of pathogenic bacteria, there are in consequence a large number of different vaccines on the market. These are made up of such germs as cause typhoid fever, pneumonia, influenza, bronchitis, colds, meningitis, tuberculosis, puerperal fever, gonorrhœa, boils, abscesses, acne, etc.

So far the most strikingly successful vaccine is the typhoid inoculation. It has been proved beyond all doubt that an injection of 1,250,000,000 of dead typhoid bacilli, followed by another injection of 2,500,000,000 ten days later, gives the individual an almost certain guarantee of immunity towards typhoid fever for at least one year.

Preventative inoculations against other diseases, however, have so far been much less successful, partly due to the fact that many of the germs in question are too poisonous or toxic to permit of large inoculations. It is reasonable to suppose that, the larger the amount of dead germs injected under the skin, the greater will be the quantity of antisubstances produced against them by the subcutaneous tissues. When, however, a given germ is so toxic that we dare only to inject a very small amount of the vaccine, then we can hardly expect a successful immunity to be produced thereby.

In South Africa there occurs a very severe and fatal form of epidemic pneumonia amongst the negroes on the rand. In recent years attempts have been made to inoculate these negroes against this disease. It was not considered safe,

however, to inject a larger number of pneumonia germs than about 250,000,000 (at one time), and the amount of immunity conferred by this amount was insufficient to prevent the epidemics. Lately, however, one investigator has boldly inoculated the negroes with doses amounting to 5,000,000,000 pneumococci and upwards, with the result that the epidemics have been stemmed in a very striking manner.

From data such as the above, as well as from experimental researches on animals, it would appear that large inoculations are necessary if we wish to obtain a certain guarantee of immunity. To solve this important factor, it is obviously necessary to find some means whereby large doses of vaccines may be injected under the skin without harmful results to the individual.

Attempts have been made in the past to reduce the toxicity of certain germs by growing them on special culture media, or by preparing the vaccine in a certain way before use. The process of "sensitisation," which consists in treating the vaccine with a specific antiserum, has helped to reduce the toxicity in some degree, but on the whole it must be admitted that these attempts have been more or less unsuccessful.

I believe, however, that now this problem of detoxication of germs has been satisfactorily solved by a chemical process which I was fortunate enough to discover, and which was published in the *Lancet* on June 28th, 1919.

This discovery was originally made with regard to the gonococcus (the germ which causes gonorrhœa). About two years ago I was greatly puzzled because the gonococcus, when cultivated artificially, gradually disintegrated and died within a few days. The germ appeared to break up into small granules, a process which is known as "autolysis." After having been baffled for eight months as to the cause of this phenomenon, I eventually discovered that it was due entirely to the chemical reaction of the medium. So long as the medium was acid, no disintegration occurred in the germs; but, on the other hand, when the medium was alkaline they autolysed very rapidly. In other words, the gonococcus was insoluble in weak acids, but was very soluble in weak alkalis. It was discovered further, by means of a certain delicate test, known as the "complement fixation reaction," that the specific chemical characteristics of the germs were not altered or destroyed by the solvent action of the alkali.

Later researches showed that, when acid was added to an alkaline solution of gonococci, the bulk of the germ substance was thereby precipitated from the solution. It was also noted that, when the acid precipitate was driven down by centrifugalisation, the supernatant fluid was extremely toxic, and when injected under the skin it produced all the poisonous symptoms which result from an injection of the actual germs themselves. The acid precipitate, on the other hand, was less toxic than an equal bulk of the germs which had not been chemically treated as above. When the process of dissolving in alkali, precipitating with acid, and removing the supernatant fluid, was repeated about six times, it was found that the supernatant acid fluid became no longer poisonous, and the sixth precipitate was about one hundred times less toxic than an equal bulk of the original germs.

Picric acid (saturated aqueous solution) proved to be an excellent test for the presence of toxin in the supernatant acid fluid. So long as toxin was present the addition of the picric caused a milky cloud, and when this test became negative the process of detoxication could be considered complete. It was also found that the toxin could be thrown out of solution by the addition of concentrated sodium chloride. This is an important fact, since it points to the necessity of using weak alkali and weak acid in the later stages of the process, since the salt produced by the union of the two reagents might be sufficiently strong to precipitate the toxin.

I was, perhaps, the first to use the "complement fixation test" as a quantitative measure of the immunity produced by vaccine inoculations. The leading article in the *Lancet* on my work criticised this method of measuring immunity. Since the test, however, is really a mode of estimating "precipitins," which are anti-substances, I fail to see why this criticism was forthcoming, and my faith in the method remains unshaken. Anyhow, it was ascertained by means of this test that large inoculations of the detoxicated germ substance produced much more immunity than small doses of the toxic germs themselves. Moreover, clinical observations corroborated the scientific findings, since the curative results of the two vaccines showed that large doses of the detoxicated type were distinctly superior in treatment to the small doses of the ordinary toxic variety.

More recently I have applied the same chemical process of detoxication to a series of other germs, with more or less similarly successful results. It is quite obvious, however, that different germs vary very much in their chemical composition.

The influenza bacillus and the meningococcus are, like the gonococcus, extremely soluble in alkali; others, however, such as tubercle bacilli, staphylococci, and streptococci, etc., are very insoluble, and require the prolonged action of strong alkali containing a trace of chlorine. When once dissolved, these latter germs can readily be precipitated by acid like the others. The most difficult germs to pass through the process of detoxication are such organisms as the typhoid bacillus, *Bacillus coli*, and Friedlander's bacillus. These are dissolved readily enough in alkali, but they are extremely difficult to precipitate by the addition of acid. It seems a good rule that light germs which sediment slowly are very difficult to precipitate, and *vice versa*; heavy germs which sediment rapidly are easy to throw out of alkaline solution by acidification. The addition of sodium chloride to the acid aids the precipitation very markedly; but, unfortunately, as already mentioned, this tends to precipitate the toxin as well. In consequence the concentration of sodium chloride which can be used is limited to about 5 per cent. Alcohol aids precipitation in some cases, but it also precipitates the toxin, so its use is to be avoided.

With regard to the gonococcus, it was noted that about two-thirds of the germ-substance was thrown out of the alkaline solution by acidification, and that about one-third of the material (which is toxic in nature) remained in the supernatant acid fluid. On the other hand, in the case of such germs as the typhoid bacillus and *Bacillus coli*, it would appear that this proportion is reversed, since only about one-third is precipitated by the acid, while two-thirds remain in colloid solution in the acid supernatant fluid. Furthermore, these germs are not so readily detoxicated as the gonococcus, since the final precipitate obtained after repeating the process six times cannot be injected in such large doses as in the case of the latter.

It would appear, however, that all germs can be detoxicated successfully by the process in a greater or lesser degree. The only point of difference is that variations in the strength of the alkali are required to suit each organism. I feel confident that, with further researches on detoxication on the above lines, the problem of the administration of the large inoculations necessary in order to produce a high degree of immunity without harmful effects will be completely solved.

Vaccines have been employed not only for preventative, but for curative, purposes. Thus small doses of the appropriate germ may be given with good results in the course of a disease. In such cases, however, the amount injected must be smaller than in the case of preventative inoculations, which are administered during health. This necessity of giving tiny doses during an illness detracts

very much from the immunising and curative value of the vaccine. For therapeutic purposes, therefore, detoxicated vaccines should prove of the greatest value, because the immunisation of the patient can be attained without appreciably adding to the toxic symptoms resulting from the disease itself.

The following table shows approximately the comparative dosage of detoxicated and ordinary vaccines :

Germ.	Detoxicated Vaccine.	Ordinary Toxic Vaccine.
Gonococcus	2,500,000,000–10,000,000,000	25,000,000–100,000,000
Meningococcus	2,500,000,000–10,000,000,000	(?)
Staphylococcus	10,000,000,000	250,000,000–1,000,000,000
Influenza bacillus	500,000,000–1,000,000,000	50,000,000–100,000,000
Pneumococcus	2,500,000,000–10,000,000,000	200,000,000–500,000,000
Micrococcus catarrhalis	1,000,000,000–2,000,000,000	50,000,000–100,000,000
Mixed organisms for coryza	1,000,000,000–2,500,000,000	100,000,000–250,000,000
Tubercle bacilli	500,000,000–1,000,000,000	(?)

Researches with detoxicated coryza vaccines in the prevention and treatment of common colds, bronchitis, and so-called influenzal attacks holds out considerable promise of success.

There is still, however, another problem of great interest in connexion with the production of immunity by means of specific inoculations. In the course of my investigations on the vaccine treatment of gonorrhœa it was noted that the amount of antisubstance produced in different patients as the result of equal inoculations varied in a very remarkable manner. Thus, while some subjects developed ten units of immunity after the injection of a total of 40,000,000,000 of detoxicated gonococci, others only developed some four or five units even after a total of 80,000,000,000. Those who had the power of developing a large amount of antisubstances got better much more rapidly than those who showed a feeble capacity in this respect. This variation of power in producing antisubstances as the result of inoculations did not appear to me to have any very definite relationship to the robustness of the patient.

It is obviously very important that the cause of this variation in different individuals should be discovered. When we are able to inject large quantities of vaccine, and when, in addition, we know the conditions necessary for a powerful response of antisubstances, then our mastery over bacterial diseases will be to a large extent complete.

In recent years our knowledge regarding bacterial antibodies has greatly increased, so much so that it is possible to detect them in the blood, and even to measure the amount produced by a given inoculation. With careful quantitative researches, therefore, on these lines there is no reason why highly efficient and successful vaccines should not be obtained for almost all of the infectious diseases.

DUST INHALATION AND MINER'S PHTHISIS (H. W. Davies, M.B., B.S., Captain, Australian A.M.C.)

MINER'S phthisis is a disease which has long been recognised, and in certain localities, notably the Transvaal, is well known on account of its prevalence and deadly character. It is caused by certain dusts, mainly the various forms of pure silica, which accumulate in the lungs, setting up a fibrosis, and rendering them very much less resistant to infection by tubercle bacilli. Coal-dust, on the other

hand, does not appear to have this effect, and unless breathed in very large amounts is harmless, if not actually beneficial—coal miners in England have a lower phthisis mortality than agricultural labourers.

Many attempts have been made to explain the reasons for this difference, and the popular idea, even among medical men, is that the deadliness of silica is due to the hardness and sharpness of the particles. However, even coal particles are often sharp and angular, and also are moderately hard. Further, it is found that, if silica be inhaled mixed with coal-dust or with clay, it becomes relatively harmless, which would not occur if its harmfulness were due solely to hardness and sharpness; so a further explanation is necessary. Experiments have been carried out by Prof. Beattie at Sheffield and by Drs. Haldane and Mavrogordato at Oxford. Guinea-pigs were exposed to various kinds of dust, and after a certain time they were killed and their lungs examined. In all cases the dust was found to be present immediately after exposure, and was taken up by the epithelial cells of the lung alveoli. In the case of silica these cells mostly remained *in situ*, and eventually produced fibrosis; but in the case of coal they became detached and wandered out with their loads of dust (hence the "black spit" of coal miners), so that the lungs of a guinea-pig exposed twelve months previously to coal-dust appeared perfectly normal. That is to say, that coal and the other harmless dusts have the power of stimulating the epithelial cells while silica is inert. This stimulating power is probably due to adsorbed substances.

It has been found that, if a mixture of silica and coal be inhaled, the mixed dust enters the epithelial cells, as in the case of the unmixed dust; the cells become detached, as in the case of coal alone, and they wander out with their mixed load of coal and silica. That is to say, that the dangerous silica is rendered harmless by the addition of coal. This opens up a new method of dealing with dangerous dusts. By means of water-sprays and ventilation in mines it is possible to greatly reduce the dust. However, a small quantity remains which is still sufficient to do damage to workmen constantly exposed. Probably it will be possible, by adding coal to this remaining dust, to render it harmless, and just as stone-dusting is used in coal mines to prevent coal-dust explosions, so coal-dusting will be used in quartz mines to prevent miner's phthisis.

The experiments at Oxford were interrupted by war work, but have now been resumed in Dr. Haldane's laboratory at the expense of the Medical Research Committee. Dr. Mavrogordato is shortly going to South Africa in an advisory capacity, and during his absence the experiments are being carried on by the writer.

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ESSAY-REVIEWS

THE GHOST-HYPOTHESIS: on Spirit Experiences, by CHARLES A. MERCIER, M.D., F.R.C.P., F.R.C.S. [Pp. 54.] (London: Watts & Co. 1919. Price 9*d.* net.)

THE psychological formation which may be called Anglo-Saxon Conglomerate or Puddingstone extends from Deal to San Francisco, and consists of good stone embedded in friable alluvium or sandstone, useless for constructional purposes; but here and there we find outcrops of the finest crystalline granite, of which our most enduring monuments have been made. Science has long hoped to congratulate itself that Dr. Charles A. Mercier[1] was of the very best grade of the latter kind of rock. Combining the clean cleavage and logical hardness of a Huxley with the sparkle and invention of, let us say, the two Samuel Butlers, he has been for a long time, we think, the best of our writers on the scientific side—great because, unlike our wits of journalism, politics, and literary criticulism, he deals only or chiefly with great things which really matter. For years he has stood for truth and straight thinking, like a cromlech in the crumbling plains of Puddingstone around him. But now we buy his last book, rub our eyes, and fear that even he has fallen!

Here is the full title: "Spirit Experiences: the Conversion of a Sceptic! Startling and astonishing experiences of a seeker after truth—Unprecedented marvels—Telepathy—Levitation—Communications with the dead—Telergy—A completely novel experience—Substitution." Oh, dear! On opening the book we find that it begins with an apology for his previous book called *Spiritualism and Sir Oliver Lodge.* He admits that he was wrong, and that his eyes were opened by a review in the *Times* which said, "Objectors [to spiritualism] must study the subject as closely and carefully as the devotees. Otherwise they necessarily come into court convicted in advance of the sin of amateur criticism." The very same evening Dr. Mercier had an opportunity for testing this dictum. He thought that his pudding at dinner was burnt. But his cook denied it and said, "It cannot have been burnt, for I cooked it over the gas." He stood convicted of amateur criticism—for he had never cooked a pudding. The pudding, therefore, was not really burnt at all: and he summarises his conversion in the apothegm that "the proof of a pudding is [not in the eating but] in the cooking."

Nevertheless we do not agree with the dictum of the *Times* reviewer as a universal truth. There is such a thing as *a priori* improbability. For example, if a man were to tell us that on measuring a thousand triangles he had found that two sides of a triangle were together less than the third side, we should—well, try to change the subject. If he were to persist and to demand that we should measure another thousand triangles together with him, in order to convince ourselves of the truth of his statement, well, we (busy men) would decline to waste our time over such a foolish undertaking.

Statements and demands equally absurd have often been actually made, even in connexion with scientific work: but let us now consider a less extreme case. Suppose a man were to tell us that he had seen Mr. Lloyd George, Mr. Asquith

[1] We regret to say that Dr. Mercier died on September 2nd.

and Mr. Bonar Law walking arm in arm down Piccadilly, we should have no reason to disbelieve him. But if he were to add that they were dressed in the engaging costume of negro minstrels and were playing banjos, we should assume an *a priori* improbability and think that he was joking, or mad, or—well, a liar. If he were to claim that he could bring a dozen other eyewitnesses of this appalling indecorum to verify his statements, we should, of course, still refuse to waste our time over the inquiry and decide that our informant was merely a politician! Indeed, even if a dozen witnesses were to tell us the same thing, we should still refuse to believe any or all of them—so great would be our respect for the three gentlemen named. Now, all spiritualism is in the same case—there is a fundamental *a priori* improbability attached to it. That improbability is even greater than the one attaching to the supposititious promenade of the three statesmen (who after all are quite free to walk down Piccadilly playing banjos if they wish); and it is nearly as great as the improbability attached to the singular behaviour of the thousand triangles. We disbelieve all three statements. If we are pressed to examine evidence we reply, "The *a priori* improbabilities of your hypothesis are almost infinite, and we do not think that you have even made out a *prima facie* case why we should waste our time over conducting any investigations on the subject at all." And, be it noted, men of science are frequently forced to give precisely the same reply when urged to investigate scores of other wild speculations besides those of spiritualism—such as the centrality or flatness of the earth and the potency of every imaginable drug to cure every imaginable disease. There are many propositions which require no further investigation, whatever their "devotees" may say, because they obviously lead at once to deductions which will not fit in with the facts around us. We do not generally trouble to argue the point, because we recognise that the devotees belong to the numerous class of people who are naturally incapable of thinking straight. For instance, it is not possible to believe the stories regarding the three Ministers or the thousand triangles; but it would take some time to explain the grounds of our disbelief to the tellers of those stories, who we think should already possess enough sense to recognise the folly of their proposals. So also with spiritualism.

Very few of our acquaintances claim to have seen ghosts; but we have read of ghosts who wore winding-sheets, chain-mail, or bob-wigs, and who carried daggers, swords, pistols, candles, and bleeding hearts. Stop—let us inquire further. If we believe these stories, then we must believe, not only in ghosts of the human form, but also in ghosts of linen, hair, iron, and tallow—we return to the profound philosophy of Swedenborg, who said that everything had a ghost. If we refuse to accept this deduction, we must believe only in naked ghosts; but here, again, we are in a corner, for a man's hands, feet, face, and body are only material parts of him. If only the soul of a person has a ghost, how can we ever recognise, say, the ghost of our wicked stepmother when she brings us poisoned gruel? Her ghost can have no resemblance to her living form, unless we admit that mere material bodily adjuncts have ghosts or may form parts of the ghost-entity. Then again, how is it that these particular ghosts have the power of appearing to our vision by producing or reflecting light-vibrations, or of speaking to us by means of sound-vibrations, both universally associated in human experience with matter? There are some thousand million people now living; so that, if ghosts exist at all, we should assume that millions of millions of ghosts have come into being during the last millennium alone, not to mention the ghosts of their inanimate habiliments and the ghosts of animals, etc. Where are they all? How is it that only such a very few of them can haunt us—or, rather those of us who like to see ghosts?

Only "restless spirits" appear before us! Ah! we fear that in this case every one of us should see one ghost at least every night. Then, why is it that ghosts do not appear to us in broad daylight, and let us examine them thoroughly, and take photographs of them? And, above all, when they speak why do they not say something great or useful? Ordinary persons like ourselves cannot answer these questions. Frankly, we do not try to. We find it easier to believe that those who tell us that they have seen ghosts are—well—not telling the truth. That is old Hume's strong position.

Then there is the whole body of evidence suggesting that mind is only "the secretion of the brain"—evidence which has not been even damaged by the modern pseudo-philosophers, who are out to prove themselves above the order of nature. We witness the development of mind in ourselves and in our children; we observe its evolution throughout the animal kingdom to mankind, and even throughout mankind from the lowest to the highest types; and we see it profoundly affected or even destroyed by material agencies. It is depraved almost out of recognition by various diseases and malformations of the brain; stimulated by good food and health; weakened by starvation, fatigue, and age; held in abeyance by opium and other drugs, and by natural sleep; excited, maddened, or abrogated by alcohol; improved by education and success; soured by failure, and even by indigestion; and finally knocked out altogether, temporarily or permanently, by such a simple process as a knock on the head. And yet our ghost-seers say that it is not of the body at all; that it is able to live independently, retaining its vast stores of memories, passions, feelings, judgments, and convictions, its muscular powers (to lift tables and chairs at séances), and its powers of utterance, all without food, body, muscles, or tongue; and that it even possesses additional powers to those which it possessed before, such as the powers of generating light, passing through walls, writing on slates in locked boxes, and communicating with living people through infinite distances of ether! True, the wily modern spiritualist is trying to abandon visible ghosts altogether (because of the difficulties mentioned above), and contents himself with "communications from the dead" through living "mediums." But the same difficulties remain; whether visible or not, the pretended ghost is supposed to retain his powers of body, language, memories, loves, and hates, and all without the substances and organs which make these things possible in life. Now, it is a universal experience that a given result is produced by one, and only by one, complex of causes. Take our friend Mr. Jones, for example. He is built up by a great complex of bone, flesh, blood, clothes, and banking account; his memory is stored with fifty years of experiences; and every action and word of his is based upon and conditioned by those experiences. We have never met another Mr. Jones, and do not believe that he exists. If another person were to claim to be he, the law would put him into gaol. But now we are told that certain particles of ether, without his body, brain, and banking account, can build up another but ghostly Mr. Jones somewhere in supernal space around us!

All this is hard to believe in—as hard to believe in, let us say, as the ghost of a candle after it is blown out, or the ghost of a steam-engine continuing to run along the rails on dark nights without coal, after its frame, cylinders, and wheels have been broken up. So strong are our *a priori* objections that we must have very strong evidence to overcome them. If a fisherman tells us that he has caught a thirty-pound pike, we are quite ready to believe him; but, as the weight of the pike increases, so does the weight of the evidence required to overcome our incredulity—and in something like an exponential ratio. Thus, when the pike

becomes a whale the evidence must be as weighty, let us say, as the star Sirius. So, also, when Jones becomes a ghost.

Now, as to the evidence actually "obtained" by the spiritualists, most people who have read accounts of it—as, for instance, in Mr. Edward Clodd's very crushing book, *The Question* (Grant Richards, 1917)—conclude (1) that all the miracles and tricks ascribed to spirits and mediums in séances are not a wit more surprising than those performed by professional conjurors merely to amuse audiences; (2) that as many of these mediums and tricks have been shown to be impostors and frauds, so probably are all the rest; and (3) that quite a common class of persons will say or do anything in order to obtain notoriety—or money. The weight of this "evidence," compared to what is required to overcome the weight of the *a priori* objections, is as the weight of a hair to that of the said star Sirius. If only these ghosts would come out and have a plain talk with us on a sunny day in an open field, or say in Hyde Park, what a lot of trouble would be saved? And why, also, cannot the miracles of clairvoyance, telepathy, etc., be performed under the same conditions? Yet any poor Indian juggler will do much more wonderful things in the open—and so will many a "professor" on an English race-course.

We therefore continue our perusal of Dr. Mercier's book with misgivings. He claims that his evidence is much more veridical than that of Sir Oliver Lodge. He certainly succeeded in transferring a thought from his own mind to the mind of another person—which we have seldom been able to do—and that right through a brick wall. He has seen instances of levitation of the most surprising kind—a solid object dematerialised in one place and materialised again in another. He must be congratulated on possessing the acquaintance of the two young telepathic mediums; but not so much on meeting the two gentlemen in the railway carriage; because, not only did they quite overcome his stupid, bigoted, benighted, dull, cowardly, unreasoning, scientific scepticism, first by means of three cards held on the tips of thumb and fingers of one hand, and then by means of certain thimbles and peas placed on a flat surface, but they also made him lose £53 in consequence of his enthusiasm for scientific investigation! But the reader must buy the book for himself. He will then find (thank Heaven!) that Dr. Mercier has not fallen after all, but that the granite is as true—and as sparkling—as ever! What the book succeeds in doing is to show that the so-called Psychic Phenomena are, millions to one, nothing but Prestigiatory Phenomena—as we have just suggested. Oh, that some conjuror would only write a book explaining his amusing art! We should then, also, obtain a full scientific explanation of our much less amusing ghosts.

Seriously, there is still room for a good essay on the evolution of the Ghost-Hypothesis. We doubt its origin in the personification of nature-forces. Much more probably it was found useful during the vast and dark ages of tribal evolution which gradually moulded man before the historic era, in overcoming the fear of death in battle, and was expressly inculcated for that very reason by the sages and minstrels of the dawn. It must also, certainly, have been a powerful deterrent against cruelty, oppression, and murder, when there were no policemen; for the criminal always had to fear his victim's ghost, if not his victim's friends. Literature is full of both these themes. At its best the Ghost-Hypothesis has probably done much for humanity, both as the enemy of vice and the friend of virtue; and we still see the good of it as an anodyne for bereavement and a stimulant for noble effort. In fact, its action has been very similar to that of alcohol or opium; and men still accept it for a similar reason, because it is pleasant and comforting to them. They believe in it because they wish to believe in it; they invent

evidence and pervert argument in order to convince themselves; and even maintain that it is true because it is pleasant.

But there are no grounds for supposing that what is pleasant must therefore be true. It is quite possible that truth may sometimes be dangerous and untruth beneficial. The Ghost-Hypothesis may have been useful in the past; but, now that men are beginning to emerge from barbarism, it is, we think, becoming less and less useful to us. As we grow up we should try to face facts more firmly. Every adult ought to see this quite clearly; and we do not admire those who still try to force the superstitions of the Middle Ages upon us.

Curiously enough, nearly the whole of this modern form of superstition emanates from the Anglo-Saxon Conglomerate—we find little of it among the older civilisations of the Latin Races, or even among the Germans. Now, the nature of the Puddingstone is that it is good in places but utterly soft in between; and we therefore see many people among us who are thoroughly sound and reasonable in the ordinary affairs of life, but who may break down in a minute into spiritualism, theosophy, telepathy, anti-vivisection, or a hundred other similar weaknesses! We understand that most of our "mediums" come from the United States—and thank Heaven for that, at least! Probably no other race but ourselves would be so gullible as to believe in anything they may see done at séances in darkened rooms, previously prepared for their deception. Probably, then, this superstition is based, not on any body of ascertained fact or reasoned argument, but on a widespread racial mental weakness, due to nature or to bad education.

As fact, the Ghost-Hypothesis is nothing; as allegory, it is everything. It has been associated in the past (for good reasons) with the greatest Teachings and the wisest Philosophies; but these can now stand by themselves. The Moral Law is based, not on any form of fabulism, but upon the broadest Common Sense. Our ghosts are our deeds, which for good or evil live after us; and our heaven and hell are those which we make for ourselves. Spiritualism is a debased version of these high truths. It is debased because it ascribes actuality to what are really parables invented by the Fathers of Humanity to guide us in our conduct, and because it does so in order to please its pupils by attempting to assuage the fear of death. We shall do well to follow in this matter the wise and beautiful mind of Matthew Arnold. Men are not divine by nature, but may become so by wisdom. So long as they continue to believe themselves divine by nature they will fail to become divine by wisdom. Why? Because they will then continue to lie to themselves. That is why spiritualism is not of Good, but of Evil.

THE WEIGHING OF WITS, by GERALDINE E. HODGSON, Litt.D., on

The Measurement of Intelligence. An Explanation of and a Complete Guide for the use of the Stanford Revision and Extension of the Binet-Simon Intelligence Scale, by LEWIS M. TERMAN, Professor of Education in Leland Stanford Junior University. [Pp. xvii + 362.] (London: G. G. Harrap, 1919. Price 6*s*. net.)

Test Material for Measurement of Intelligence, by LEWIS M. TERMAN. 18 cardboard sheets (London: G. G. Harrap, 1919. Price 3*s*. 6*d*. net.)

RECENTLY, Pedagogic Journals have been richly furnished with articles bristling with diagrams and statistics. Novelty-lovers buzzed round them; older stagers

shrugged their shoulders and wore that gently derisive smile which is so irritating; a few scoffed openly; and the rest, surmising that it was "some new dodge for finding out whether you are stupid or not," placidly left them uncut.

Now, Prof. Findlay claims that, though Binet-Simon Method textbooks have appeared, "this work by Prof. Terman is the first comprehensive and detailed account prepared for the use of educators in dealing with school-children" (Preface, p. v).

Unfortunately, dealings with red tape nowadays curtail some teachers' opportunities and energies for dealing with their proper objective—children. But if, as Prof. Findlay suggests, these methods are upon us, let them be weighed, for if unsound they may prove disastrous, educationally and socially.

Part I of this book describes, in seven chapters, the Binet-Simon theory with its "Stanford-revision," attempting to justify its use in general, and its results in particular. Part II presents carefully, and rather tediously, tests for children at each year between 3 and 14; for the "average" and for the "superior" adult.

The "Material," eighteen cardboard sheets, consists of pictures, a colour scheme and diagrams, to test observation, judgment, memory; the latter, perhaps, overrated as a sign of intelligence—certainly Montaigne would say so.

It is fair to note that Prof. Findlay expressly states that "those who use the tests make no claim that the entire mental life of the subject is comprehended within the scale"; and again, "Just as the chemist analyses a sample of milk, and then sends his report to the competent authority, so the Binet-Simon tests enable the psychologist to report on certain facts: the educator takes his own responsibility in attaching due weight to these" (p. viii). Also, Prof. Terman says: "Before offenders can be subjected to rational treatment, a mental diagnosis is necessary; and while intelligence tests do not constitute a complete psychological diagnosis, they are, nevertheless, its most indispensable part" (p. 11); and, "It would be a mistake to suppose that any set of mental tests could be devised which would give us complete information about a child's native intelligence" (p. 135); still more forcibly, "Once more let it be urged that no degree of mechanical perfection of the tests can ever take the place of good judgment and psychological insight. Intelligence is too complicated to be weighed, like a bag of grain, by anyone who can read figures" (p. 136). Unfortunately, as so often happens with propagandists, admissions, qualifications and disclaimers being made are afterwards, apparently, forgotten or abandoned.

But even with such admissions, the book's philosophical defect is its failure to appreciate the impossibility of measuring intelligence in isolation, simply because it does not exist so. Biology, physiology, ethics, economics, sociology—where does the list end?—claim their shares, because the human being, it is a platitude, is not only "native intelligence," but an amalgam of thought, will, feeling, interacting, interdependent, and, moreover, swayed, changed by times and places. This underrating of the extreme, often conflicting, complexity of human nature persists through the book. An excellent example is Prof. Findlay's simile of the sample of milk, also used by Prof. Terman. Yet no child is so homogeneous with other children as is a sample of milk with the rest of which it was once a part; nor—an incalculable enhancement of difficulty—is any child so homogeneous with itself, there being more continuous stability of contents in milk than in any child, even the most immovably stolid.

Prof. Terman blames a teacher for stupidity in not recognising that a child, in school for months, is "deficient"; yet, what of his own state when he proposes that a period, from 25 to 60 minutes, taken at random, in artificial circumstances,

shall, by the child's performance, serve as a test to settle its grade of intelligence, and possibly its future chances? The poorest teacher would refuse, by one brief test, to appraise any

child of a thousand chances 'neath the indifferent sky.

What would an alienist say of such methods?

Prof. Terman talks about "establishing *rapport*" with the victim of his research, lightly allotting "three to five minutes in a majority of cases" (p. 124); but children's confidence is not so easily won.

The following passage throws a curious light on his view of children: "Nothing contributes more to a satisfactory *rapport* than praise of the child's effort. Under no circumstances should the examiner permit himself to show displeasure at a response, however absurd it may be. In general, the poorer the response, the better satisfied one should appear to be with it. An error is always to be passed by without comment, unless it is painfully evident to the child himself, in which case the examiner will do well to make some excuse for it—e.g., 'You are not quite old enough to answer questions like that one; but, never mind, you are doing beautifully, etc.' Exclamations like 'Fine!' 'Splendid!' etc., should be used lavishly. Almost any innocent deception is permissible which keeps the child interested, confident, at his best level of effort" (p. 125). Quite a little observation of children will disclose the fact that, ordinarily, they possess penetrating *flair* where their elders are concerned. Combined patronage and hypocrisy would be likely to evoke wide-eyed contempt from average children.

Prof. Terman pours considerable scorn on teachers, parents, and physicians; but has it, one wonders, occurred to him that there are such things as temperamental compatibility and its opposite? Not every two mortals "suit" each other, even though they be teacher and taught. Suppose a "Stanford" examiner did not chance to "suit" the youthful experimentee. What a disaster if, through such incompatibility, it passed out of the laboratory labelled "deficient," or—deplorable title to ordinary ears—"a moron."

In Chapter II, Prof. Terman describes three teachers' attempts to judge five children not known to them. He complains that they were "reduced to the much-abused test methods," and declares that they "employed very awkwardly a very excellent method," adding "the test method is but a refinement and standardisation of the common-sense approach." But if it be, teachers do not claim to gauge a child's intelligence by one trial; "standardising" questions will not make human nature suitable for "exact" methods. Possibly sweet-pea plants, guinea-pigs, blue mice are sufficiently alike to warrant the application to all of conclusions drawn from observation of a few; but "pet" animals whose development is changed and diverted by human friends will serve the purpose poorly. That human beings escape these precise tests is shown by the rough-and-ready surmises we make about each other, our blunders crystallising in such familiar grumbles as "I should never have expected So-and-So to do that." The nearer we approach to mathematics, the farther we recede from essentially human life. For example, in J. S. Mill's philosophical hands Political Economy remained "human." With his mathematical predecessor, Ricardo, and with many of his successors who used "exact" methods, it gained an appearance of cold relentlessness which may have contributed to its unpopularity. The properties of plane figures, the constituents of a chemical compound, the main plant characteristics permit, in their several degrees, of the application of "exact methods"; but not

Amorist agonist man, that, immortally pining and striving,
Snatches the glory of life only from love and from war;
Man that . . .
Seemeth so easy to shatter, and proveth so haid to be cloven;

. . . . .

Man whom Fate, his victor, magnanimous, clement in triumph,
Holds as a captive king, mewed in a palace divine.

Those restorations which avail to turn an abstract into a concrete science are impossible in the "human sciences," where, in some degree, each man is a law to himself. Not without reason has someone—was it not Dr. Schiller?—suggested that the desideratum is "as many individuals, so many metaphysics."

Further, is Prof. Terman justified in implying the existence of a fixed standard of correctness and error? Handling replies to "test-questions"; he labels them right or wrong. Yet, belauded for their definiteness, they seem, not seldom, to admit of a variety of shades and differences in reply. The test for an "average adult," whatever that may be, is—"What is the difference between laziness and idleness?" Prof. Terman judges the reply on strictest etymological lines. But words, long used, acquire meanings: dictionaries, not alone English ones, admit these as synonyms. It may be inaccurate, but it is not "unintelligent," to say "So-and-So is an idle fellow," meaning that he could work, but won't. Moreover, words do not abide alone; theories grow round them. What delicate entertainment might accrue from a symposium on laziness and idleness, say by Mr. Bertrand Russell, Lord Northcliffe, Mr. Smillie, and the Duke of Northumberland, with perhaps Mr. Bernard Shaw as Official Critic. It comes to this, man is a live creature, not an equilateral triangle, and the science of him is inexact; very useful in capable hands, but inexact. Between abstract and concrete is a great gulf fixed. Doubtless, *cæteris paribus*, $2 + 2 = 4$; but in the concrete what sort of "four" can one make of two elephants and two stars? Can genuine Science afford to watch the establishment of methods whose affinity seems closer to some noisily-advertised systems of "Wisdom while you wait" than to serious Psychology? It is difficult to believe that it can.

Theory-mongers often write as if they were pioneers, and not all share Pestalozzi's frankness in boldly eschewing all forerunners. Prof. Terman warns us against the deceptions of superficial quickness, which Ascham had done inimitably, centuries ago, in his famous comparison of "quick wits" and the "staffish."

A philosopher should not generalise rashly—e.g., "the ability to comprehend and use language is one of the most reliable indications of the grade of mental intelligence" (p. 143). Is it? Some of us are naturally glib; more acquire fluency by training and practice; some, quite able people, never attain to it. Again, "All of us in early childhood lacked moral responsibility. We were as rank egoists as any criminal" (p. 11). All! Were we all really as bad as that? A famous sentence about the difficulty of "drawing up an indictment against a whole people" flits through the mind. Of humanity, Burke, like Talleyrand, might have said "I know the beast."

Why, being a psychologist, does Prof. Terman care, apparently, so little for "Interest"? These tests, flung suddenly at children wrenched from their familiar surroundings, hardly to be reassured by "a reasonably small room," furnished simply with "a table and two chairs" (p. 122), lack interest of any sort: mostly, they are as flat as ditchwater, monotonous in their avoidance of any effort to gauge literary or æsthetic power; tedious in their diurnal utilitarianism.

Finally, if success ever attended the effort to force any considerable part of the population through such an examination, there is danger of grave injustice through mistakes; injustice to individuals through which social disturbance might arise. England is obviously wearying of officialism. The establishment of an autocratic educational bureaucracy with power to compel an examination of "capacity," and to settle individual careers accordingly, might bring disaster reaching far beyond the educational world. That the author might not shrink from such a plan is suggested by the book's distressing lack of humour. For instance, in the melancholy satisfaction which escapes round the recital of the life-histories of the deplorable "Kallikak," "Nam" and "Jukes" Families, it never seems to strike him that to embark on life's voyage with such patronymics is, in itself, no negligible handicap.

Yet teachers may lose something if they leave the book unread. At least, it shines in comparison with some of its kind, in containing definite, comprehensible matter. It is not, as many are, interminable verbiage wound round shadowy hypotheses. Its propositions may be disputable, its mistakes apparent; but it is suggestive, even if sometimes only of what not to do.

REVIEWS

MATHEMATICS

Synopsis of Linear Associative Algebra. A Report on its Natural Development and Results reached up to the Present Time. By JAMES BYRNIE SHAW, Professor of Mathematics in the James Millikan University. [Pp. 145.] (Washington, D.C.: Published by the Carnegie Institution of Washington, 1907.)

ALTHOUGH this "Publication No. 78" of the Carnegie Institution was published many years ago, it is interesting and important to review and read it at the present time, both for its own sake and because it throws light on a recent book by Prof. Shaw on the philosophy of mathematics, which is reviewed elsewhere in the present number of this Journal. The *Synopsis* aims to set forth the present state of the theory of linear associative algebra, "not in a comparative study of different known algebras, nor in the exhaustive study of any particular algebra, but in tracing the general laws of the whole subject" (p. 5). In view of the historical and critical work referred to on p. 5, no historical review is given in this memoir. However, a bibliography to some extent takes the place of a historical survey, and the bibliography (pp. 133–145) to this work is long and fairly complete. It would have been preferable to refer to some of the numerous papers by Grassmann as well as to the two separately-published books of this author; there are other relevant papers, and at least one book, by Peano besides those mentioned; it would surely have been better to refer more in detail to the papers of Hamilton and Sylvester than merely by the words "numerous papers"; De Morgan's work ought to have been noticed, and surely Weierstrass did not publish a paper in English!

Prof. Shaw's memoir is divided into three parts: "General Theory," "Particular Algebras," and "Applications." In the first part "is given the development of the subject from fundamental principles, no use being made of other mathematical disciplines, such as bilinear forms, matrices, continuous groups, and the like" (p. 5), and the other two lines of development of linear associative algebra are then described (cf. p. 6). In the last part there is "a sketch of the theory of general algebra, placing linear associative algebra in its genetic relations to general linear algebra" (p. 7). Here it is that we meet those views which explain the attitude of the author to work on the logical foundations of mathematics. "The foundations of mathematics," we read on p. 75, "consist of two classes of things, the *elements* out of which are built the structures of mathematics, and the *processes* by which they are built. The primary question for the logician is: What are the primordial elements of mathematics? . . . To the mathematician these elements do not convey much information as to the processes of mathematics. The life of mathematics is the derivation of one thing from others, the transition from data to things that follow according to given processes of transition." Of course, the logical relations between the entities of mathematics are included among what Prof. Shaw calls the "elements"—though Prof. Shaw hardly seems

to recognise this—and the logician is naturally concerned with mathematics as a logical structure. It is no disparagement to him as such—though Prof. Shaw seems to think so—that the psychological incidents of discovery are excluded by the logician as irrelevant, for the same reason at bottom as information about the diet of mathematicians is excluded from a book on mathematical theory.

PHILIP E. B. JOURDAIN.

Empirical Formulas. By THEODORE R. RUNNING, Associate Professor of Mathematics, University of Michigan. [Pp. 144.] (New York: John Wiley & Sons; London: Chapman & Hall, 1917. Price 7*s*. net.)

THIS is No. 19 of the *Mathematical Monographs* edited by Merriman and Woodward. "In the results of most experiments of a quantitative nature, two variables occur . . . [such that] on plotting the sets of corresponding values . . . the points so located lie approximately on a smooth curve. In obtaining a mathematical expression which shall represent the relation between the variables so plotted, there may be two distinct objects in view, one being to determine the physical law underlying the observed quantities, the other to obtain a simple formula, which may or may not have a physical basis, and by which an approximate value of one variable may be calculated from a given value of the other variable. In the first case correctness of form is a necessary consideration. In the second correctness of form is generally considered subordinate to simplicity and convenience. It is with the latter of these that this volume is mostly concerned" (p. 9). The first five chapters are on the simple tests which may be applied to a set of data, "and which will enable us to make a fairly good choice of equation" (p. 11), and the calculations of the values of constants in these empirical formulæ, and the methods employed, are almost wholly graphical. Chapter VI is devoted to the evaluation of the constants in empirical formulæ by the method of least squares; in Chapter VII formulæ for interpolation are developed and their applications briefly treated; and Chapter VIII is devoted to approximate formulæ for areas, volumes, centroids, moments of inertia, and a number of examples are given to illustrate their application.

The purpose of the sixth chapter "is not to develop the method of least squares, but only to show how to apply the method to observation equations so as to obtain the best values of the constants" (p. 90). We have the theorem (p. 19): If two variables, x and y, are so related that, when values of x are taken in an arithmetical series, the nth differences of the corresponding values of y are constant, the law connecting the variables is expressed by the equation $y = a + bx + cx^2 + \ldots + qx^n$. A case ($n = 2$) of such a relation is worked out (pp. 90–4), so that ten observation equations are found for various x's and the corresponding y's, from which we have to obtain three equations which will yield the most probable values of the three unknowns a, b, and c. By the method of least squares such a way of combination is known (see p. 97), and there remains only the problem of calculation.

Frankly, it is difficult to see what scientific purpose is served by an investigation of an (x, y) graph when "simplicity and convenience" are more important than "correctness of form" (p. 9): surely *the* object in all such investigations is to find, at least approximately, the form of the functional relation connecting x and y. However, this book will undoubtedly be found useful to those confronted with the problem of deciding upon a suitable relation between x and y and determining the constants.

PHILIP E. B. JOURDAIN.

Differential Calculus for Colleges and Secondary Schools. By CHARLES DAVISON, Sc.D., Mathematical Master at King Edward's High School, Birmingham. [Pp. viii+309.] (London: G. Bell & Sons, 1919. Price 6s.)

THIS good but somewhat conventional addition to the "Cambridge Mathematical Series" is divided into two parts, the first dealing with the principles, and the second with the applications, of the differential calculus. But the author wisely points out (p. v) that he does not suggest that this order should be adhered to in reading the book. The first part consists of chapters on the differential coefficient, differentiation of certain functions, successive differentiation, the expression of functions in power-series, and the evaluation of indeterminate forms. The second part deals with maxima and minima, and the usual geometrical applications, including singular points and curve-tracing in rectangular and polar co-ordinates. A very welcome feature is the suggestion (pp. 246 *sqq.*) of essays on rather more general aspects of the subject than are included in ordinary examination questions.

In the treatment of Rolle's theorem (p. 61), the conclusion that a continuous function which is first negative and then positive "must pass through the value zero for some value of x" relies implicitly on a crude geometrical notion of the continuity of a function. It is, of course, important that a student should be shown that all concepts in mathematics start from crude notions, but he should also be shown that concepts must be distinguished from the crude notions.

PHILIP E. B. JOURDAIN.

Projective Vector Algebra: an Algebra of Vectors independent of the Axioms of Congruence and of Parallels. By L. SILBERSTEIN, Ph.D., Lecturer in Mathematical Physics at the University of Rome. [Pp. viii + 78.] (London: G. Bell & Sons, 1919. Price 7s. 6d. net.)

THIS very original little book was at first intended as a paper, and its purpose is to construct, by means of the axioms of connexion and of order alone, a very simple algebra of vectors that is to embrace only the equality, the addition, and hence also the subtraction, of vectors (pp. 1–2). "There is no essential difficulty in introducing also an appropriate vector multiplication of two vectors. This, however, besides being superfluous, does not share the remarkable simplicity which will be seen to belong to the proposed vector addition, and did not, therefore, seem sufficiently interesting to be given in this little book" (p. 2). In fact, the whole of projective geometry falls within the scope of an algebra in which multiplication is not defined for vectors (p. 38).

Starting from the postulates given by F. Schur in his *Grundlagen der Geometrie* of 1909, and the most essential Desargues' theorem (pp. 2, 7–8),—indeed, the reader requires hardly anything more than the knowledge of this theorem as a consequence of the axioms of order and of connexion (p. 4),—the addition of two coinitial vectors is defined without the concept of "equality" of vectors being introduced (pp. 4–5). With this definition, which depends on two arbitrary points on the lines representing vectors, a most valuable extension of the usual definition of addition is obtained; indeed, Dr. Silberstein's definition of vector sum "is manifestly a generalisation of the Euclidean one, and is valid also for such spaces in which there are no Euclidean parallels nor even Lobatschevskyan parallels or asymptotic lines" (p. 5). The associative law is then proved (pp. 6–9), and the sum of collinear vectors defined (pp. 9–18). The concept of

equality" of non-coinitial vectors is explained (pp. 21–27), and the use of the vector algebra described further treated (cf. pp. 37–40). Finally the book contains a treatment of vector equations of straight lines and planes (pp. 40–41), some examples (pp. 41–49), a proof of Pascal's theorem (pp. 49–52), and a treatment of ranges and pencils of conics (pp. 52–66). There is an Appendix (pp. 67–76) and an Index.

The author remarks (p. 2) that it will not be necessary to subdivide the contents of the book into a formally logical array of numbered definitions, axioms, lemmas, theorems, and corollaries, and, from a formal point of view, objections might possibly be made to this exposition. But it must be acknowledged that of late years the tendency to put mathematical work into formal shape succeeded in hiding under a cloak of pedantry the essential nature of any advances that may have been made by the symbolists. This is one of the many points for which all mathematicians will value Dr. Silberstein's excellent and stimulating work.

In conclusion, it may be mentioned that the promise made in the note on p. 2 has been fulfilled in Dr. Silberstein's "Further Contributions to Non-Metrical Vector Algebra" in the *Philosophical Magazine* for July 1919.

PHILIP E. B. JOURDAIN.

ASTRONOMY

Navigation. By HAROLD JACOBY, Rutherford Professor of Astronomy in Columbia University. Second Edition. [Pp. xi + 350, with 22 figures.] (New York: The Macmillan Company, 1918. Price 11*s.* 6*d.* net.)

THE study of navigation received a tremendous impetus in the United States, after their entry into the war, and many professional astronomers were employed instructing officers for the mercantile marine. The volume under review, the first edition of which appeared in 1917, is an outcome of this development. It is an admirable little volume, written with remarkable clearness, and with a full explanation of the practical methods. Formal mathematical or astronomical knowledge is not assumed, and though some readers may object to the quoting in the text, without proof, of various formulæ, those who have mathematical ability can either prove the formulæ for themselves or find proofs in the usual textbooks. That a second edition is already called for shows that the volume has met a real need. The contents include an account of methods of dead reckoning without and with logarithms, descriptions of the compass and sextant, details of coastwise navigation, an explanation of the pages of the Nautical Almanac, and a very lucid account, first of the methods of the older navigation (by noon-sights and time-sights), and then of the methods of the newer navigation (by the Sumner-line, including Saint Hilaire's improvement). The latter possesses an enormous advantage over the older methods, yet the writer was surprised to learn recently that on some, at any rate, of the largest vessels in the British Mercantile Marine, the old time-sight and noon-sight methods are still employed. This volume can be thoroughly recommended to those who desire a clear exposition of the new methods. The final chapter is entitled, "A Navigator's Day at Sea," and gives an account of an imaginary voyage from New York to Colon with all the observations made *en route* worked out in detail.

At the end of the volume are given certain (abridged) nautical tables with a view to making the volume complete in itself, so that with its aid a vessel could be navigated without other books or tabular works excepting only the Nautical

Almanac. An appendix on Compass Adjusting has been added to this (second) edition.

H. S. J.

Textbook on Practical Astronomy. By GEORGE L. HOSMER, Associate Professor of Topographical Engineering, Massachusetts Institute of Technology. Second Edition. [Pp. ix + 205, with 78 figures.] (New York: John Wiley & Sons; London: Chapman & Hall, 1917. Price 9s. 6d. net.)

THE first edition of this work appeared in 1910, the purpose of the author being to meet the needs of students of civil engineering, who have not sufficient time to devote to the advanced study of astronomy, but who require more than the short chapter usually given in textbooks on surveying. The book therefore deals mainly with the class of observations which can be made with surveying instruments, and the author has wisely omitted reference to those requirements which, though of vital importance in precise astronomy, are beyond the accuracy of an engineer's observations, and would only tend to confuse the student.

The author has kept the requirements of such students well before him, and the elementary principles are explained in great detail and with numerous diagrams; in particular, the methods of measuring time, which are frequently so confusing to beginners, are clearly and carefully explained. The use of the Nautical Almanac and star catalogues are dealt with. The corrections to observed altitudes on account of the earth's figure, parallax, refraction, dip, etc., are explained with examples. Descriptions of the principal observing instruments—transit, sextant, chronograph—are given. A short chapter on the Constellations is included, and the remainder of the text contains a selection of the most suitable methods of observing latitudes, time, longitudes, azimuths, and positions at sea. Several useful tables are given at the end of the volume, including tables for computing the azimuth of Polaris at any hour angle (furnished by the U.S. Coast and Geodetic Survey). The volume contains throughout the text numerous examples worked out in detail to illustrate the principles, and other examples are given for the student to solve for himself. The volume is therefore eminently suitable for the student who is pursuing a course of study on his own. A short appendix on the tides has been added.

H. S. J.

PHYSICS

Electrical Phenomena in Parallel Conductors: Vol. I, Elements of Transmission. By F. E. PERNOT, Ph.D., Assoc. Member A.I.E.E. [Pp. xii + 332, with 83 diagrams.] (New York: John Wiley & Sons, Inc., 1918. Price 18s. 6d. net.)

SOME thirty years ago, when the problem of the transmission of electric disturbances along wires was first solved by Heaviside, the solution was found so difficult that the engineer had to neglect it. Later on, when our own Post Office and the late National Telephone Company were forced to consider the solutions for the case of long-distance telephony, simpler methods were devised of discussing the problem, and tables of hyperbolic functions were specially constructed to obviate the labour of computation. In this book the author discusses the problem mainly from the point of view of the power engineer. He points out that, now elaborate tables of the hyperbolic functions are available, it is unnecessary to use

the approximate solutions so frequently employed by the practical engineer. He proves that in many cases the exact solution can be obtained with less labour than that entailed in using the approximate solutions.

This book should be read as a sequel to the usual elementary mathematical treatises on the subject—for instance, the introductions to the theory by Fleming or Kennelly. It is not suitable for the student who approaches the problem for the first time.

In the first two chapters "direct current" lines having leakage are discussed, the solutions being given in terms of hyperbolic functions. They thus form an excellent introduction to the more difficult alternating current case. In Chapter III a most excellent discussion, although somewhat difficult to follow, is given of practical harmonic analysis. The author has a thorough grasp of the problem. He contemplates measuring 72 ordinates per wave length, and gives forms to facilitate the calculation. The calculations indicated, however, strike us as being laborious, and, noticing that in practice there are unavoidable errors made, both in drawing the curve and in measuring the lengths of the ordinates, we think that simpler formulæ would suffice. It has to be remembered also that an infinite number of curves can be drawn through the extremities of the 72 ordinates, and that the method makes the harmonics of low order have the same value for all these curves—a result which seems improbable. The author recognises this, and says his analysis will give the "simplest" curve drawn through the points. Opinions may differ as to what the simplest curve is. We think mathematicians would say that the curve whose equation was—

$$y = a_0 + a_1 x + a_2 x^2 + \dots + a_{71} x^{71}$$

was the simplest curve, and this certainly is not the author's curve. In our opinion the proper curve must be computed by the theory of probability, and no one has yet done this.

The concluding chapters give a very thorough discussion of the ordinary differential equations, and are excellently illustrated by diagrams. Many numerical examples are given which prove that when the problem is attacked in the proper way the numerical labour involved need not be excessive. The mathematical appendix given has been taken from the Smithsonian Mathematical Tables, and is distinctly helpful. Very complete tables of the logarithms of the hyperbolic functions are given, in computing which the author has the help of Mr. B. M. Woods.

Many novel methods are given which will be helpful in practical work—for example, the methods given of constructing voltage regulation curves. No attempt has been made to discuss how the "skin effect" will modify the solutions given, but perhaps this is reserved for a subsequent volume. We have not noticed any misprints, and the numerical accuracy of the examples is worthy of commendation. The want of an index takes away from the value of the book. The author uses "direct current" and "continuous current," and "arc tan x" and "$\tan^{-1}x$" indiscriminately. Engineers have now definitely adopted direct current, which is often contracted to d.c. They have long used a.c. for alternating current.

The book can be recommended to mathematicians interested in this important problem, and also to mathematical engineers who have to discuss the long-distance transmission of speech or electric power along wires.

A. Russell.

The Theory of the Relativity of Motion. By R. C. TOLMAN, Ph.D. [Pp. ix + 225.] (Berkeley: University of California Press, 1918.)

THERE are two well-known works in English on the Theory of Relativity (in its earlier or "restricted" form)—one by Dr. Cunningham and one by Dr. Silberstein. Both these works present the theory as it grew historically, and deal with the work of Larmor and Lorentz on the modification of Maxwell's equations of the electromagnetic field for moving axes of reference, before passing on to the interpretation of the Lorentz transformation first given by Einstein.

The volume before us abandons the historical method of presentation, except or a very brief outline of the historical development of ideas as to the nature of the space and time of science, which is given in the first chapter. In Chapter II the author considers the two main postulates of the Relativity Theory and the experimental evidence on which they rest. In the third chapter there is given an elementary deduction of a number of the most important consequences of these postulates. The mathematics employed in this chapter is of the simplest type; yet the reader is enabled thereby to grasp the new and important ideas as to measurements of time and space in the moving system, the non-additivity of velocities, the variation of mass with velocity, and the practical coalescence of mass and energy as different names for the same fundamental entity—in fact, all the startling and apparently paradoxical results of the new non-Newtonian mechanics.

Thereafter Dr. Tolman takes up a more analytical treatment of the subject, developing fully the dynamics of particles and rigid bodies, the dynamics of elastic bodies, of a thermodynamic system and of an electromagnetic system, making free use of the principle of Least Action, which is found not to conflict with the requirements of Relativity Theory. The book concludes with an exposition of four-dimensional analysis as applied in Relativity problems, making use, however, of a real time co-ordinate in place of the more usual imaginary co-ordinate.

In plunging straightway into problems of dynamics, Dr. Tolman has reversed the order of presentation adopted in the earlier treatises referred to above, and in the German work of Laue. In them electromagnetic theory receives first attention after the development of Einstein's kinematics, and the dynamics of a particle is expounded later on an electromagnetic basis. Also these authors make extensive use of quaternionic or four-dimensional vectorial analysis throughout—weapons which Dr. Tolman avoids introducing until his last chapter. As to the relative values of the two modes of presentation opinions will differ. Many teachers believe that it is of great advantage to a student to approach a new body of knowledge by the historical path, and learn how tentative and redundant hypotheses were raised only to be dropped and replaced by others of a more convenient character as increasing experimental knowledge or better analysis required. But it is at least arguable that such a procedure involves a certain initial confusion of thought on the part of the student, which is, of course, gradually cleared away, as he arrives at the finally-evolved body of consistent theory, and that a presentation in the first place of the theory in its final form (to be followed by a historical account of its growth) would prove to be a sounder method. On that ground, Dr. Tolman's book is heartily to be recommended. For most students of Mathematical Physics, Dynamics is a fundamental subject on whose principles the interpretation of all physical phenomena must rest, and the writer believes that Dr. Tolman's method is to be preferred—viz., a clear exposition of the manner in which Dynamical principles must be modified in order

to accord with the postulates of Relativity, followed by the treatment of electromagnetic phenomena.

The book only deals with the "restricted" Relativity of two systems of reference in uniform relative motion. No account is given of the more recent generalised form of it as applied by Einstein and others to the problems of gravitation.

J. R.

CHEMISTRY

Biochemical Catalysis in Life and Industry. Proteolytic Enzymes. By JEAN EFFRONT. [Pp. xii + 752.] (New York: John Wiley & Sons; London: Chapman & Hall, 1917. Price 23*s*. net.)

THIS book, which is translated from the French by Samuel C. Prescott, Professor of Industrial Microbiology at the Massachusetts Institute of Technology, forms in a sense a companion volume to Prof. Effront's "Enzymes and Their Applications," which was published several years ago. The present volume opens with an introductory chapter on the general properties of colloids and enzymes; the rest of the book is divided into six parts, each of which is subdivided into chapters or sections according to size. Part I is devoted to coagulating enzymes, and contains a comprehensive account of the various views concerning the origin and mechanism of the blood-clotting enzyme Thrombin, together with the experimental evidence on which these views are based; then follow a couple of pages on Myosinase, the name given to the enzyme of somewhat doubtful existence, which, according to a few authors, whose views are not universally accepted, is supposed to coagulate muscle plasma. Part I is brought to an end by a chapter on Rennet extending over some fifty pages, in which are described in some detail the preparation and properties of this enzyme, and the influence upon its action of the reaction of the medium. With regard to the question of the identity of different rennets, it is shown that the differences in properties of individual samples are much influenced by the presence of impurities connected with their source of origin; although it is generally held that the various rennets are not identical, it is difficult to speak with certainty on this subject while the methods of isolating enzymes are so imperfect; even the purest samples are contaminated with other substances which may influence their activity favourably or otherwise.

Part II is devoted to Pepsin; the subject is fully dealt with from the point of view both of the conditions under which the enzyme acts and the products which it forms. The question as to the possible identity of rennet with pepsin is complicated by the fact that the two are always associated with each other; the supporters of the view that the two enzymes are in reality one consider that the enzyme exerts either a coagulating or a peptonising action according to the condition of the medium; in the author's opinion the question will not be settled until one or other of these enzymes is found occurring alone in a plant, or until an antienzyme is prepared by immunisation, which is specific for either rennet or pepsin. The second chapter of Part II, entitled "Reversible Action of Enzymes," has a somewhat incomplete bibliography, with a number of important omissions.

Part III deals with pancreatic Trypsin and a number of similar enzymes from vegetable sources, and also with immunisation and antibodies.

Parts IV and V are devoted to Erepsin and Amidases respectively; while Part VI, occupying about one-quarter of the whole book, gives an account of the numerous applications of enzymes to medical science and industry; this part is,

of course, of more general interest. The author points out the probable improvement which would result to the baking industry from the use of artificial leavens containing, in addition to substances which generate carbon dioxide, some suitable proteolytic enzymes, since it would appear that most of the advantage observed in the use of compressed yeast over ordinary brewer's yeast is due to the more favourable action on the gluten contained in the flour of the proteolytic enzymes in the compressed yeast.

With regard to brewing, the author holds the view that a more economical and nutritive beer should be obtainable by the use of enzymes cultivated on cheaper materials than barley malt. As an illustration of the improvement resulting from the employment of selected enzymes, the cheese-making industry is cited, in which the efforts of scientific workers have ensured more uniform and certain results. Among other subjects dealt with are the rôle of proteolytic enzymes in tanning, in the formation of petroleum, in soil bacteriology, in the recovery of nitrogenous waste and in the production of artificial nitrogenous foods, all of which provide most interesting reading and much useful information.

The least satisfactory parts of the book are the bibliographies, which bear traces of having been only hastily revised, with the result that many names are misspelt and dates and titles of papers are frequently omitted. Apart from these minor defects, however, the book may be confidently recommended as a most valuable compendium of the subject it deals with.

P. H.

BOTANY

Botany of the Living Plant. By F. O. Bower, Sc.D., F.R.S., Regius Professor of Botany in the University of Glasgow. [Pp. x + 580, with 147 figures.] (London: Macmillan & Co., 1919. Price 25*s.* net.)

Several excellent introductory textbooks of botany have appeared in this country in recent years, and also a number of textbooks written for those preparing for special examinations. Prof. Bower's book differs from the former in being a full presentation of the fundamentals of botany, while it is free from the disadvantages of a book written for a special examination. It should be of much use to the University student beginning the study of botany, while it is written in such plain and simple language that the non-scientific reader who wishes to become acquainted with the problems of plant life should find here what he wants.

The five divisions of the book follow the five main divisions of the plant kingdom, commencing with the Angiosperms and concluding with the Thallophyta. As might be expected, the morphological aspects of the subject are always excellently treated. The treatment of physiological matters, on the other hand, often calls for some criticism. Examples may be cited from two chapters. In the chapter on the Water-Relation the staining of the walls of the tracheides and vessels of the wood by coloured liquids, and the stoppage of the transpiration-stream by plugging with gelatine the cavities of the tracheæ of a cut shoot, are advanced as conclusive evidence that the transpiration-stream takes place through the cavities of the vessels. It only shows, of course, that the ascent of water takes place through the vessels and tracheides; it does not eliminate the possibility of passage through the walls, as it is these latter which are, of course, stained by the dye, while the gelatine used to block the cell cavities may also affect the permeability of the walls. As Dixon has shown, further evidence than this is necessary to establish the location of the conducting channels.

In the chapter on Nutrition, it is a pity that the only method mentioned for the measurement of photosynthesis should be the "bubbling method," for, in spite of the obvious usefulness of this method as a demonstration, it is the most unreliable of a number of methods which it is possible to employ. The statement (p. 104) that the first step in photosynthesis is the breaking-up of the carbon dioxide molecule and the separation from it of free oxygen is not founded on any reliable experimental evidence; it is simply a view which forms the basis of one group of the hundred and more theories of carbon assimilation. The statement (p. 110) that glucose is probably the first product of photosynthesis is also only an expression of personal opinion; the experimental evidence on the point is not clear, and the conclusions drawn by the experimenters themselves are contradictory. And why does the author say that the process which he calls "Photo-Synthesis" (an unusual way of writing the word) was formerly called Assimilation? The two recent workers who between them have been responsible for the greater part of the work in this field are F. F. Blackman and R. Willstätter. The former uses the term "Vegetable Assimilation" in the general title of all his papers, and the latter invariably employs the term "Kohlensäureassimilation," and this as recently as last year! And why discard the modern word "protein" in favour of the obsolescent "proteid"?

The book contains no more phylogeny than is necessary for a proper appreciation of the problems of the plant, and a minimum amount of chemistry, although there is an appendix dealing with vegetable foodstuffs. The absence of chemistry is an advantage to the lay reader and to the elementary student who is also an elementary student in physics and chemistry, but it should be understood that no very deep appreciation of the life of the plant is possible without an adequate acquaintance with those fundamental sciences. W. S.

A Laboratory Guide for General Botany. By C. STUART GAGER, Director of the Brooklyn Botanic Garden. [Pp. x + 206.] (Philadelphia: P. Blakiston's Son & Co., second edition, 1919. Price $1 net.)

THIS book contains directions for students taking their first course of botany in a University. It is divided into two parts—one comprising Anatomy and Physiology, the other Morphology and Life History. The directions are rather full, and should train the student in powers of observation, and not merely in the acquisition of a number of botanical facts. As in the author's *Fundamentals of Botany*, the fact that the plant is a living organism solving the problems of existence in a certain environment is never lost sight of. The book is for this reason, and for the fullness of the directions, one of the best laboratory guides in general botany that has ever appeared, and it is satisfactory that appreciation of it has been sufficient to necessitate the appearance of a new edition.

In the second part of the book, where the morphology and life history of types rom different parts of the plant kingdom form the subject of study, more space might be given to a consideration of algæ. Thus, whereas one brown alga (*Fucus*) and three green algæ (*Vancheria*, *Spirogyra*, and *Pleurococcus*) are studied, no less than nine fungal types are dealt with. The reasons for the arrangement adopted in this part of the book are not obvious; for instance, why should Pteridophyta follow Fungi, when the first type studied is one of the former? This is, however, a very minor point, for the teacher can vary the order of study of the different types without any trouble.

W. S.

The Strawberry in North America: History, Origin, Botany, and Breeding. By S. W. FLETCHER, Professor of Horticulture at the Pennsylvania State College. [Pp. xiv + 234.] (New York: The Macmillan Company, 1917. Price $1.50.)

THIS is an interesting account of the history of the strawberry, especially as it concerns North America. The early cultivation of strawberries in gardens led, in the first quarter of the nineteenth century, to strawberry-growing on a commercial scale, until to-day the commercial strawberry crop of the United States is valued at £4,000,000 annually—more than half the strawberry crop of the world. The first two chapters give a very adequate account of the rapid development of this plant industry in America. The subjects of the next chapter are rather mixed—packages, training and pollination are considered mainly from a historical standpoint. The fourth chapter is entitled "Origin and Botany." It illustrates well the difficulty of tracing back to their origin the cultivated races of plants, even in the case of a plant whose cultivation is as recent as the strawberry's.

The remaining chapters are devoted to the desirable characters in a variety, and to the question of breeding. The book is well written, and contains a number of illustrations of different varieties of strawberries as well as of other subjects.

W. S.

Afforestation. By JOHN BOYD. [Pp. 39.] (London: W. R. Chambers, Ltd., 1918. Price 1*s*. net.)

IT is universally admitted that for many years Forestry has received but scant attention in the United Kingdom. It was not until the late war that most of us began to understand the great importance of timber for every kind of industry, and this little book supplies some practical ideas for the re-afforestation of our countrysides.

In his opening chapter Mr. Boyd points out the neglect into which forestry has fallen, and gives a short sketch of the attempts made to induce the Government to take an intelligent interest in schemes of re-afforestation. He goes on to give a description of the widespread destruction of woodlands which has been going on for several centuries, and he demonstrates the absolute necessity for timber in the majority of our industries.

The writer shows that the interests of Agriculture and Forestry are in no way antagonistic, and he points out that very little agricultural land would be required for timber, except for nursery purposes.

That all harmful ground game, such as hares, rabbits, and deer, should be rigorously dealt with he insists upon, and he also advocates the destruction of any harmful game-birds, or, at any rate, their restriction within reasonable limits.

There follow some practical suggestions as to where planting operations should commence, as regards altitude, shelter and aspect, and soil quality. The relations of the natural herbage to the area of timber which can be grown are also considered.

Finally, there is a list of trees which have come under the writer's special notice. We should have thought that the European larch would do well in Perthshire; this tree having been introduced there from the Tyrol in 1738 by Menzies of Culdares.

C. C. R.

ZOOLOGY

Animal Life and Human Progress. Edited by ARTHUR DENDY, F.R.S. [Pp. vii + 227, with one plate and several diagrams and illustrations.] (London: Constable & Co., Ltd., 1919. Price 10*s*. 6*d*. net.)

THIS book is a collection of nine public lectures delivered at King's College, London, in 1917–18, under the auspices of the Imperial Studies Committee of the University of London. Such contributors as Dendy, Bourne, Tate Regan, J. Arthur Thomson, Punnet, Herdman, Newstead, Leiper, and Wood-Jones, have written articles of first-rate value on a number of different topics, all, however, relating to Man and the Animal Kingdom. Dendy gives some account of his late work on grain and grain-destroying Coleoptera, and discusses in an attractive way the question of Man's Account with the Lower Animals. Prof. G. C. Bourne deals with "Some Educational and Moral Aspects of Zoology" in a characteristic and able manner; he points out how the Germans have misinterpreted the message of Darwin to mean the promiscuous introduction into ethics of brutal conceptions, and of a bellicose application of the doctrine of the Survival of the Fittest. He points out how this had become a cult among ultra-patriotic Germans, and with what result. The next essay is that of Tate Regan, and treats of Museums, Fish-glue, and Research. Prof. J. Arthur Thomson writes on "Man and the Web of Life," the "Web" being the "external systematisation of interrelations," social and otherwise, of man and his neighbour. Prof. Wood-Jones writes a deeply interesting article on the "Origin of Man." He vigorously attacks the old school of thought that directly associated man's uprising and origin with that of the apes. He considers Man arose not on a direct line or single common stem from the apes, but that "Man is no new-begot child of the ape, bred of a struggle for existence upon brutish lines, nor should the belief that such is his origin, oft dinned into his ears by scientists, influence his conduct. Were he to regard himself as an extremely ancient type, distinguished chiefly by the qualities of his mind, and to look upon the existing primates as the failures of his line, as his misguided and brutish collaterals, rather than as his ancestors, I think it would be something gained for the ethical outlook of Homo." In the following essays, Dr. Leiper, whose brilliant work on the Bilharzia worm in Egypt has left that country and mankind under a debt to him, Prof. Punnett, Prof. Newstead, and Prof. Herdman discuss matters in which they are specially interested. This book is an excellent collection of essays, and makes interesting reading.

J. BRONTÉ GATENBY.

Jungle Peace. By WILLIAM BEEBE, Curator of Birds, New York Zoological Park, and Director of Tropical Research Station. [Pp. 295, with 16 illustrations from photographs.] (London: Witherby, 1919. Price 8*s*. net.)

TO those who have never visited jungles, Mr. Beebe's title must sound strange, for in two words it shatters one's entire conceptions of these haunts of strange and dangerous animals. The mind naturally jumps to those time-honoured accounts which dwell with emphatic persistency on the tortures of *bête rouge*, tick, ant, and innumerable hordes of other deadly insects; or on the dangers of tarantula, vampire, and that most poisonous of snakes, the bushmaster; or, again, on the sudden death awaiting one in silence at the jaws of the jaguar or alligator; and when one recalls the suicide of Dr. Appun, but sixty years ago—for fear of a less pleasant death at the hands of Indians—in the very district with which

Mr. Beebe's book is concerned, one can be forgiven for expressing surprise at its title. And yet, long before one has read from cover to cover, one feels that the author is right. There is peace, and a very real peace, in the jungle.

The volume is an account of Mr. Beebe's rest-cure after a period of active service with the Flying Corps, and is well printed and illustrated. The opening chapters deal in an interesting way with the voyage out. It is no commonplace account, for it describes some unusual achievements in the matter of handling the captain of the ship and getting permission to do many curious things that no one but an ardent and daring naturalist would conceive.

But the charm of the book lies in the description of the jungle. The chapter on the Hoatzin has already and but recently been published in Mr. Beebe's more technical account of his work in British Guiana, and might well have been omitted, for it is not quite in keeping with the rest of the book. The bird is, however, so unique that its life-story will no doubt prove of interest to the general reader for whom the book is intended.

The harmless habits of the vampire; the encounter with army ants; the catching alive of the venomous bushmaster; the hunt by night with an Indian as sole companion—are some of the delightful stories contained in the volume, which bring home the truth of its title. After all these years of war, the book, well and vividly written, is a delight to read, and leaves one with the happy impression that somewhere, though it be in the wilds of British Guiana, there is Peace.

WM. ROWAN.

A Practical Handbook of British Birds. Editor: H. F. WITHERBY, F.Z.S., M.B.O.U. Authors of the various sections: ERNST HARTERT, ANNIE C. JACKSON, REV. F. C. R. JOURDAIN, C. OLDHAM, NORMAN F. TICEHURST, and the EDITOR. [Parts I and II. Pp. xvi + 128, with 5 plates and numerous text figures.] (London: Witherby, 1919. To be published in 18 parts at 4*s.* net per part.)

IT requires a bold and enterprising publisher to add a new book on British Birds to the very large number already written on that subject. The book itself requires not merely to differ from previous ones to arouse any interest, but to be an improvement on them. With Mr. Witherby himself as editor and part-author, he has seen to it that the book fulfils both these requirements. The book will be an invaluable addition to the library of every British ornithologist.

The book is based on *A Hand-List of British Birds* (Witherby), published in 1912—all the authors of that volume contributing to the present one. In addition, Miss Annie Jackson, whose research work on the plumages of ducks is well known, contributes. Also Mr. C. Oldham, than whom there is no man in England to-day more fitted to deal with the subject of the characters of birds in the field. To the lover of living birds his paragraphs will be very welcome, and in them alone the book has achieved something fresh.

Based on the *Hand-List* (some of the paragraphs are mere reprints), the same criticisms are applicable in regard to certain sub-species. The strict adherence to the International rules of nomenclature is here for once observed, so that we are worried with no *Nomina conservanda*, which involve a quite impossible attitude to the progress of science.

The "Key" to the orders, genera, etc., somewhat on the lines of some American books, is a useful innovation. There is also a good glossary of bird terms. Both of these will be much appreciated by beginners.

The main feature of the book, however, lies in the descriptions of the birds· Here for the first time we find complete accounts of British Birds in all their various plumages from nestling to adult—an enormous undertaking. There are, unfortunately, several gaps due to lack of material. The brief descriptions of closely-related sub-species not on the British list are provided for the benefit of travellers to the Continent.

The colour-plates are accurate and well printed and the text figures are good. It seems a pity that a plate such as Plate 3, representing five species of Redpoll, should be printed in black and white, in which form it is almost valueless; while Plate 4, depicting young and adult Crossbills—often before figured—should be honoured with colours. This plate is, moreover, done in the coloured-drawing style, reminiscent of old-fashioned German natural history illustrations, and in no way improves the present volume.

The book is written in short and pithy sentences for the economising of space. As it is, the two volumes into which it is proposed to compress the eighteen parts will be ponderous enough. Three volumes, proportionately thinner, would appear to us to be handier.

The editor claims, in the introduction, to be supplying some long-felt wants in British Bird literature. In conclusion we can only say that his claim is justified.

WM. ROWAN.

Class Book of Economic Entomology, with Special Reference to the Economic Insects of the Northern United States and Canada. By WILLIAM LOCKHEAD, B.A., M.S. [Pp. xiv + 436, with a frontispiece and 257 illustrations.] (Philadelphia: P. Blakiston's Son & Co., 1919. Price $2.50 net.)

ECONOMIC entomology is a phase of the study of Insects that has come into considerable prominence of late years. The more intense the exploitation of the plant resources of a country becomes, the more evident it is that in agriculture, horticulture, and forestry, more and more attention must be paid to the relations of Insects and plants. The word Insects here needs a note of explanation, for those included in the present volume, Wood Lice, Mites, Centipedes, Slugs, and Eelworms, are surely inclusive enough to satisfy the most rapacious entomologist. It would be pedantic to quibble over that, however, for nine-tenths of the pests are true Hexapods, and the relations of these other forms to plants, the methods of combating them, etc., are similar to those of the true Insects.

The book is divided into four sections, the first treating of the anatomy, morphology, life cycle, and economics of insects in general; the second takes the common crops and plants, describes the disease, and indicates the insect responsible; the third treats of the classification, description, and identification of common insects; and the fourth deals with the methods of controlling them.

As the author points out, the book is in the main a compilation from numerous sources, as such a book must of necessity be, but it is none the less a valuable and useful piece of work. The cross-references, the references to the sources of information, glossary, index, to say nothing of the illustrations, will make it invaluable in all courses on entomology in the area with which it deals.

The book will save teachers and scholars much time, and the author well merits their thanks.

C. H. O'D.

Field Book of Insects: with Special Reference to those of the North-Eastern United States, aiming to answer Common Questions. By FRANK E. LUTZ, Ph.D. [Pp. x + 509, with about 800 illustrations—many in colours.] (New York and London: G. P. Putnam's Sons, 1918. Price $2.50 net.)

WHEN one considers that it has been estimated that about 15,000 insects are to be found within a radius of about fifty miles of New York, one cannot avoid admiring the intrepidity of an author who endeavours to deal with the insects of the North-Eastern United States in a book of reasonable size. Yet here we have a volume of 500 pages, that can easily be slipped into the coat pocket, in which this vast ground has been covered in an amazingly successful manner. Not merely is a very well illustrated key to all the most important genera provided, but a most useful introduction on the general structure of insects, methods of collecting and preserving, and a comprehensive index also included.

The soundness of Dr. Lutz's work on the insects and his wide experience are matters of general knowledge. When to this is added a breezy, clear and concise way of handling the subject, the result is, of course, a book that is indispensable to anyone starting out to collect insects in North-Eastern America, and it is assured of a wide circulation.

While its appeal is particularly to collectors in that region, it will serve as a useful model of the way things should be done elsewhere. We only wish we had had the fortune to come across such a volume when starting off "bug-hunting."

The Monograph of the Land and Freshwater Mollusca of the British Isles. By JOHN W. TAYLOR, M.Sc., F.L.S. (Leeds: Taylor Bros., 1900. Vols. I, II, III, published, Vol. IV in course of publication. Price £2 2*s.* net per volume.)

THE title of this work is sufficiently indicative of its contents to need no further expansion. It is indeed worthy of the name of Monograph, for each species is treated in a very full manner from the anatomical, bionomic, distributional, palæontological and systemic points of view. All the species are dealt with from every possible point of view, and so fully that the work will undoubtedly be invaluable as a work of reference to all malacologists. If anything calls for special notice in this fine work, it is, perhaps, the illustrations, which merit special praise. The subjects illustrated are very accurately drawn and the plates well reproduced. Not only are there a large number of text figures, but the vignettes of people historically connected with various groups and sketches of characteristic localities add much to the charm of the book. Few works of this type are written nowadays, and it represents an enormous amount of careful and studious work. It is an excellent volume that will firmly establish the author's reputation for exactness and scholarship.

C. H. O'D.

Spencer Fullerton Baird, a Biography including Selections from his Correspondence with Audubon, Agassiz, Dana and Others. By W. H. DALL, A.M., D.Sc. [Pp. xvi + 462, with 19 illustrations.] (Philadelphia and London: J. B. Lippincott Co., 1915. Price $3.50 net.)

ALTHOUGH somewhat late, this volume has only just come to my hands for review, and I have read it with much pleasure. Baird was, of course, known

widely to the scientific world as the assistant secretary, and later secretary, of the Smithsonian Institute, and also for his connexion with the United States Commission of Fish and Fisheries, as well as for his numerous contributions to the Fauna of North America. What is perhaps not quite as well known or perhaps so fully recognised is the enormous amount of work he did and the great influence he had upon the development of biological sciences in North America. With the exception of Agassiz, no man had a wider influence or left a more enduring mark upon subsequent work.

With a thorough scientific training and an extensive zoological knowledge, he united a business capacity of quite an exceptional kind—a rare combination. He seemed equally well at home in the consideration of general principles and of small details. Through his organising ability and power of infecting others with his own enthusiasm he was able to inspire collectors, and was responsible for the sending out of a large number of expeditions.

He will be gratefully remembered by zoologists the world over for initiating at the Smithsonian Institute a policy of generosity in the distribution of literature that has characterised it, and the various other publishing organisations subsequently established in the United States, and one that might be adopted with considerable advantage by the public institutions in Great Britain. Not alone was he in the secretariat of the Smithsonian Institute, a task in itself sufficient for any man, but for years he was responsible for the Fish and Fisheries Commission which he was instrumental in founding, including the Biological Laboratory at Wood's Hole. This was done not merely at the expense of his time and energy, but it also was a very serious drain upon his none too ample income. Lastly, he was responsible for the formation of the National Museum, and the collections he had gathered together constituted the basis upon which it was built up.

When he took up biology in the United States, it was in a very poor way, yet he left his country about the best organised in the world for biological inquiry and research.

This volume is a readable account of the activities of a great man who was actuated by a love of science, and a desire to do his utmost for it and his country. It does this mainly by selections from his vast correspondence, and forms a welcome addition to any biological or natural history library.

C. H. O'D.

MISCELLANEOUS

The Principles of Electric Wave Telegraphy and Telephony. By J. A. FLEMING, M.A., D.Sc., F.R.S. Fourth Edition. [Pp. xv + 707, with 7 plates and 457 illustrations.] (London: Longmans, Green & Co., 1919. Price 42*s*. net.)

THIS new edition of Prof. Fleming's well-known book is an amplified reprint of the third edition published four years ago. The book retains the same character as the previous editions; it is a scientific survey of the whole field of wireless telegraphy and telephony rather than a handbook of apparatus; as the author says in the Preface, it deals with principles rather than descriptions. The most interesting additions to the new volume are those describing the improvements that have been made in the Thermionic Valve. This valve, which is based on Fleming's two electrode valve detectors, is proving, in radiotelegraphy, to be one of the greatest discoveries that has yet been made. By means of it, radiotelephony

has become comparatively easy; the generation of a continuous stream of high-frequency oscillations by a pliotron can be effected as easily as the switching on of an electric lamp. The chapter on Radiotelephony in this edition is greatly extended; but when all that has been done in Continuous Wave Telegraphy and Telephony during the last four years is published, it will be necessary for Prof. Fleming to amplify it still further. This book should be on the shelves of all who conduct scientific work in Wireless Telegraphy; it is an invaluable and reliable work of reference for the expert, as well as a sound and trustworthy text-book for the student. Wireless telegraphy and telephony have progressed in an altogether remarkable way during the last few years, and it is difficult to keep up with all the most recent developments. Prof. Fleming's book, however, enables this to be done—it contains nearly everything a wireless telegraphist wants to know.

The Adventure of Life. By ROBERT W. MACKENNA, M.A., M.D. [Pp. xiii + 306.] (London: John Murray, 1919. Price 6*s*. net.)

THIS is a work of popular philosophy, in which Dr. Mackenna reviews the problems of life and death in a simple manner that can be understood and appreciated by anyone. He clearly owes little to any previous writers, but has evolved his theories from his own head while engaged on active service in France. Herein lies both the strength and the weakness of his work. Its strength is in its freshness and honesty of style and presentation. Its weakness is in a lack of depth and of knowledge; for he often does not understand the theories which he attacks, and he produces other very ancient theories as though they were new—theories long ago discarded or revised by their protagonists. The main doctrine at the back of the book appears to be that all Nature is guided by intelligence, working towards a purpose of beneficent character. Thus, he attacks the mechanistic tendencies of physiology, without apparent knowledge of the arguments on either side, but because it is opposed to his innate way of thought. His theory requires him to show that pain is advantageous to humanity; and he does not shrink from tormenting his facts till they are forced into this Procrustean bed. The pains of childbirth, for instance, may be for the purpose of strengthening maternal love, which "receives a sacramental grace from suffering." In short, his theory involves him in the belief in a beneficent utility of all things; yet he offers no evidence for such an opinion. The expression of a popular theory with force and conviction doubtless carries with many readers greater weight than a reasoned argument. Whereas the general public will probably find in this book a suitable expression of their normal beliefs, it cannot honestly be said that the man of science will find in it anything that concerns him.

HUGH ELLIOT.

BOOKS RECEIVED

(Publishers are requested to notify prices)

History of the Theory of Numbers. Vol. I, Divisibility and Primality. By Leonard Eugène Dickson, Professor of Mathematics in the University of Chicago. Published by the Carnegie Institution of Washington, 1919. (Pp. xii + 486)

Descriptive Geometry. By W. Miller, M.E. Fourth Edition. New York: John Wiley & Sons; London: Chapman & Hall, 1918. (Pp. v + 176.) Price 7*s*. net.

Four-place Logarithmic and Trigonometric Tables. Together with Interest Tables. Edited by Louis C. Karpinski, Associate Professor of Mathematics, University of Michigan. Michigan, Ann Arbor: George Wahr, 1918. (Pp. 30.) Price 30 cents.

The Fundamental Equations of Dynamics and Its Main Co-ordinate Systems. Vectorially Treated and Illustrated from Rigid Dynamics. By Frederick Slate. Berkeley: University of California Press, 1918. (Pp. x + 233.)

Introductory Mathematical Analysis. By W. Paul Webber, Ph.D., Assistant Professor of Mathematics in the University of Pittsburgh, and Louis Clark Plant, M.Sc., Professor of Mathematics in Michigan Agricultural College. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. xiii + 304.) Price 9*s*. 6*d*. net.

Planetary Rotation Periods and Group Ratios. Two Essays on the Relations between the Planets in Diurnal Rotation and in Mass. By F. A. Black, F.R.S.E. Edinburgh and London: Gall & Inglis. (Pp. xii + 115.) Price 3*s*. 6*d*. net.

Fats and Fatty Degeneration. A Physico-Chemical Study of Emulsions and the Normal and Abnormal Distribution of Fat in Protoplasm. By Dr. Marian O. Hooker, Instructor in Physiology in the University of Cincinnati. New York: John Wiley & Sons; London: Chapman & Hall, 1917. (Pp. ix + 155.) Price 9*s*. 6*d*. net.

The Chemistry and Manufacture of Hydrogen. By P. Litherland Teed, A.R.S.M. (Mining and Metallurgy), A.I.M.M., Major, R.A.F. London: Edward Arnold, 1919. (Pp. vii + 152.) Price 10*s*. 6*d*. net.

The Metals of the Rare Earths. By James Frederick Spencer, B.Sc., D.Sc., Ph.D., F.I.C., Lecturer in Physical and Inorganic Chemistry at Bedford College (University of London), Reader in Physical Chemistry in the University of London. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1919. (Pp. x + 279.) With Diagrams. Price 12*s*. 6*d*. net.

The Analysis of Minerals and Ores of the Rarer Elements for Analytical Chemists, Metallurgists, and Advanced Students. By W. R. Schoeller, Ph.D., and A. R. Powell. London: Charles Griffin & Co., Exeter Street, Strand, W.C.2, 1919. (Pp. x + 239.) Price 16*s.* net.

Applied Mechanics. Vol. II, Strength of Materials. By Charles E. Fuller, S.B., and William A. Johnston, S.B., Professors of Theoretical and Applied Mechanics, Massachusetts Institute of Technology. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. xi + 556.) Price 17*s.* 6*d.* net.

Physical Laboratory Experiments for Engineering Students. Part I, Mechanics, Sound, Heat, and Light. By Samuel Sheldon, Ph.D., D.Sc., Professor of Physics and Electrical Engineering at the Polytechnic Institute of Brooklyn, and Erich Hausmann, E.E., Sc.D., Associate Professor of Physics and Electrical Engineering at the Polytechnic Institute of Brooklyn. London: Constable & Co., 1919. (Pp. v + 134.) With 40 Illustrations. Price 6*s.* net.

A Manual of Machine Design. By Frank Castle, M.I.Mech.E., late of the Mechanical Laboratory, Royal College of Sciences, South Kensington, Lecturer in Practical Mathematics, Machine Construction and Drawing, Building Construction and Engineering Science, at the Municipal Technical Institute, Eastbourne. London: Macmillan & Co., St. Martin's Street, 1919. (Pp. ix + 351.) Price 7*s.* 6*d.* net.

Scientific Signalling and Safety at Sea. By Prof. John Joly, M.A., D.Sc., F.R.S., F.G.S. London: Taylor & Francis, Red Lion Court, Fleet Street, 1919. (Pp. 36.) Price 1*s.* 6*d.* net.

Irrigation Engineering. By Arthur Powell Davis, D.Sc., Mem. Am. Soc. C.E., Director and Chief Engineer, U.S. Reclamation Service, and Herbert M. Wilson, C.E., Mem. Am. Soc. C.E., former Chief Engineer and Irrigation Engineer, U.S. Geological Survey., Seventh Edition, Revised and Enlarged. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. xxiii + 640.) With 250 Figures in the Text. Price 21*s.* net.

The Causes and Course of Organic Evolution. A Study in Bioenergics. By John Muirhead Macfarlane, D.S., Professor of Botany, Director of the Botanic Garden, University of Pennsylvania. New York: The Macmillan Company, 1918. (Pp. ix + 875.) Price 17*s.* net.

Lectures on Sex and Heredity. Delivered in Glasgow, 1917-18. By F. O. Bower, J. Graham Kerr, and W. E. Agar. London: Macmillan & Co., 1919. (Pp. vi + 119.) Price 5*s.* net.

A Dictionary of the Flowering Plants and Ferns. By J. C. Willis, M.A., Sc.D., F.R.S., European Correspondent, late Director, Botanic Gardens, Rio de Janeiro. Fourth Edition, Revised and Rewritten. Cambridge: at the University Press, 1919. (Pp. xii + 701.) Price 20*s.* net.

Botany for Agricultural Students. By John N. Martin, Professor of Botany at the Iowa State College of Agriculture and Mechanic Arts. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. x + 585.) Price 11*s.* 6*d.* net.

Strawberry-growing. By S. W. Fletcher, Professor of Horticulture at the Pennsylvania State College. New York: The Macmillan Company, 1917. (Pp. xxii + 325.) Price $1.75.

Vegetable Growing. By Jesse George Boyle, B.S., M.S. in Agriculture, Master of Horticulture, Associate Professor of Horticulture in the Purdue University, La Fayette, Indiana. Philadelphia and New York: Lea & Febiger, 1917. (Pp. ix + 350.) With 154 Engravings.

The Living Cycads. By Charles Joseph Chamberlain, Professor of Botany, The University of Chicago. Chicago: The University of Chicago Press. (Pp. xiv + 172.) Price $1.50 net.

The Preparation of Substances Important in Agriculture. A Laboratory Manual of Synthetic Agricultural Chemistry. Third Edition. By Charles A. Peters, Ph.D., Professor of Inorganic and Soil Chemistry, Department of General and Agricultural Chemistry, Massachusetts Agricultural College. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. vii + 81.) Price 4*s.* net.

Heredity. By J. Arthur Thomson, M.A., LL.D., Professor or Natural History in the University of Aberdeen. Third Edition. London: John Murray, Albemarle Street, W., 1919. (Pp. xvi + 627.) Price 15*s.* net.

Aquatic Microscopy for Beginners; or, Common Objects from the Ponds and Ditches. By Dr. Alfred C. Stokes. Fourth Edition, Revised and Enlarged. New York: John Wiley & Sons; London: Chapman & Hall, 1918. (Pp. ix + 324.) Price 10*s.* 6*d.* net.

The Carbohydrate Economy of Cacti. By Herman Augustus Spoehr. Published by the Carnegie Institution of Washington, 1919. (Pp. 79.)

The Sugar-Beet in America. By F. S. Harris, Ph.D., Director and Agronomist, Utah Agricultural Experimental Station, and Professor of Agronomy, Utah Agricultural College. New York: The Macmillan Company, 1919. (Pp. xviii + 342.) Price $2.25 net.

Fossil Plants. A Textbook for Students of Botany and Geology. By A. C. Seward, M.A., F.R.S., Hon. Sc.D., Dublin, Professor of Botany in the University, Master of Downing College, and Hon. Fellow of Emmanuel College, Cambridge. Vol. IV, Ginkgoales, Coniferales, Gnetales. Cambridge: at the University Press, 1919. (Pp. xvi + 542.) With 190 Illustrations.

A Textbook of Physiology. By Martin Flack, C.B.E., M.B., B.Ch. (Oxon), and Leonard Hill, M.B., F.R.S. London: Edward Arnold, 1919. (Pp. viii + 800.) With 485 Figures in the Text. Price 25*s.* net.

Problèmes Scientifiques d'Alimentation en France pendant la Guerre. Comptes Rendus des Séances de la Commission d'Alimentation de la Société de Biologie. Tenues sous la Présidence du p^er^ Charles Richet; et Bibliographie Analytique des Travaux Français publiés pendant la Guerre (1914-18), par R. Legendre, Secrétaire de la Commission. Paris: Masson et Cie. Libraires de l'Académie de Médecine, 120, Boulevard Saint-Germain, 1919. (Pp. 157.)

The Boy's Own Book of Great Inventions. By Floyd L. Darrow, Head of Science Department, Polytechnic Preparatory Country Day School. New York: The Macmillan Company, 1918. (Pp. ix + 385.) With 33 Illustrations. Price $2.50 net.

Annals of the Philosophical Club of the Royal Society. Written from its Minute Books. By T. G. Bonney, Sc.D., LL.D., F.R.S., Emeritus Professor of Geology, University College, London, Fellow of St. John's College, Cambridge, Hon. Canon of Manchester. London: Macmillan & Co., St. Martin's Street, 1919. (Pp. x + 286.) Price 15*s.* net.

Hindu Achievements in Exact Science. A Study in the History of Scientific Development. By Benoy Kumar Sarkar, Professor, National Council of Education, Bengal. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Calcutta, Bombay, and Madras, 1918. (Pp. xiii + 82.) Price 5*s.* net.

The Evolution of the Dragon. By G. Elliot Smith, M.A., M.D., F.R.S., Professor of Anatomy in the University of Manchester. Manchester: at the University Press. London: Longmans, Green & Co., 1919. (Pp. xx + 234.) With 26 Plates. Price 10*s.* 6*d.* net.

A Vision of the Possible. What the R.A.M.C. might become. An Account of some of the Medical Work in Egypt; together with a Constructive Criticism of the R.A.M.C. By James W. Barrett, K.B.E., C.B., C.M.G., M.D., M.S., F.R.C.S., Temporary Lieut-Col., R.A.M.C. London: H. K. Lewis & Co., 136, Gower Street, W.C.1, 1919. (Pp. xx + 182.) With 2 Maps and Plate. Price 9*s.* net.

Woman: The Inspirer. By Édouard Schuré. Authorised Translation by Fred Rothwell. London: The Power-Book Co., 52A, High Holborn, W.C., 1918. (Pp. vii + 166.) Price 4*s.* 6*d.* net.

Education in Ancient Israel from Earliest Times to 70 A.D. By Fletcher H. Swift, Professor of Education, College of Education, University of Minnesota. Chicago and London: The Open Court Publishing Co., 1919. (Pp. xii + 134.)

Anatole France. By Lewis Paget Shanks. Chicago and London: The Open Court Publishing Co., 1919. (Pp. xi + 241.) Price $1.50 net.

Six Weeks in Russia in 1919. By Arthur Ransome. London: George Allen & Unwin, 40, Museum Street, W.C.1. (Pp. viii + 151.) Price 2*s.* 6*d.* net.

ERRATUM

On p. 183 of the July number (No. 53), the price of "Catalogue of Lewis's Medical and Scientific Circulating Library" should read "12*s.* 6*d.* net, and 6*s.* net to subscribers," instead of "12*s.* 6*d.* net to subscribers."

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By Dorothy Wrinch, University College, London.

G. W. Smith, *American Journal of Math.*, xli. (1919), pp. 143–164, discusses nilpotent algebras generated by two units: the paper is based on the researches of Shaw, *Trans. American Math. Soc.*, iv (1903), pp. 251–287, 405–422.

H. Holden, *Messenger*, xlviii (1919), pp. 77–87, discusses a diophantine problem in three unknowns.

G. Humbert, *Comptes Rendus*, 168 (1919), pp. 917–923, 969–975, investigates the measure of classes of positive ternary quadratic forms with a given determinant. His results are extensions of the classical formulæ due to Eisenstein and H. J. S. Smith; the same author studies the measure of Hermitian forms, *ibid.*, pp. 1240–1246, and Hermitian forms in a quadratic imaginary field, *ibid.*, 169 (1919), pp. 360–365, 407–414.

J. E. McAtee, *American Journal of Math.*, xli (1919), pp. 225–272, discusses modular invariants of a quadratic form for a prime power modulus; the first part of the paper is an extension of L. E. Dickson's researches, *Trans. American Math. Soc.*, x (1909), p. 123, when the modulus is prime; the second part is occupied with the polynomial modular invariants of a binary form.

O. E. Glenn, *Trans. Amer. Math. Soc.*, xx (1919), pp. 154–168, investigates certain modular concomitant scales of the binary quadratic; the same author, *ibid.*, pp. 203–212, investigates a new treatment of theorems of finiteness of algebraic invariant systems.

A. Lodge, *Messenger*, xlviii (1918), pp. 101–107, discusses the connexion between the cross ratio of the roots of a quartic and the roots of its reducing cubic.

G. A. Miller, *Trans. Amer. Math. Soc.*, xx (1919), pp. 260–270, investigates the properties of groups which possess a small number of conjugate operators; the same author, *Bull. Amer. Math. Soc.*, xxv (1919), pp. 408–413, investigates groups containing operators of order two.

G. Giraud, *Comptes Rendus*, 169 (1919), pp. 131–134, in-

vestigates the classification of the substitutions of certain automorphic groups in invariables.

W. A. Manning, *Trans. Amer. Math. Soc.*, xx (1919), pp. 66–78, continues his researches on the order of primitive groups.

N. Kryloff, *Comptes Rendus*, 168 (1919), pp. 721–723, investigates approximation formulæ based on generalisations of the theory of mechanical quadratures.

R. L. Moore, *Trans. Amer. Math. Soc.*, xx (1919), pp. 169–178, discusses a set of postulates for plane analysis situs.

S. Lepschetz, *Comptes Rendus*, 168 (1919), pp. 672–674, discusses the analysis situs of algebraic varieties in n-dimensional space ; and in a subsequent note (pp. 758–761) he examines Abelian varieties.

E. Borel, *Comptes Rendus*, 168 (1919), pp. 1178–1150, discusses the use of decimals to illustrate theorems in the theory of sets, and, in particular, in the theory of sets of zero measure.

W. H. Young, *Proc. Royal Soc.*, xcvi (A), (1919), pp. 71–81, makes an important contribution to the theory of the area of surfaces ; after criticising the classical theory and the work of Lebesgue, Peano and Minkowski, the author formulates a theory which appears free from objection ; he gives a definition of the area of a surface which leads to a formula involving a double integral, and proves that the area, so defined, exists if the partial derivates of the co-ordinates with respect to the parametric variables are bounded functions of the parametric variables.

E. W. Chittenden, *Trans. Amer. Math. Soc.*, xx (1919), pp. 179–184, continues his researches on the convergence of sequences of functions, *ibid.*, xv (1914), by examining limit functions of sequences which converge 'relatively uniformly.' The same author with A. D. Pitcher, *ibid.*, 213–233, discusses the theory of developments of an abstract class in relation to the *Calcul fonctionnel* of Fréchet.

G. Fichtenholz, *Quarterly Journal*, xlviii (1918), pp. 142–147, discusses conditions under which an integral may be differentiated with respect to a parameter under the integral sign. The integrals considered are defined in accordance either with Riemann's or de La Vallée Poussin's or Lebesgue's definition, and conditions for the validity of Leibniz' theorem more general than those previously given by the author and by G. H. Hardy are obtained.

H. Blumberg, *Trans. Amer. Math. Soc.*, xx (1919), pp. 40–44, extends the concept of 'convex functions' of a real variable to that of convex functions of n real variables.

P. Lévy, *Comptes Rendus*, 169 (1919), pp. 375–377, studies

the concept, due to R. Gateaux, of mean value in a functional domain.

W. H. Young, *Proc. Royal Soc.*, xcvi (A), (1919), pp. 82–91, investigates formulæ for the change of independent variables in a multiple integral.

A. Buhl, *Comptes Rendus*, 168 (1919), pp. 504–506, discusses the interchange of parameter and argument of certain algebraic integrals.

P. Faton, *Comptes Rendus*, 168 (1919), pp. 501–502, criticises a recent note by A. Denjoy (*ibid.*, p. 387) on lines of singularities of analytic functions, pointing out that Denjoy's results are but slight generalisations of previously known theorems. Denjoy subsequently (*ibid.*, pp. 848–851) applies his results to the evaluation of definite integrals.

Carleman, *Comptes Rendus*, 168 (1919), pp. 843–845, gives an elementary proof of a theorem proved by Koëble in 1906, that ' Every function establishing a conformal representation between two domains, each bounded by a finite number of circles (one of which includes all the others which are mutually exclusive) is a linear function.' An extension is made to the case in which the boundaries are ' simple curves.'

A. Denjoy, *Comptes Rendus*, 169 (1919), pp. 219–221, examines the connexion between Riemann integration and Lebesgue integration.

G. H. Hardy and J. E. Littlewood, *Proc. London Math. Soc.* (2), xviii (1919), pp. 205–235, discuss the most far-reaching generalisation of Tauber's theorem, the converse of Abel's well-known theorem concerning the continuity of a power series along a radius up to its circle of convergence.

G. H. Hardy, *Messenger*, xlviii (1918), pp. 107–112, gives the first elementary proof of a theorem due to Hilbert that, when a_n is positive, the convergence of the series Σa_n^2 entails the convergence of the double series $\Sigma\Sigma a_m a_n\,(m + n)$.

J. W. Hopkins, *Trans. Amer. Math. Soc.*, xx (1919), pp. 245–259, investigates convergent developments associated with irregular boundary conditions ; the function satisfying the assigned boundary conditions is a solution of a certain linear differential equation of the third order, and the analysis is based on Birkhoff's discussion, *ibid.*, ix (1908).

G. Rémoundos, *Comptes Rendus*, 168 (1919), pp. 1265–1268, discusses the summability of asymptotic solutions of linear differential equations of the first order, and establishes results left unproved by Borel in his well-known *Leçons sur les séries divergentes*.

G. Julia, *Comptes Rendus*, 168 (1919), pp. 502–504, states a general property of integral functions which is associated with Picard's theorem, that the function assumes all finite values

(save one) in the neighbourhood of its singular point. Julia shews that the function assumes all finite values (save one) in a certain restricted neighbourhood of the singular point. Sequels to the paper appear on pp. 598–600, 718–720, 812–814, 882–884, 990–992, 1087–1089.

W. H. Young, *Proc. London Math. Soc.* (2), xviii (1919), pp. 163–200, makes important contributions to the theory of Bessel series, $\Sigma A_r J_m(k_r z)$, where the numbers k_r are the roots of the equation $kJ'_m(k) + HJ_m(k) = 0$. These series, which are of considerable importance in various branches of mathematical physics, have been investigated by Fourier, Hankel, Harnack, Dini and Hobson, the first reliable results being due to Dini. The analysis and results obtained by Prof. Young are on lines quite different from those of previous writers.

W. H. Young, *Proc. London Math. Soc.*, (2) xviii (1919), pp. 141–162, investigates the theory of Legendre series, $\Sigma a_n P_n(x)$; by a powerful method based on the introduction of "Restricted Fourier Series," the author makes the whole fabric of the theory of Legendre series repose on the theory of Fourier series.

J. R. Airey, *Proc. Royal Soc.*, xcvi (A), (1919), pp. 1–8, gives some approximate formulæ for Legendre functions of high order, which are extensions of approximations due to H. M. Macdonald and the late Lord Rayleigh. The paper contains a table from which $P_n(\cos\theta)$ may be calculated to six places of decimals, when n exceeds 20, for each degree of the quadrant. The familiar $P_n(\cos\theta)$ is partially replaced in this paper by the notation $P_n(\theta)$.

Defourneaux, *Comptes Rendus*, 168 (1919), pp. 880–882, 969–975, investigates properties of electro-spherical polynomials; an example of such a polynomial is $2\cos n\theta$ *qua* function of $2\cos\phi$.

E. Kogbetliantz, *Comptes Rendus*, 169 (1919), pp. 226–228, investigates an integral, considered by Angelesco in a Paris thesis 1916. The integral is a generalisation of the integral obtained by Poisson's method of discussing Fourier series.

E. Kogbetliantz, *Comptes Rendus*, 168 (1919), pp. 992–994, 169 (1919), pp. 54–57, 322–324, 423–426, examines Jacobi's hypergeometric polynomials, and discusses the summability of the series obtained by expanding an arbitrary function in a series of them. The results resemble those obtained by Chapman and others in connexion with the summability of series of Legendre polynomials. A paper on the general theory of summability by the methods of Riesz (by the same author) appears, *ibid.*, 168 (1919), pp. 1090–1092, and a note on the summability of Fourier series, *ibid.*, pp. 1193–1194.

E. A. Milne, *Messenger*, xlviii (1919), pp. 153–159, obtains a

number of results concerning expansions of polynomials, by an application of the calculus of Residues; though the results could be built up by elementary methods, they are suggested directly only by Cauchy's calculus.

N. E. Nörlund, *Comptes Rendus*, 169 (1919), pp. 166–68, 221–223, investigates generalisations of Euler's polynomials; the ordinary Eulerian polynomials satisfy a difference equation resembling that satisfied by the Bernoullian polynomials, and the extension effected by Nörland consists in the discussion of a polynomial in n variables, satisfying a system of such difference equations; in a subsequent paper (*ibid.*, pp. 372–375) he studies a single difference equation which is a generalisation of the Eulerian equation.

S. A. Joffe, *Quarterly Journal*, xlviii (1919), pp. 193–271, continues his researches (*Quarterly Journal*, xlvii) on Eulerian numbers; of these numbers he calculates eighteen more than those previously calculated, making fifty in all.

G. H. Hardy, *Messenger*, xlviii (1918), pp. 90–100, gives an account of some of the fundamental properties of 'Stieltje's integrals.'

S. Pollard, *Messenger*, xlviii (1918), pp. 87–89, gives a new and simplified proof of the fundamental exponential inequality required to prove the equivalence of the definitions of the Gamma function as an infinite integral and an infinite product.

L. Bairstow and Arthur Berry, *Proc. Royal Soc.*, xcv (A), (1919), pp. 457–475, investigate solutions of Poisson's and Laplace's equations, with a view to their applications to problems of Elasticity and Hydrodynamics; the functions discussed are of the nature of 'Green's functions.'

M. Kuniyeda, *Quarterly Journal*, xlviii (1918), pp. 113–153, investigates various special cases of oscillating Dirichlet's integrals which have been untouched by G. H. Hardy in his general investigations, *Quarterly Journal*, xliv (1918).

G. A. Larew, *Trans. Amer. Math. Soc.*, xx (1919), pp. 1–22, investigates Mayer's problems in the calculus of variations.

V. Brun, *Comptes Rendus*, 168 (1919), pp. 544–546, attacks the proof of Goldbach's theorem that every even number is the sum of two primes, with the aid of the sieve of Eratosthenes. He obtains some remarkable results, of which may be stated (1) For all sufficiently large numbers n, there exists a number between n and $n + \sqrt{n}$ with not more than 11 prime factors. (2) All sufficiently large even numbers are expressible as the sum of two numbers with not more than 9 prime factors.

H. Cramér, *Comptes Rendus*, 168 (1919), pp. 539–541, enun-

ciates an advance on the results already known concerning the situation of the zeros of the zeta-function of Riemann, and states that it can be proved without employing the celebrated "Riemann hypothesis." Subsequently (*ibid.*, pp. 1153–1154) he proves a theorem, concerning limits, which is of importance in connexion with the distribution of prime numbers.

G. H. Hardy, *Proc. London Math. Soc.* (2), xviii (1919), pp. 201–204, gives a short supplement to his paper on "The Average Order of the Arithmetical Functions $P(n)$ and $\Delta(n)$," *ibid.*, (2), xv (1916), pp. 192–213; these functions are two functions which make their appearance in connexion with the investigation of the number of representations of n as the sum of two squares and with the number of divisors of n.

G. H. Hardy and J. E. Littlewood, *Quarterly Journal*, xlviii (1919), pp. 272 *et seq.*, give a new solution of Waring's problem; the assertion made by Waring was that every positive integer is expressible as the sum of at most four squares, nine cubes, nineteen fourth powers, and so on. The authors give a proof, based on the use of Cauchy's theorem, which has no points of contact with the previously known solution, due to D. Hilbert *Math. Ann.*, lxvii (1909), and subsequently simplified by his students.

A. J. Pell, *Trans. Amer. Math. Soc.*, xx (1919), pp. 23–39, discusses linear equations in infinitely many unknowns with unsymmetric coefficients. Such equations include linear integral equations with unsymmetric nuclei as a special case; the present paper contains extensions of the theorems concerning integral equations which have previously been discussed by the author in 1910–11.

J. Drach, *Comptes Rendus*, 168 (1919), 497–501, discusses the integration by quadratures of a class of differential equations of the second order.

P. Boutroux, *Comptes Rendus*, 168 (1919), pp. 1150–1152, discusses definitions of multiform functions, with a view to obtaining representations of the totality of their branches throughout their domain of existence. Later (*ibid.*, pp. 1307–1310) he discusses a particular class of such functions which are integrals of a differential equation of the first order.

H. T. H. Piaggio, *Phil. Mag.* (6), xxxvii (1919), pp. 596–600, makes a contribution to the numerical integration of differential equations of the first order; his results are an extension of those due to Runge, *Math. Ann.*, xlvi (1895), and in certain cases are more accurate than approximations previously given.

R. Garnier, *Comptes Rendus*, 169 (1919), pp. 223–225, states

some theorems concerning linear differential equations with irregular singularities, preparatory to collecting his researches in a larger treatise.

Just as the notion of a derivate of a function suggests that of differential equation, so the notion of the saltus function (or oscillation function) leads to that of "saltus equation"; H. Blumberg, *Amer. Journ. of Math.*, xli (1919), pp. 183–190, investigates the complete solutions of a number of such saltus equations of various simple types.

A. L. Nelson, *Amer. Journ. of Math.*, xli (1919), pp. 123–132, computes a set of semi-invariants of systems of partial differential equations; the results have applications to projective differential geometry.

E. H. Neville, *Quarterly Journal*, xlviii (1918), pp. 136–141, gives proofs of formulæ connected with moving axes with variable angles; the formulæ have been announced by him without proof at the Fifth International Congress in 1912.

C. D. Rice, *Amer. Journ. of Math.*, xli (1919), pp. 165–182, examines the invariants of differential geometry by the use of vector forms. The analysis is much more simple and compact than the familiar analysis involving Cartesian co-ordinates, which is to be found in Forsyth's *Differential Geometry*.

A. Egnell has examined, in a thesis on Infinitesimal Vectorial Geometry, asymptotic directions in vectorial fields, and has proved that there are, in general, two asymptotic directions. He now investigates, *Comptes Rendus*, 168 (1919), pp. 1263–265, the exceptional case, pointed out to him by C. Guichand, in which the asymptotic directions are determinate. Another solution of Egnell's problem is given by R. Garnier, *Comptes Rendus*, 169 (1919), pp. 324–326.

L. Silberstein, *Phil. Mag.*, (6), xxxviii (1919), pp. 115–143, extends the ideas in his 'Projective Vector Algebra' (London, 1919) to the projective definition of the scalar product and the vector product of two vectors.

E. Bompiani, *Comptes Rendus*, 168 (1919), pp. 755–757, investigates quasi-asymptotic curves on surfaces in n-dimensional space. His results form a generalisation of Koenig's theorem that the projection on a plane of the asymptotic lines of a surface forms a conjugate net with equal invariants.

W. Sensenig, *Amer. Journ. of Math.*, xli (1919), pp. 111–122, contributes to the Invariant Theory of Involutions of Conics by deriving in terms of the system of two conics the complete simultaneous system of the involution of the pencil of conics through their four common points and their harmonic conic; the work follows on the researches of Gordan connected with invariants of two conics and the researches of Baker and Ciamberlini on three conics.

L. L. Dines, *Trans. Amer. Math. Soc.*, xx (1919), pp. 45–65, discusses projective transformations in function space; the transformations are extensions to infinitely many variables of the general projective transformation of n variables.

F. R. Sharpe and V. Snyder, *Trans. Amer. Math. Soc.*, xx (1919), pp. 185–202, discuss various types of involutorial space transformations; the only type which has been systematically investigated hitherto is the *monoidal* type, by Montesano, *Ist. Lombardi Rend.*, (2), xxi (1888).

A memoir by the late G. M. Green (*dec.* January 24, 1919, *æt.* 27) is published in the *Trans. Amer. Math. Soc.*, xx (1919), pp. 79–153, on the general theory of surfaces and rectilinear congruences; among the many advances made by the author in the theory of the differential geometry of surfaces from the projective aspect may be mentioned his discussion of the projective substitute for the normal to a surface.

L. E. J. Brouwer, *Comptes Rendus*, 168 (1919), pp. 677–678, enumerates regular Riemann surfaces of genus unity. In a subsequent communication (*ibid.*, pp. 845–848) he enumerates the finite groups of topological transformations of the anchor ring. A paper to be associated with the latter appears on pp. 1042–1044.

R. L. Hippisley, *Proc. London Math. Soc.*, (2), xviii (1919), pp. 136–140, gives a new method of describing a three-bar curve, based on the theorem that the pedal triangle of a point on a three-bar curve, with reference to the triangle of foci, has the property that its vertices are at fixed distances from a variable point.

T. C. Lewis, *Messenger*, xlviii (1918), pp. 113–128, continues his researches on pentaspherical co-ordinates.

C. H. Sisam, *Quarterly Journal*, xlviii (1918), pp. 104–112, discusses the locus formed by the point of concurrence of three tangents (with collinear points of contact) to a given algebraic curve; and also the envelope derived from the reciprocal construction.

C. H. Sisam, *Amer. Journ. of Math.*, xli (1919), pp. 212–224, investigates surfaces containing two pencils of cubic curves. The paper is a continuation of a former paper (*ibid.*, pp. 49–59) on surfaces generated by an algebraic system of cubic curves not forming a pencil.

B. Gambier, *Comptes Rendus*, 168 (1919), pp. 674–677, examines surfaces applicable to a paraboloid of revolution.

T. Cohen, *Amer. Journ. of Math.*, xli (1919), pp. 191–211, gives a number of properties of the plane quartic and various derived curves.

R. Goormaghtigh, *Nouv. Ann. de Math.*, (4), xix, pp. 93–111, investigates a family of plane curves, defined by a some-

what complicated intrinsic equation; the family includes numerous well-known curves, notably Ribancour's curve and the sinusoidal spiral.

A. R. Forsyth, *Messenger*, xlviii (1919), pp. 131–144, discusses the consequences of assuming various laws of facility of error.

A. Guldberg, *Comptes Rendus*, 168 (1919), pp. 815–817, examines Bravais' law of error; Bravais established the law for spaces of three and four dimensions, but was unable to extend it; but Guldberg shows that, by his methods, the law can be at once extended to n-dimensional space.

T. J. I'A. Bromwich, *Phil. Mag.*, (6), xxxviii (1919), pp. 231–235, gives some formulæ, derived from Stirling's formula, which have applications to Bernoulli's theorem in the theory of probability.

J. W. L. Glaisher, *Quarterly Journal*, xlviii. (1918), pp. 151–192, publishes the first of a series of papers on early tables of logarithms and the early history of logarithms. The main topics of this paper are Napier's logarithms and the transition from them to decimal logarithms. Dr. Glaisher emphasises in his introduction the inadequacy and incorrectness of existing accounts.

G. H. Bryan, *Math. Gazette*, ix (1919), pp. 333–352, gives very compact four-figure logarithm tables, including logarithm tables of the trigonometric functions to every two minutes. The tables are free from repetitions frequent in most tables, and are in that respect superior when space is a consideration; when space is not a consideration the repetitions possibly save mental effort when large masses of computations have to be performed.

W. W. Johnson, *Messenger*, xlviii (1919), pp. 145–153, gives an historical account of Napier's 'circular parts' of spherical triangles.

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., The Royal Observatory, Greenwich.

The Total Solar Eclipse of 1919, *May* 28–29.—During the past two years frequent reference has been made in Science Progress, in these notes and elsewhere, to Einstein's generalised theory of relativity. An essential feature of this theory is the so-called "principle of equivalence" according to which a gravitational field is indistinguishable from a spurious field of force produced by an acceleration of the axes of reference. Einstein showed that the theory was capable of accounting exactly for the so-called anomaly in the motion of the peri-

helion of Mercury; this was a distinct initial success which at once directed attention to the theory. He further pointed out two other results predicted by the theory, which should be capable of verification or disproof. The first of these was a displacement to the red of the Fraunhofer lines of the solar spectrum of about 0·008 angstroms. The careful measurements of Dr. St. John at Mount Wilson do not appear to confirm this prediction, although the smallness of the quantity in question and the possibility of its being concealed by other disturbing factors may cause doubt in some minds as to the validity of Dr. St. John's conclusion. The second prediction of the theory is that the path of a ray of light should be slightly deflected in passing through a gravitational field. The only means of testing this conclusion is to ascertain whether there is any displacement in the apparent position of a star seen near the edge of the sun. Naturally this can only be attempted during a solar eclipse. The displacement predicted should vary inversely as the distance from the sun's centre and amount to 1″·75 at the sun's edge. In this connection, it must be pointed out that if light is assumed to possess inertia —which is a reasonable supposition, since a beam of light contains energy and can exert a pressure—then it necessarily follows that it must be deflected in any field of force. On Maxwell's theory, therefore, we should expect a deflection which calculation shows would amount to 0″·87 at the edge of the sun, or to exactly half the amount predicted by Einstein. If, on the other hand, light does not possess inertia, there should be no deflection. There were thus three possible alternatives—no deflection, or deflections of 0″·87, or 1″·75 respectively, any one of which might not unreasonably be expected to be obtained when the test should be made. Of these the nil deflection would probably be most unexpected.

As already pointed out in these notes, the eclipse of May 28–29 last provided a very favourable opportunity for testing these alternatives, owing to the presence of several bright stars in the neighbourhood of the sun at the time of totality. As soon as it appeared probable, after the Armistice, that it would be possible to send expeditions to observe the eclipse, preparations were commenced for two expeditions to set out under the auspices of the Joint Permanent Eclipse Committee of the Royal and Royal Astronomical Societies. One expedition went to Sobral, in North Brazil, the observers being Dr. A. C. D. Crommelin and Mr. C. Davidson; the other went to the Island of Principe, off the West Coast of Africa, the observers being Prof. A. S. Eddington and Mr. Cottingham. Both expeditions were fortunate in securing observations during totality, although there was much cloud in the sky at

the time: the region near the sun was partially obscured by thin cloud during the whole of totality at Principe and during a part at Sobral. Consequently, the plates obtained at Sobral show more stars than those obtained at Principe and give a better determination of the deflection at the limb.

The astrographic object glasses of the Greenwich and Oxford Observatories were used respectively at Sobral and Principe in conjunction with 16-inch cœlostats: in addition, a 4-inch lens of 19 feet focal length was used at Sobral with an 8 inch cœlostat.

The results obtained are given in a paper by the Astronomer Royal and the observers, read at a joint meeting of the Royal and Royal Astronomical Societies on November 6, 1919. They may be summarised as follows:

(1) The best results were obtained from the long-focus 4-inch lens at Sobral, which gave images of good definition. The photographs were measured against other photographs of the same region taken at the same altitude from the same station a few months subsequent to the eclipse, when the sun had moved away. A reliable determination of scale was in this way obtained, the scale-value naturally being of fundamental importance. The mean displacements of the seven brightest stars actually obtained, and those to be expected on Einstein's theory, are given in the following table:

	Displacement in R.A.		Displacement in Dec.	
Star No.	Observed.	Calculated.	Observed.	Calculated.
11	−″·19	−″·32	+″·16	+″·02
5	−·29	−·31	−·46	−·43
4	−·11	−·10	+·83	+·74
3	−·20	−·12	+1·00	+·87
6	−·10	+·04	+·57	+·40
10	−·08	+·09	+·35	+·32
2	+·95	+·85	−·27	−·09

A very satisfactory accordance in both co-ordinates is thus obtained.

(2) The images on the astrographic plates taken at Sobral were diffuse, this being attributed to a change in figure of the cœlostat minor; there was a rather large discordance between the mean results from the individual plates, but from the whole series an outward deflection at the limb of 0″·93 was obtained.

(3) The plates taken at Principe showed only a few stars and the scale could not be directly determined. Plates of another region of the sky taken at the same altitude were used, and compared with plates of the same region and of the eclipse field obtained previously at Oxford. The determination of scale was therefore somewhat weak, though the uniformity of

temperature at Principe was in its favour. The final result of the discussion of these plates was an outward deflection of $1''\cdot61$, with a probable error of $\pm 0''\cdot30$.

Summarising, the 4-inch lens at Sobral gave a value of the deflection of $1''\cdot94$ from declinations and of $2''\cdot06$ from right ascensions, these values being in excellent agreement and giving a weighted mean value of $1''\cdot98 \pm 0''\cdot06$. The Principe observations have a much larger probable error ($\pm 0''\cdot30$) but tend to confirm the result, and both these series point to the full deflection $1''\cdot75$ of Einstein's theory and to the predicted rate of falling off with increase in distance from the sun's centre. There remain the Sobral astrographic plates which gave a value discordant by an amount far exceeding the limits of its accidental error, which discrepancy may possibly be due to change of figure of the mirror. On the whole, therefore, Einstein's predicted displacement is confirmed, but there is sufficient uncertainty in the result to render it desirable that further tests should be made at future eclipses.

Finally it should be pointed out that it seems clear that the deflection must be attributed to the sun's gravitational field. Refraction by coronal matter naturally suggests itself as a possible explanation, but a simple investigation at once shows that an impossibly high value of the refractive index would be necessary to account for the deflection.

The announcement of the result attracted widespread attention, and so many distorted accounts and unjustified statements have appeared in the daily press that it is well to consider briefly what interpretation is to be put upon it.

First, then, the result may be considered independently of any theory, as a purely observational fact. It cannot be denied that it has been definitely established that the light from a star is deflected in passing near the sun's edge, though there may still remain some doubt as to the exact amount of the deflection. Since it seems very improbable that the phenomenon is due to refraction, it must be concluded that it is due to the direct effect of gravitation on the field of light. The establishment of this fact is a scientific achievement of the greatest importance. Many attempts have been made to gain some clue as to the nature of gravitation, but always with a negative result. Gravitation has remained the most elusive of the forces of nature. Theory after theory has been proposed to account for it, but none could be accepted because none could be tested. Now we know the nature of light and its velocity can be measured; the electro-magnetic theory of light is to-day universally accepted. The present result therefore shows a direct inter-relationship between gravitational and electro-magnetic forces.

How, then, is the result to be interpreted? There seems little doubt but that the deflection which has been obtained is very closely double that which would have been anticipated on Newton's law of gravitation, supposing light to possess inertia. This suggests that Newton's law of gravitation is merely a first approximation. There is nothing very surprising in this suggestion, although the popular press has made much of it; we have already become accustomed to the idea that Newtonian mechanics is but a first approximation, requiring modification when velocities comparable with the velocity of light are concerned. It is not, then, altogether surprising that the law of gravitation may also be modified for very high velocities, though it would not be possible *a priori* to say what the modification would be.

The theory of Einstein has naturally commanded attention as it *predicted* the result which has apparently been obtained. The difficulty of understanding what is involved in the theory, owing to the apparent impossibility of separating the basic ideas involved in it from the mathematics with which they are bound up, has perhaps not unnaturally caused some scepticism as to whether the theory provides the true explanation of the result. Our conservative ideas rebel against the belief that a fundamental law of nature can only be stated by invoking the aid of the calculus of variations. May it not be, however, that, if only other axes of reference were chosen, a simpler law would result? As a very elementary illustration, the expression of phenomena occurring on a rotating body assumes a simpler form if axes rotating with the body are chosen. Einstein's theory is a theory of space and time and *not* a theory of gravitation, and the distinction is an important one. It is an extension of the older relativity theory which developed out of the attempts to explain the negative results of all experiments seeking to obtain evidence of a motion of the earth relative to the æther. It has been stated that the result now obtained, justifying Einstein's theory, has disproved the existence of the æther. This is incorrect. If the æther is not necessary on the generalised theory of relativity, then it was not necessary on the older theory. In fact, many scientists, including the present writer, considered that the older relativity theory had made the conception of the æther unnecessary. The theory of Einstein has extended the conceptions of the older theory and, in addition to the relativity of velocities, has, as it were, through the introduction of the equivalence hypothesis, introduced the conception of the relativity of accelerations. It is this added feature which leads to the results affecting gravitation, but an explanation as to the nature of gravitation is *not* given.

The negative results indicated so far by the spectroscopic evidence form at present a serious argument against the complete acceptance of Einstein's theory. It may also be mentioned that mathematically there is a difficulty involved in the boundary conditions at infinity not being satisfied. It is possible that the expression which Einstein obtains for the law of gravitation may be the correct one, but that the method of deriving it may require modification. A careful critical examination will be required to ascertain how much of the theory would be affected if the negative spectroscopic evidence were substantiated: diverse opinions have been expressed on this point, and the writer is not in a position at present to give a considered judgment.

Meanwhile, we must suspend final judgment and remain satisfied that a definite step forward has been made. The field is now open for the solar spectroscopists on the observational side and for the mathematicians on the theoretical side to provide the further material and analysis which will indicate whether, and if so in what directions, the theory requires modification.

Variability of the Sun's Radiation.—For some fourteen years regular observations of the solar constant of radiation have been made at the Smithsonian Astrophysical Observatory on Mount Wilson, California. Before the observations had been continued for many years, it was noticed that the values obtained showed a variation greater than could be attributed to errors of observation, and this suggested that the sun was a variable star. A two-fold variation was indicated: one was of long period, the sun-spot period, with a range of from 3 to 5 per cent.; superposed on this was a short-period irregular variation, running its course in intervals of a few days, weeks, or months, and having a range up to 7 per cent. To test the reality of this supposed irregular variation, an expedition was sent to Bassour, in Algeria, and during 1911 and 1912 simultaneous observations were carried on at Mount Wilson and Bassour. Whilst these observations did not prove conclusively that the variations found were true variations of the sun's radiation, they supported one another on the whole and indicated that the variations were due to causes outside the earth's atmosphere.

To test the matter further, in 1918 another expedition was sent to a station at Calama, Chile, at an altitude of 7,500 feet, which was selected on account of being one of the most cloudless regions of the world and practically devoid of rain. Simultaneous observations have since been carried out at this station and at Mount Wilson, and the preliminary results of the computations are given by C. G. Abbot in *Proc. Nat.*

Acad. Sci., Washington, 1919, **5**, 383. In order to exhibit as clearly as possible any relationship, points were plotted whose co-ordinates were the values of the solar constant obtained on any given day at the two stations. Obviously, if there was no variation in the sun's radiations, the plotted points should form a normal error distribution about the point whose ordinate and abscissa were both equal to the solar constant. If, however, the solar constant is variable, the plotted points will be stretched out along a line passing through this point and making an angle of 45° with the two axes. This was found to be actually the case: in fact, Abbot states that with but very few exceptions all of the plotted points could be brought on to this line with a change of less than 1 per cent. The two stations being more than 5,000 miles apart and in different hemispheres, this result seems difficult of explanation on any other supposition than that there is a true short-period variation in the sun's radiation, and this result may be regarded as being now definitely established.

Abbot states that investigations by Clayton show that a correlation exists between terrestrial temperatures and pressures and these changes in the sun, and that daily solar radiation reports are telegraphed from the station at Calama and used in forecasting temperature in Argentina. The correspondence is stated to have been very good so far. Whether further investigations will confirm this statement remains to be proved, but should this be the case, a great impetus will be given to the study of the sun's variations, and the establishment of additional solar radiation stations will be necessary in order to eliminate as completely as possible the effects of local disturbing causes.

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LOUS, K., A Certain Class of Periodic Orbits around the Two Finite Masses in the Restricted Problem of Three Bodies, *ibid.* 1919, **209**, No. 5008.

BELL, L., The Physical Interpretation of Albedo. II. Saturn's Rings, *Astroph. Journ.* 1919, **50**, 1.

BRUNN, A. v., Zur Berucksichtigung des Dampfdruckes bei der Berechnung der Refraction, *Ast. Nach*, 1919, **209**, No. 5009.

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JONES, H. S., Results Obtained from Seven Years' Observations made with the Cookson Floating Zenith Telescope at the Royal Observatory, Greenwich, *M.N., R.A.S.*, 1919, **79**, 542.

CONRADY, A. E., Star-Discs, *ibid.* 1919, **79**, 575.

BECKER, L., On a New Artificial Horizon for Sextants, *ibid.* 1919, **79**, 593.

LINDBLAD, B., On the Use of the Grating for Determining Spectral Type and Absolute Magnitude of the Stars, *Astroph. Journ.* 1919, **49**, 289.

LUNDAHL, C. F., On Some Properties of the Stars of Spectral Type F., *Lund. Medd., Ser. II.*, No. 21, 1919.

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HARVARD OBSERVATORY, Proper Motions of Stars in the Zone $-9°50'$ to $-14°10'$, *H.A.*, 1919, **81**, No. 1.

BAILEY, S. I., Variable Stars in the Cluster Messier 15, *ibid.* 1919, **78**, No. 3.

REYNOLDS, J. H., The Distribution of Hydrogen and Nebulium in the Orion Nebula, *M.N., R.A.S.*, 1919, **79**, 561.

CANNON, J. B., Orbit of Spectroscopic Binary 32 θ_2 Cygni, *Pub. Dom. Obs. Canada*, 1919, **4**, No. 10; Orbit of Boss 1082, *ibid.* No. 11; The Spectroscopic Binary Boss 1275, *ibid.* No. 12.

HARPER, W. E., Orbits of the Spectroscopic Components of Boss 5173, *ibid.* 1919, **4**, No. 13.

SHAPLEY, H. and M. B., Studies based on the Colours and Magnitudes in Stellar Cluster: (12) Remarks on the Arrangement of the Sidereal Universe, *Astroph. Journ.* 1919, **49**, 289; (13) The Galactic Planes in 41 Globular Clusters; (14) Further Remarks on the Structure of the Galactic System, *Mt. Wilson Conts.*, 1919, Nos. 161–2.

KESTER, F. E. and ALTER, D., Spectral Series and the Grouping of the Asteroids, *Astroph. Journ.*, 1919, **50**, 50.

PHYSICAL CHEMISTRY. By PROF. W. C. McC. LEWIS, M.A., D.Sc., University, Liverpool.

The Source of Radio-Active Change.—As a result of the work of several investigators during the last few years, it is now coming to be generally recognised that *all* chemical reactions, whether thermal or photo-chemical, are due to radiation. In photo-chemical changes, as ordinarily examined, the temperature of the external source of light and therefore of the radiation itself is considerably higher than that of the material upon which the radiation acts. In ordinary or thermal actions the origin and source of the chemical change is the radiation present in the matter, and in equilibrium with it, in virtue of the temperature of the matter. In ordinary chemical changes we include such physical processes as change of state.

This wide significance of radiation for chemical and physical changes has been extended still further by Perrin in a recent paper (*Annales de Physique* [ix], **11**, 1919), who attempts to find the origin of radio-active change in the existence of an extremely short-wave type of radiation—"ultra-X" rays—emanating from the earth and capable of penetrating matter to a much greater extent than X-rays themselves. At first

sight this might appear to be purely fanciful. Perrin's treatment of it is such, however, as to bring it within the range of serious scientific thought.

The fundamental change introduced by this idea is that radio-active processes have to be regarded no longer as typically exothermic, but, on the contrary, as strongly endothermic, in the sense that a much greater amount of energy is first of all absorbed by the radio-active parent substance than is emitted in the observed radio-active change. Perrin has even succeeded in calculating roughly the wave-length of the "ultra-X" rays, which would account for the radio-active change, by taking into account the kinetic energy of an expelled a particle. The frequency of the "ultra-X" radiation is of the order 10^{21} vibrations per second (whilst that of visible light is of order 5×10^{14}).

The attempt is made frankly by Perrin to bring radio-active changes into line with all other atomic and molecular processes, thermal reactions, photo-chemical reactions, fluorescence, phosphorescence, and ionisation, all of which involve radiation of certain wave-lengths. Ionisation of a gas by X-rays represents, in fact, according to Perrin a group of reactions intermediate between ordinary chemical reactions and radio-active transformations. Following out his idea Perrin proceeds to consider several astrophysical or rather astro-chemical problems, the existence of proto-atoms, and the stability of atoms of various types as a function of their environment.

The most striking characteristic of radio-active change, viewed in relation to other types of chemical change, is the apparent absence of any influence exerted by altering the temperature of the radio-active material. In the case of ordinary thermal reactions temperature is a most important factor, the rate of change increasing threefold for a rise in temperature of ten degrees in the neighbourhood of room temperature.

As Perrin points out, however, the presence or absence of a temperature effect upon a process depends upon the frequency or wave-length of the radiation producing the change. If the effective radiation frequency is small, say of the order 10^{14} vibrations per second, there is known to be a sensible amount of this type of radiation present per unit volume of any material at ordinary temperatures, and this so-called density of radiation increases rapidly as the temperature of the material rises. The result is that the rate of the chemical process (depending upon this low-frequency radiation) increases rapidly with the temperature. If, on the other hand, the process requires radiation of frequency 10^{15} vibrations per second, such a

process will not proceed with measurable speed at ordinary temperatures, because there is almost none of this type of radiation present in a material system at ordinary temperature. To render the process observable it is necessary to expose the material to an external source emitting this high-frequency radiation in sufficient quantity. The conditions are in fact those met with in photo-chemical reactions, so-called. On raising the temperature of the material by ten degrees under these conditions there is still no sensible increase in the density of the radiation of the effective type, and the process is, in fact, independent of temperature. This is already known to be approximately the case with photo-chemical changes. What is practically true of processes requiring radiation of the frequency 10^{15} is even more rigidly true of processes which require a frequency of the order 10^{21}. Perrin's calculation that the frequency of the external radiation causing radio-active change is of this order is therefore in agreement with the fact that the temperature alterations which can be brought about under ordinary experimental conditions are absolutely without influence upon the rate of radio-active transformations.

At the present time the hypothesis lacks a *direct* experimental basis. If its originator is able to make progress in this direction it will constitute one of the most striking advances of physical chemistry.

ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., University College, London.

DURING the last quarter a number of communications dealing with vitamines have been published, showing the ever-increasing attention which is being paid to this interesting class of compounds. The report of the Lister Institute for Preventive Medicine for 1919 contains an account of the work done in connection with the anti-scorbutic accessory factor in common food-stuffs. From this it would appear that the juice of the lemon is far superior in antiscorbutic activity to that of the lime either fresh or preserved; it has, moreover, been found that the "lime juice" originally employed in combating scurvy in the British Navy was in those days actually prepared from lemons. Swede juice is found to be the most efficient substitute for the juice of the lemon or the orange. Vegetables lose practically all their antiscorbutic properties in tinning, and, as their food value is not high, they are not economical. Dried vegetables likewise are deficient in antiscorbutic properties. Dried milk has considerably less antiscorbutic action than fresh or scalded milk, but there is not much difference in the growth-promoting properties of the two. By making

determinations of the minimum daily ration of any substance which will just protect an experimental animal from scurvy, under the conditions of the experiment, and by comparing the values so obtained for the unpreserved and the preserved material, it has been found possible to determine whether there is any loss in antiscorbutic power during the preserving process.

According to Drummond (*Biochem. J.*, 1919, **13**, 77) an adequate supply of all three accessory food factors (i) Fat soluble A, (ii) Water soluble B, and (iii) water soluble C (the antiscorbutic factor) is essential for the well-being of higher animals. The striking difference in properties between these various vitamines is illustrated by an investigation by Zilva (*Biochem. J.*, 1919, **13**, 164) in which it is shown that the Fat soluble factor A in butter is inactivated by exposure to ultraviolet light for eight hours, while the other two are not destroyed, although the butter itself is so profoundly affected as to be bleached and unfit for consumption.

Drummond (*loc. cit.* p. 81) also has shown that the growth-promoting factor A in animal fats is not so stable to high temperatures as has been assumed, that contained in butter or whale-oil being destroyed by exposure to a temperature of 100° for one hour: the substance could not be identified with any known components of fats, such as cholesterol, phosphatides or pigments, and it is suggested that it may be of an enzymic nature.

Although it is commonly stated that stinging-nettles contain formic acid, the statement rests on somewhat slender evidence furnished by Gorup Besanez, who in 1849 obtained a slightly acid solution by distilling an aqueous extract of stinging-nettles with phosphoric acid. Since then a number of observers have obtained similar distillates by boiling various plant parts with water or by steam distilling them, and it cannot be accepted as proved that the formic acid obtained by Gorup Besanez really came from the stinging hairs of the nettles. To establish the presence of this acid in the hairs, Dobbin (*Proc. Roy. Soc. Edin.*, 1919, **39**, 137) has recently employed a somewhat ingenious method: he impregnated filter-paper with barium carbonate by soaking it in 2 per cent. baryta water and exposing it to the air; taking the paper in gloved hands, he pressed it on the upper and lower surfaces of a large number of leaves of growing nettles. Dealing in this way with several hundred leaves, the liquid expelled from a very large number of stinging hairs was collected. The paper was then extracted with water and the filtered extract distilled with phosphoric acid; the distillate was treated with lead hydroxide to convert the acid into the lead salt, and

crystals of lead formate were subsequently identified under the microscope. It is of course possible, even now, that the formic acid may have been produced during the distillation of the original extract with phosphoric acid. In any case, the author points out that the establishment of formic acid in no way affects the question as to whether or not this acid is the main cause of the intense irritation produced by stinging-nettles. According to Haberlandt (1886) the active poison is most probably an enzyme.

The mode of action of colloidal silver sols has recently been studied by Professor Marshall (*Proc. Roy. Soc. Edin.*, 1919, **39** (2), 143). He finds that the bactericidal action of silver sols is relatively slight as compared with that of the free silver ions in a silver nitrate solution. Such bactericidal action as they have is, however, not due to bombardment by particles of silver in Brownian movement, since bacteria were seen to move freely for long periods in a moderately concentrated colloidal metal sol, in spite of their being frequently bombarded. The chief antiseptic action of silver sols appears to be due to ultramicroscopic particles between 5 $\mu\mu$ and 15 $\mu\mu$ diameter, and it is suggested that these particles may be taken up by the bacteria and converted into soluble products within the organism. This would explain why colloidal silver sols act more slowly than ionised silver.

Bancroft (*J. Phys. Chem.*, 1919, **23**, 356 and 365), in the course of his studies on the colours of colloids, discusses the blue colour of eyes and feathers. With regard to the former it is pointed out that there is no pigment on the front of the iris in blue eyes; the colour is due to turbid media, and is deeper the finer the suspended particles. Other colours in eyes are due to pigments in the front of the eye which thus mask the blue colour produced by the turbid media. In the case of feathers the colours fall into three categories: *firstly*, those which are produced by a pigment, and include black, brown, orange and yellow; *secondly*, blue and violet, which are structural colours produced by finely divided air-bubbles contained in a transparent layer below the epidermal cells; *thirdly*, there are colours depending on the position of the light and the eye produced by a sheath acting like a prism. By displacing the air-bubbles in the case of the blue feathers by a liquid such as Canada balsam or benzene, which has the same refractive index as the cell-wall, the blue colour disappears.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., F.G.S., University, Glasgow.

Economic Geology.—F. L. Stillwell has continued his researches on the origin of the gold-quartz veins of Bendigo in "Factors

Influencing Gold Deposition in the Bendigo Gold-field," Part II (*Commonwealth of Australia, Advisory Council of Science and Industry, Bull.* No. **8**, 1918, pp. 45). The chief new point is that the force of crystallisation of the quartz is regarded as instrumental in opening up the fissures (see Taber's views below), although some reefs have been formed by the infilling of already open fissures by quartz, and others are due to the replacement of slate and sandstone.

In a discussion of Stillwell's earlier paper (see SCIENCE PROGRESS, 1918, **13**, 215) S. Taber advances the view that the veins have largely made room for themselves by mechanically displacing the country rock. He does not think it possible that large cavities could remain open at the crests of the folds during a period of intense folding under conditions of deep burial and great pressure. Possibly small cavities were filled by the silica-bearing solutions, and some replacement of country rock effected; but both these processes are believed to have been of minor importance (*Economic Geology*, 1918, **13**, 538–46).

Stillwell replies to this criticism (*Ibid.*, 1919, **14**, 430–4) by pointing out that he has partially adopted Taber's views in his second paper.

The first full description of the famous Charters Towers gold-field of Queensland is given by J. H. Reid (*Geol. Surv. Queensland, Publ.*, No. **256**, 1917, pp. 236). The country rock consists of a pre-Devonian metamorphic series of schists, slates, phyllites, quartzites, etc., which are intruded by a great mass of granodiorite. This, in its turn, is intersected by numerous sets of dykes. The lodes occur along fault-fissures in crushed granodiorite and consist of quartz reefs carrying pyrite, galena, zinc-blende, and native gold. Of rarer occurrence are tellurides of gold and silver, arsenopyrite, and chalcopyrite. The two main auriferous belts are connected with groups of diorite-porphyrite dykes. The ores are believed to have been deposited by ascending magmatic waters at intermediate depths.

The Amisk-Athapapuskow Lake District (Saskatchewan-Manitoba), described by E. L. Bruce (*Geol. Surv. Canada, Memoir*, **105**, 1918, pp. 91), consists of an immense series of Archæan granites, schists, and gneisses, partly covered by Ordovician limestones. Some rich gold quartz veins occur in the older Archæan rocks, along with chalcopyrite-sphalerite replacements which carry low gold and silver values. Both types are believed to be genetically connected with the granite batholiths.

The controversy as to the origin of the Sudbury nickel ores is carried further by Roberts and Longyear in a paper on the

genesis of the ores as indicated by recent exploration (*Trans. Amer. Inst. Min. Eng.*, 1918, **59**, 27–67). They leave uncertain the nature of the differentiation by which the micropegmatite and norite members have been produced; but believe that the sulphides were carried down by the norite, remained in solution while the norite consolidated, and were concentrated in association with an acid extract which solidified as granite. The presence of mineralisers in these solutions enabled them to replace the wall rocks to some extent. The dominant factor controlling the deposition of the ore is thus magmatic segregation *in situ*; but the ore belongs to the latest stage in the history of the magma. This view has a considerable resemblance to that put forward by Goodchild (see SCIENCE PROGRESS, **13**, 1919, 376).

An occurrence of chalcopyrite and bornite in syenite and syenite-pegmatite dykes (La Fleur Mountain, Washington) may be added to the list of the magmatic sulphides, according to an investigation by McLaughlin (*Econ. Geol.*, 1919, **14**, 403–10). These minerals are later than the normal rock minerals, a sequence which is found to hold with most occurrences of magmatic sulphides.

An important paper by Billingsley and Grimes (*Trans. Amer. Inst. Min. Eng.*, 1918, **58**, 284–368) seeks to compare the origin of the various ore deposits associated with the great Boulder batholith of Montana, in which Butte, the most important copper region of the world, is situated. It was found that the ores are associated with intrusive rather than extrusive igneous action, the largest number of deposits being connected with the period of greatest intrusion. Successive magmatic differentiates furnish increasing proportions of vein-forming solutions. The sequence of ore deposition follows a definite order in any igneous phase: first, contact and border; second, internal segregation; and third, fissure and fault veins filled from deep-seated sources. A number of other interesting generalisations for the student of ore deposition in relation to igneous action are given, which it is impossible to quote here.

The Biwabik iron-bearing formation (taconite) of the Mesabi Range, Minnesota, is believed by Grout (*Econ. Geol.*, 1919, **14**, 452–64) to have been deposited in shallow water mainly by precipitation through organic processes, resulting in the production of lean, ferruginous cherts, with more or less iron-bearing carbonates, ferric oxide, and greenalite. Alternating with periods of precipitation came periods of solution, leaching, oxidation, and wave action, producing conglomerate and granular rocks much enriched in iron, and probably some layers of pure ferric oxide. Deep burial under slates developed

sufficient pressure and heat to recrystallise a large part of the formation.

The unique seam of iron pyrites occurring at Trefriw (Carnarvonshire) is 6 feet thick, and separates an intrusion of dolerite from black shales of *Dicranograptus* age (R. L. Sherlock, *Q.J.G.S.* 1919, **74**, pt. 2, 106–15). It lies at the horizon of certain pisolitic iron ores in North Wales. The pyrites is believed to have originated by the action of heated magmatic waters, derived from the dolerite magma, upon the iron ore.

For the pyritic deposits of Röros, Norway, Ries and Somers (*Amer. Inst. Min. Eng.*, 1918, **58**, 244–64) favour the view that these are due to magmatic injection into the enclosing schists. In support of this hypothesis they point to the absence of hydrothermal alteration in the wall rocks; the uniformly close association of the ore with gabbro; the presence of unorientated inclusions of wall rock in the ore; and the nature of the ore itself, which is massive, occasionally porphyritic, and not always conformable to the enclosing schists.

In a paper on geosynclines and petroliferous deposits, M. R. Daly (*Trans. Amer. Inst. Min. Eng.*, 1918, **57**, 1054–70) shows that the latter follow the main zones of dislocation upon the globe. "Petroliferous accumulations are generally coincident with diastrophic deformation, synchronous with them, and essentially a result of them."

Garfias and Hawley describe funnel and anticlinal ring structure associated with igneous intrusions in the Mexican oil-field (*Trans. Amer. Inst. Min. Eng.*, 1918, **57**, 1071–88). By these terms is meant the dragging down of strata around the periphery of a volcanic vent, producing an anticlinal ring surrounding the volcanic funnel. This is the familiar structure seen in many vents of the Midland Valley of Scotland, as noted by the authors. In the Mexican oil-field these structures have been found to be ideal for the accumulation of oil.

Prof. P. G. H. Boswell has followed up his well-known work on glass sands by an equally useful memoir on refractory sands for furnace and foundry purposes in *A Memoir on British Resources of Refractory Sands for Furnace and Foundry Purposes*, pt. I (published at the instructions of the Ministry of Munitions of War, by the Imperial College of Science and Technology and the University of Liverpool. London: 1918, pp. 246). This contains by far the fullest discussion of the characters of moulding sands that has yet appeared. A novel point is the insistence upon the value of the pellicle of ferric oxide around the grains of the red Bunter moulding sands as a medium for the retention of water, thus strengthening the "bond" of the sand. The concise chapter describing the

industrial uses for which refractory sands are required will also be much appreciated by geologists.

Under an unpromising title, L. Reinecke (Non-bituminous Road Materials, *Econ. Geol.*, 1918, **13**, 557–97) presents the best modern study of road metals, including the methods of investigation, and geological considerations in highway construction.

In a study of the barite deposits of Missouri, W. A. Tarr (*Econ. Geol.*, 1919, **14**, 46–67) describes the mineral as occurring in veins, disseminated deposits, sink-hole or cave deposits, and residual deposits, of which the last-named are the most productive. The deposition of barite by circulating ground waters is considered improbable; and the view that ascending thermal solutions deriving their barium from deep-seated igneous rocks are responsible for the primary deposits of the mineral is favoured.

In an investigation of the potash salts of the Punjab Salt Range and of Kohat, Stuart (*Records, Geol. Surv., India*, 1919, **50**, pt. 1, 28–56) arrives at the conclusions that no continuous bed of potash is to be found in these localities. The rock-salt is foliated, and the contained potash occurs in discontinuous lenticles and irregular folia.

This work led to a new hypothesis as to the origin and history of the rock salt and gypsum of the same regions (*ibid.* pp. 57–59). While the salt was originally sedimentary, the nature of the banding, the frequent lenticular arrangement, the striking flow structure, and the nature of the associated salt marl beds show that the salt has been subjected to flow, the direction of which has not coincided with the original bedding. The gypsum is believed to be a secondary mineral formed by the reaction of calcareous material in the adjacent sediments with the sulphuric acid formed by the decomposition of iron sulphides within the salt formation.

In a discussion of the origin of asbestos and asbestiform minerals S. Taber (*Trans. Amer. Inst. Min. Eng.*, 1918, **57**, 62–98) comes to the conclusion that the fibrous nature of these minerals is due to the accentuation of a normal prismatic habit, through the limitation of crystal growth by physical conditions.

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MINERALOGY AND CRYSTALLOGRAPHY. By ALEXANDER SCOTT, M.A., D.Sc.

AN elaborate account of crystal optics is given by J. Beckenkamp (*Statische und kinetische Kristalltheorien*, **2**, pp. 1–523, 1915). The second part of the same volume (pp. 524–646) is occupied by a discussion of the relationships between the various polymorphic modifications of silica, especially quartz and tridymite. The relative specific gravities, double refraction, and so forth, are explained on the basis of the crystal structure (cubo-octahedral) assigned to the two minerals; the analogous relationships between pyrites and marcasite are also considered. The optical effects observed when plates of

dextro- and laevo-rotatory quartz are superposed in various ways are discussed mathematically by T. Liebisch (*Sitz. preuss. Akad. Berlin*, **15**, 821, 1918). In a paper on "The Perpendicular Passage of Linearly Polarised Plane Waves through Plane Parallel Plates of Transparent Inactive Crystals" (*Ann. Phys.*, **58**, 165, 1919; *Sci. Abs.*, **20A**, 219, 1919) M. Berek calls attention to the fact that ordinary optical theory ignores the wave-absorption in the crystal, as well as certain boundary effects. The former is negligible in the case of completely transparent media, but the effect on the amplitudes of the reflections at the boundary surfaces is appreciable since the reflecting power varies with the refractive index. The discrepancy between the theoretical (geometrical) and observed results should become appreciable in crystals having high double refraction. Adopting the conventional method of representing the component amplitudes by two sides of a parallelogram, it is found that, for perpendicularly incident light, the resultant diagonals, obtained from the electromagnetic and geometrical theories respectively, do not coincide. An expression connecting the tangent of the angle between the diagonals and the refractive indices is given, and according to this relation the former increases with the double refraction. The phenomena seem to be of an order of magnitude sufficiently great for experimental determination. The reflection of infra-red rays by various crystals has been investigated by T. Liebisch and H. Rubens (*Sitz. preuss. Akad. Berlin*, **16**, 198, 1919). The minerals examined include quartz, apatite, rhombohedral and orthorhombic carbonates and the orthorhombic sulphates, the variation of reflecting power with the wave-length being indicated by means of curves. A theoretical paper by M. Born (*Phys. Zeit.*, **19**, 539, 1918), deals with the derivation of the frequency of infra-red oscillations in diatomic crystals by means of the space lattice, while a mathematical discussion of dispersion is given in another paper by the same author (*Verh. deutsch. phys. Ges.*, **19**, 243, 1917). K. Brieger (*Ann. Phys.*, **57**, 287, 1918) has investigated the optical behaviour of water of crystallisation by determining the displacement of the position of proper oscillation, compared with that of free water, in several types of sulphates, such as the alums, some orthorhombic monoclinic sulphates and selenates, and in analcite. The curve of the reflection intensities for water shows certain maxima, which, however, are double in the case of most of the salts examined. Further, the displacement in the case of sulphates is generally appreciable and towards the longer wave-lengths, but in analcite, on the other hand, it is in the opposite direction. The results are held to be in agreement with the work of Schaefer and

Schubert (*Ann. Phys.*, **50**, 283, 339, 1916; **55**, 397, 577, 1918) on the rôle of water in the alums.

The determination of optical properties by means of the microscope is becoming increasingly important in connection with the identification of artificial chemical substances as well as minerals. The determination of refractive indices is discussed by F. E. Wright (*Jour. Wash. Acad. Sci.*, **6**, 465, 1916; *Jour. Amer. Chem. Soc.*, **38**, 1647, 1917), J. W. Evans (*Min. Mag.*, **18**, 45, 52, 1916; **18**, 130, 1917), and S. Tsuboi (*Jour. Geol. Soc. Tokyo*, **25**, 38, 1918). The dispersion phenomena of minerals, particularly the felspars, is treated in detail in a series of papers by S. Kôzu (*Min. Mag.*, **17**, 189, 1915; **17**, 237, 253, 1916; *Bull. Soc. franc. Min.*, **40**, 36, 1917; *Jour. Geol. Soc. Tokyo*, **25**, 31, 43, 1918). E. T. Wherry (*Jour. Washington Acad. Sci.*, **8**, 277, 319, 1918; *Amer. Min.*, **3**, 134, 1918) has investigated the relations between the optical properties and crystalline form of a number of tetragonal organic substances. It is found that the ratio of the refractivities obtained by substituting the " ordinary " and " extraordinary " refractive indices in the Lorentz-Lorenz formula is the inverse of the crystallographic axial ratio. The same author (*Jour. Amer. Chem. Soc.*, **40**, 1852, 1918) gives an account of the optical properties of *α-d*-lyxose, *α-d*-xylose and *β-d*-arabinose, and L. Delhaye (*Bull. Soc. franc. Min.*, **41**, 80, 1918) has made a detailed study of the optical properties of sodium chromate tetrahydrate.

The fact that crystals under certain conditions emit light rays has long been known, and the phenomena have been differentiated into various types according to the nature of the exciting agent (Wiedemann, *Ann. Phys.*, **34**, 446, 1888). Thus when the luminescence is due to light or cathode rays, the phenomenon is photo- or cathodo-luminescence respectively; tribo-luminescence appears when the material is crushed, crystallo-luminescence during crystallisation, and so forth. The terms fluorescence and phosphorescence are used accordingly as the emission of light ceases when, or continues after, the exciting agent is removed. Cf. S. E. Sheppard *Illum. Eng.* **10**, 178, 1917; *Sci. Als.*, **20A**, 471, 1917. In many cases the phenomena seem to be due to the combination of ions or to other chemical reactions, and this view is adopted by H. Weiser (*Jour. Phys. Chem.*, **22**, 439, 480, 576, 1918) in a recent examination of several substances showing crystallo- and tribo-luminescence. He concludes that, so far as the former is concerned, in the case of such alkali haloids, sodium and potassium chlorides, and potassium bromide and iodide, as show it, the reaction responsible for the effect is the combination of the ions to form the undissociated salt. It is found that factors such as con-

centration, temperature, and viscosity of the solutions and the presence of colloids exert an influence on the intensity of the light emitted. In the third paper, the relation of tribo-luminescence to crystallo-luminescence is considered. All substances possessing the latter property also possess the former, but not *vice versa*. Further, the nature of the light emitted by such substances as arsenious oxide and alkali sulphates during grinding is the same as that emitted during rapid crystallisation. In each case the effect is ascribed to the re-formation of molecules broken up by crushing and solution respectively. The intensity of the light, as might be expected, varies greatly with the nature of the substance.

So far as kathodo- and photo-luminescence is concerned, the main result of numerous investigations (*e.g.* Wiedemann and Schmidt, *Ann. Phys.*, **56**, 208, 1895; Lenard and Klatt, *ibid.*, **15**, 225, 425, 603, 1904) has been to establish that the phenomenon is due to the presence of a small amount of impurity, supposed by Schmidt (*Die Kathodenstrahlen*, 99, 1907) to be in solid solution.

According to E. L. Nichols and H. L. Howes (*Phys. Rev.*, **11**, 327, 1918; **12**, 351, 1918; *Proc. Nat. Acad. Sci.*, **4**, 305, 1918), the luminescence of calcite from Franklin Furnace, N.J., is due to the presence of oxide of manganese, while prepared impure calcium carbonate behaves in the same way. It is suggested that the luminous centres may be calcium oxide in the neighbourhood of which the impurity is located. From the evidence of curves showing the decay of the luminosity, two types of phosphorescence are recognised, one in which the slope of the decay curve diminishes with time, and another in which it increases. In the case of natural calcite, cathodo-luminescence belongs to the latter type, and photo-luminescence to the former. The effects of temperature on both types and on different parts of the spectrum were also investigated.

E. MacDougall, A. W. Stewart, and R. Wright (*Jour. Chem. Soc.*, **111**, 663, 1917. Cf. J. W. T. Walsh, *Proc. Roy. Soc.*, **93A**, 550, 1917) have made a detailed investigation of the factors influencing the phosphorescence of zinc sulphide. The effects, due to the nature of impurities, temperature, and so forth, on the luminescence of zinc sulphide under the excitation of kathode rays and rays from radio-active materials have been examined, and it is found that the results obtained in the two cases are by no means concordant. The view that the maximum luminescence occurs when the sulphide is in a state of incipient crystallisation, in preference to the amorphous and truly crystalline states, is verified. C. Waggoner (*Phys. Rev.* **9**, 175, 1917), on the other hand, maintains that the luminescent spectra of zinc salts are independent of the mode

of excitation. E. Karrer and E. H. Kabakjian (*Jour. Franklin Inst.*, **186**, 317, 1918) consider that the luminescence of radio-active salts is due to changes in the crystalline state. M. B. Hodgson (*Phys. Rev.*, **12**, 431, 1918), in a paper on the physical characteristics of the fluorescence produced by X-rays, finds that the fluorescence varies with the nature of the crystal structure so far as calcium tungstate is concerned. The question of cathodo-luminescence is also considered by F. G. Wick and L. S. McDowell (*Phys. Rev.*, **9**, 241, 1917) and T. B. Brown (*ibid.*, **11**, 38, 1918).

In a paper on "Radio-activity and the Coloration of Minerals," E. Newbery and H. Lupton (*Proc. Manchester Lit. Phil. Soc.*, **62**, Mem. 10, 1918) come to the conclusion that the colour and thermo-luminescence of many minerals are due to the action of radio-active materials, either during formation or subsequently. The colours produced by α, β, and γ rays may be different, but in each case they are most probably due to the dissociation of impurities present in minute quantities and not to the dissociation of the main substance. This latter point is regarded as still in doubt (cf. Goldstein, *Nature*, 494, 1914), but the impossibility of a pure substance showing phosphorescence is considered to be established (cf. Lenard and Klatt, *loc. cit.*; G. Urbain, *Ann. Chim. Phys.*, **8**, 222, 289, 1909; E. Tiede, *Ber.*, **49**, 1745, 1916). Similar methods have been employed by B. Blount and J. H. Sequeira (*Jour. Chem. Soc.*, **115**, 705, 1919), in an investigation of the colour of fluorspar. It is concluded that the colour is due to the presence of traces of organic material, not to radio-active effects. Since Lupton and Newbery came to the opposite conclusion after an exhaustive examination of the same mineral, the question of the origin of the colour must still be considered to be open.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., University College, London.

Anatomy and Cytology.—Further details regarding the structure of the stele of *Platyzoma microphyllum* are furnished by McLean Thompson (*Trans. Roy. Soc. Edin.*), who finds that the stele is devoid of perforations or leaf gaps, but that considerable changes occur with respect to the medulla and the internal endodermis. The latter was found in one specimen to contract and finally disappear. Expansion of the pith was accompanied by a reappearance of the endodermis, and the same features were again repeated at a higher level in the stem.

The structure of Protoplasm formed the subject of Prof. Harper's Presidential Address to the Botanical Society of America (*Amer. Jour. Bot.*), in which he considers especially

the relation of modern chemical and physical knowledge to Cytological interpretation. It is pointed out that the most important advances in our knowledge of cell architecture have been the recognition of localised spatially differentiated regions in which certain processes occur. The work on elaio-plasts is particularly cited, whilst plastids, chromosomes, and centrosomes furnish further examples. Attention is drawn to the occurrence of almost every gradation from the undifferentiated "chromatophore" of *Hydrodictyon* to the highest types exhibiting not merely a well-delimited outline, but also a special membrane. With regard to the relation of chrondriosomes to the organised bodies of the adult cell, the opinion is expressed that far more evidence is required than has so far been adduced.

The author proceeds to explain the structure of the cell conceived as a polyphase colloidal system of complex chemical compounds. Such a system, however, whilst consistent with many observed facts, would, so far as we are aware, be devoid of polarity and would exhibit an absence of the non-homogeneity which must be postulated for protoplasmic structure in order to account for the diverse chemical processes which proceed simultaneously within the cell.

Our knowledge of multinuclear cells has been increased by the account furnished by Beer and Arber (*Proc. Roy. Soc.*), who enumerate 117 species belonging to sixty families in which such cells are present. These examples represent Pteridophyta, Gymnosperms, Dicotyledons and Monocotyledons, and almost every variety of vegetative organ. Most commonly to be met with in the stem, this type of cell appears to be characteristic of active tissues, and is regarded as a normal feature in the growth of the majority of the higher plants succeeding the meristematic phase. The power of division would appear to be lost first by the cytoplasm and only subsequently by the nucleus. By this means it is suggested that a more effective relation between chromatic material and cytoplasm may be maintained. In more than 100 species the several nuclei were found to arise by mitotic division, but though this proceeds normally up to the spindle phase and a cell plate is even formed, this latter is resorbed and the daughter-nuclei become enclosed in a hollow sphere which enlarges till it is coextensive with the cytoplasm lining the cell wall. Subsequently one or more of the nuclei may degenerate so that a uninucleate condition is established.

The Cytology of *Nemalion multifidum* has been studied by R. C. Cleland (*Ann. Bot.*), who finds that there is no cytological alternation of generations, but that, with the exception of the first mitosis of the zygote, the chromosome number is eight

throughout the life cycle. It is during this first division of the zygote that reduction is effected.

Morphology.—A new Lepidodendraceous genus is described by Bassler (*Bot. Gaz.*) under the name *Cantheliophorus*. The species, of which some were already imperfectly known, and seven additional representatives are here described, are characterised by a large lamellar sporangiophore in the radial plane of the cone axis and bearing a sporangium on each side. There is a prominent sterile lamina, but no ligule has as yet been detected.

In the same journal Stout describes the occurrence of almost every gradation, in the flowers of *Plantago lanceolata*, from the typically hermaphrodite to those which are completely female. The various grades are characterised by different degrees of development of the stamens, and indications are not lacking that there is also variation with respect to the pistil. Even on the same plant similar variability may be exhibited.

It appears from experiments by Kundson (*Amer. Jour. Bot.*) that in culture solutions the sloughed root cap cells of Pea and Corn are still alive at the time of detachment, and may remain so for a prolonged period.

Taxonomy.

Spencer le More contributes to the *Journal of Botany* descriptions of new species belonging to the genera *Buchnera*, *Rhamphicarpa*, *Streptocarpus*, *Justicia*, *Dicliptera*, *Lippia*, *Clerodendron* (4 spp.), *Loranthus*, *Acalypha* (2 spp.), and *Mollinedia*.

In the same Journal Wernham describes further Rubiaceæ belonging to *Mussaenda* (2 spp.), *Sabicea*, *Bertiera*, *Randia*, *Ancaralia* (2 spp.), and *Morinda*.

A critical review of species of *Echium* appears in the *Journal of the Linnean Society* from the pen of Mr. Lacaita.

A new genus of Marattiaceæ (*Protomarattia*) is distinguished by Bunzo Hyata from Tonkin. It differs from Marattia in possessing linear sori borne on a simply pinnate frond, and in having more numerous loculi in each synangium. New species of *Phoma* are enumerated in the *Kew Bulletin*, and in the *Trans. Roy. Soc. South Africa*, Pole-Evans and Bottomly describe a new species of *Diplocystis*.

Economic.

Barber, in an extensive paper of over 150 pages, published by the Indian Department of Agriculture, deals with the underground branching of the sugar-cane.

Dixon (Notes from the Botany School, Dublin) enumerates the microscopical characteristics of a number of species commercially known as Mahogany.

Ecology.—The presence of colloidal substances as normal constituents of the soil is a factor regarding the influence of which our knowledge is all too small. Jenning (*Amer. Jour. Bot.*) has investigated the effect of this class of substances on

the growth of Wheat in Culture solutions. The addition of Agar was found to have a stimulating effect on growth when the nutrient solution had a low concentration, but the reverse effect when the concentration of nutrient salts was raised. The addition of fine quartz sands caused a decrease in dry weight which was the most marked when the finest-grained sand was employed, and a similar depression was observed with both Ferric and Aluminium hydroxide. It is suggested that the concentration of the nutrient substances becomes reduced through the presence of the adsorbent. Increased weight with colloidal silica appears to be due to direct absorption of this substance, a result in agreement with the conclusions of Gregoire. Other workers have, however, found that finely-divided material tends to promote growth, and this has been explained as an outcome of the adsorption of toxins.

In an interesting account of studies on Pasture, Jenkin (*Pasture Studies*, Bangor, 1919) describes the changes of vegetation in an area laid down to grass. All the areas studied showed a decrease in the proportion of sown grasses, but an increase of the grasses not sown. The influence of the method of treatment, and the character of the original seed mixture, are emphasised.

Blackman (*Ann. Bot.*) calls attention to the fact that the rate of growth increment, in the earlier phases, follows the compound interest law, and is therefore dependent on the initial capital, as represented by the size of the seed and the efficiency of the plant to form new material. Similarly, at any given phase the amount of material present is an important factor in determining the increment of the future.

PLANT PHYSIOLOGY. By PROF. WALTER STILES, M.A., University College, Reading. (Plant Physiology Committee.)

Influence of Environmental Factors on Growth and Development of Plants.—During the last few years a vast amount of work has been published dealing with the influence of various factors of the environment on the development of plants; consequently it will only be possible here to review a representative selection from recent researches on this question.

The chief environmental factors are light, temperature, and humidity. In regard to light the measurement offers considerable difficulties. D. T. MacDougall and H. A. Spoehr (*Science*, **45**, 616–8, 1917) point out that the only reliable measurements of solar radiation available to plant physiologists have been made with the Angström and Abbot type of pyrheliometer. This records in heat units the total insolation, but is not sufficiently sensitive to the violet end of the spectrum.

The photo-electric cell of Elster and Geitel is not open to this objection, and it is therefore suggested by the authors as a more satisfactory instrument for plant physiological measurements. Measurements of the same insolation made with the two instruments show considerable differences.

Undoubtedly some of the most important investigations on the influence of environmental factors are those of Klebs (*Biol. Zentralbl.* **37**, 373–415, 1917; *Flora*, **11–12**, 128–51, 1918). In the first of these papers cited, evidence is brought forward in support of the author's thesis that growth and rest periods in plants are ultimately the result of external conditions. In the beech, for example, the normal development of leafy shoots from the bud only continues for a few weeks at most. By the employment of the continuous illumination of an Osram lamp and by limiting the number of illuminated shoots to one at a time, it was found possible to make a small beech tree develop leafy shoots continuously for eight months. In the same way an oak tree was made to develop leafy shoots continuously for a period of four summer months and three winter months. A large number of experiments were also carried out with *Ailanthus glandulosa*, in which the influence of environmental factors on development was clearly shown. Among the more important points brought out in this investigation are the fact that the development of a young plant of *Ailanthus glandulosa* can be stopped by gradually decreasing photosynthesis, or by decreasing the content of mineral salts in the soil, while the rest period can be brought to an end by bringing the plant into a warmer, more humid and dark environment, by introducing the plant into a soil rich in nutrient salts, or by continuous illumination.

F. Weber (*Ber. deut. bot. Ges.* **34**, 7–13, 1916) opposes the view of Klebs that under natural conditions the rest period of the beech in winter is due to low light intensity, since potted plants of the beech will unfold their leaves in midwinter when exposed to an atmosphere of acetylene. Weber's argument seems, however, to be beside the point.

In the second paper of Klebs referred to above, the results of an investigation into the causes of the transition from vegetative growth to flower-production are recorded, the plant investigated being a species of *Sempervivum*. It had been found earlier that this transition takes place when certain changes occur in the environmental conditions. For instance, humidity accelerates growth and hinders flower-production, and conversely intense transpiration acts in the reverse way. Klebs had previously expressed the opinion that the essential condition for flower-formation is the accumulation of carbohydrates in the plant. Hence, increase in

carbon assimilation is decisive. In the paper under special consideration here the environmental factor specially considered is light. By the use of the artificial illumination of an Osram lamp, Klebs was able to bring *Sempervivum* to flower at any time of year, but a certain light intensity is necessary for development of the inflorescence. A continuous low light-intensity is sufficient for the flower rudiments to form, but without the necessary light-intensity the inflorescence does not develop further. Curiously enough it is only at higher temperatures that intermittent light and darkness can be made to inhibit flower-formation; at low temperatures (4° to 6° C.) darkness does not hinder flower-production. As regards wave-length, red light accelerates the production of reproductive shoots, while blue light retards it.

Among other recent work dealing with the influence of light on growth and development may be mentioned that of C. Olsen (*Tidssk. f. Skov. og Træhdl.* **30,** 1–48, 1918), who finds the nettle (*Urtica dioica*) grows best in half-shade, while nitrate and water content of the soil are also important factors. It is also possible that light is an important condition in bringing about full development of colour in the flowers of edelweiss (*Leontopodium alpinum*). O. Rosenheim (*Biochem. Journ.* **12,** 283–9, 1918) transplanted some plants of this species, collected in the Alps at an average altitude of 2,000 metres, to a garden in a London suburb. In this latter habitat the colour of the flowers was much less intense, and on analysis it was found that the quantity of chromogenic substance in the flowers was only a quarter of that contained in flowers growing in their native environment. Rosenheim argues that this may be an adaptation of the plant to less intense insolation, the pigment in the flower in its native habitat serving a protective function against too intense sunlight. While it is by no means certain that this is really the function of the pigment, it appears to be premature to speak of adaptation to the less intense insolation in the case of the plants grown in London; on the other hand, it may well be that light is an important factor in determining the extent of development of the colour.

The influence of temperature on growth has attracted much attention in recent years. E. Talma (*Kon. Akad. Wetensch. Amsterdam,* **24,** 1840–4, 1916) has measured the increase in length of radicles of *Lepidium sativum* grown in different temperatures and taken the values so obtained as measures of the rate of growth. The radicles were about 8 mm. long and the increase in length was that taking place in 3·5 hours. It was found that at 0° C. growth still took place, and that the growth-rate increased with increasing temperature up to 28° C. Above this temperature the rate of growth fell rapidly

with increasing temperature. Even between 0° C. and 28° C. the van't Hoff rule only holds over a small range of temperature, the temperature coefficient decreasing with rise of temperature. A more extended series of measurements has been made by I. Leitch (*Ann. of Bot.* **30**, 25–46, 1916), who measured the rate of growth of the main root of *Pisum sativum* at different temperatures. This author's experiments were of two kinds, long-period experiments extending over 22·5 hours, in which the increase in length of the roots was measured macroscopically, and experiments in which measurements were made every half-hour with the aid of a microscope. The author concludes from her experiments that the relationship between temperature and growth does not obey the van't Hoff rule, but between—2° C. and 29° C. agrees with the curves found by Krogh for "standard metabolism" (see Krogh, *Respiratory Exchange of Animals and Man*, London, 1916) and by Keuper for respiration in *Pisum*.

Above 29° C. it is impossible to construct a curve to show the relationship between temperature and growth, because the latter decreases with time. Consequently between 30° C. and 40° C. time-growth curves must be constructed for each temperature. Above 44·5° C. there is no growth.

From among observations made in the field those of H. N. Vinall and H. R. Reed (*Journ. Agric. Research*, **13**, 133–47, 1918) may be selected. These observations have been made on the American crop plant *Sorghum*. This plant is semi-tropical and will not grow well if the temperature is too low. On the other hand there is a marked contrast between its growth in California and in Washington, although the mean temperatures and "total positive heat units" available in the two localities are not much different. The differences appear to be ascribable to differences in illumination. On the other hand, temperature has a marked influence on flower and fruit formation, higher temperatures during flowering and fruiting depressing the yield of grain.

W. B. McDougall (*Amer. Journ. Bot.* **3**, 384–92, 1916), working with forest trees, finds that growth starts in the spring as soon as the temperature of the soil is sufficient for root-absorption to commence, and ceases in the autumn when the soil becomes too cold. Sometimes there is a summer resting period, but not always. When this does happen it is due to decrease in water-supply, and is not a periodicity phenomenon.

From among other work dealing with humidity of the soil may be mentioned that of H. A. Allard (*Amer. Journ. Bot.* **3**, 493–501, 1916), who found that plants which had become stunted through growing in dry soil nevertheless bore as many leaves as properly developed plants. On the other

hand the inflorescence was reduced in size in the stunted plants, and in extreme cases might be reduced to a single flower. Finally mention may be made here of a recent publication of J. E. Coit and R. W. Hodgson (*Univ. California Publ. Agric. Sci.* **3**, 283–368, 1919), who have investigated the shedding of young fruits of the Washington Navel Orange. This they find is due to the stimulus given to abscission owing to daily water deficits in young developing fruits. Among other observations they find that in this tropical shade plant the stomata lose their power of movement early in the life of the plant and remain practically closed. As a result transpiration occurs to a very great extent through the epidermis, and 25 per cent. takes place through the upper epidermis. The orange thus appears to stand in great contrast to the cereals barley, wheat, oats, and rye, in which J. Gray and G. J. Pierce (*Amer. Journ. Bot.* **6**, 131–54, 1919) have shown that the stomata open with light and close with darkness. These observations have been made, however, only with young plants.

PALÆOBOTANY IN 1918. By MARIE CARMICHAEL STOPES, D.Sc., Ph.D., Lecturer in Palæobotany, University College, London.

THE reduction in output of Palæobotanical work due to the war is even more apparent this year than in the previous war years, and the papers published in 1918 are few. The younger workers who might have otherwise been expected to produce memoirs have nearly all been engaged in some form of "war-work," while not only death, but serious illness has been busy among the elder leaders of the science in several countries. The death of Dr. G. A. N. ARBER was recorded in the *Annals of Botany* (vol. xxxii, No. 128) in a short biography by Dr. D. H. Scott, who concluded by saying: "When he died so prematurely on June 14, 1918, he left behind extensive unpublished manuscripts."

The number of publications worth note in the current review being so small, the grouping under various headings which has hitherto been followed by this annual survey will not be maintained.

On the TERTIARY palms work symbolic of the later phases of the war appeared in Tokyo by Prof. A. KRYSHTOFOVICH, who escaped the terrors of Russia and meanwhile added to the scanty information hitherto available about the Tertiary floras of Japan ("Two Ferns and a Palm from the Tertiary of the Takashima Coal Mine," *Jour. Geol. Soc. Tokyo*, vol. xxv; and "Occurrence of the Palm *Sabal nipponica*, n. sp., in the Tertiary Rocks of Hokkaido and Kyushu," *Jour. Geol. Soc.*

Tokyo, vol. xxv, No. 303). The species is based on good leaf impressions, and though in some parts of the world fossil palms are common, these are of interest as they represent a region from which very little plant material is known, and which appears to correspond to the Laramie in America. The specimens from Hokkaido indicate a warmer climate than at present. American Tertiary plants were described by Dr. E. W. Berry ("Fossil Plants from the Late Tertiary of Oklahoma," *Proc. U.S. Nat. Mus.*, vol. liv, pp. 627–36). These Dicotyledonous leaf impressions were in some respects similar to the Florissant flora, but Berry inclined to place them in the Upper Miocene.

From Berry also we had an interesting general account of "The Jurassic Lagoons of Solenhofen" (*Sci. Monthly*, October), in which was pictured both the plant and animal life. As regards the former, Berry explained its relative scarcity in comparison with the innumerable animals by "the macerating action of the water, the non-deciduous character of the foliage of Jurassic plants, the activity of bacteria in the warm sea water, and most of all the situation of the deposits, away from any estuary with its stream-borne load of land-derived débris."

Records from the Antipodes were represented by A. B. Walkom's continued publications on the Mesozoic floras of Queensland. In "The Flora of the Maryborough (Marine) Series" (*Queensland Geol. Surv.*, publ. No. 262) a number of species, some of them new, were described, and were of special interest as the only record available of the land vegetation of the time, although found in a marine deposit. The species range from Equisetites to Conifers, the greater part being Gymnosperms.

The same author gave a more general account of the Mesozoic floras of his country, accompanied by sketch maps of ancient land distribution, in "The Geology of the Lower Mesozoic Rocks of Queensland, with special reference to their Distribution and Fossil Flora, and their Correlation with the Lower Mesozoic Rocks of other Parts of Australia" (*Proc. Linnean Soc. N.S. Wales*, vol. xliii, pt. 1). Coals are distributed through some of the horizons, which, of course, lend the plant records additional interest. Printed as it is on thin paper and in small octavo size, it is externally inconspicuous in the year's output: had it been produced on thick paper and in handsome quarto, as has been the fashion in other quarters, this memoir would then have been obviously what it is actually, namely, the most significant publication of the year's general Palæobotany.

Two memoirs on the detailed anatomical features of

Palæozoic plants were published by Dr. D. H. SCOTT: "The Structure of *Mesoxylon multirame*" (*Ann. Bot.*, vol. xxxii, No. 127), well illustrated by microphotographs and drawings of the stem and leaf base tissues of the species earlier described by Scott and Maslen, including an amended diagnosis. The accumulated detail concerning this plant accentuates the relation of the genus to *Cordaites*.

The LOWER CARBONIFEROUS genus *Calamopitys* (recently restored to prominence by the description of *C. americana* by Scott and Jeffrey) was dealt with fully by SCOTT ("Notes on *Calamopitys*, Unger," *Linn. Soc. Jour.*, vol. xliv). In this monograph the five species described in the genus were critically considered and discussed, with supplementary details fully illustrated. The general result being to retain the species in the genus, and not separate *C. fascicularis* and *C. beinertiana* under the generic name *Eristophyton*, as Zalessky proposed.

The divisions of the Coal Measure series in France received treatment at the hands of P. BERTRAND ("Les grandes divisions paléontologiques du Stéphanien du bassin de la Loire," and also "Caractères distinctifs des flores houillères de Saint Etienne et de Rive-de-Gier," *Compt. rend. Acad. Sci.* vol. clxvii). In the Stéphanien three zones were recognised and subdivided according to their characteristic plant species; the work being a preliminary to more extensive detailed re-examination of the famous French deposits.

On COAL itself G. Knox published a semi-popular résumé of current knowledge of its origin and structure (*Proc. S. Wales Inst. Engineers*, vol. 34, No. 1).

The "Monograph on the Constitution of Coal," by M. C. STOPES and R. V. WHEELER (*Pub. Dept. Sci. Indus. Research*), is a critical examination of what is known up to date of the details, chemical, palæobotanical, and general, of the inherent composition of ordinary Palæozoic bituminous coal, supplemented by the views and new observations of the authors. The work is comprehensive, and brings within one cover the essential facts and hypotheses bearing on this much-discussed and difficult subject, and is accompanied by an extensive bibliography.

ZOOLOGY. By PROF. CHAS. H. O'DONOGHUE, D.Sc., F.Z.S., University of Manitoba, Winnipeg, Canada.

Protozoa.—Pack, in "Two Ciliata of Great Salt Lake" (*Biol. Bull.*, vol. xxxvi, April 1919), has investigated the effect of a reduction in the salinity of the water upon two ciliates. Great Salt Lake is very saline indeed, and has but a limited number of living forms in it. Two ciliates were chosen and kept in

water which had its salinity reduced by continuous dilution. It was found that these animals were capable of a certain amount of adaptation, becoming more and more active, larger, more flexible, and exhibiting a marked shortening of the feeding cirri.

Invertebrata.—Papers include:

Gemmil, "Ciliary Action in the Internal Cavities of the Ctenophore *Pleurobrachia pileus*" (*Proc. Zool. Soc*, Pts. III and IV, March 1919).

Faust, in a series of three papers—"Notes on the Excretory System of an Amphistome *Cercaria convoluta* nov. spec." (*Biol. Bull.*, vol. xxxvi, April 1919); "The Excretory System in Digenea: II, Observations on the Excretory System in Distome Cercariæ" (*ibid.*, May); and "III, Notes on the Excretory System in a Monostome Larva, *Cercaria spatula* nov. spec." (*ibid.*)—deals particularly with the flame cells. They are important since their group plan is indicative of at any rate sub-family relationship. A fundamental homology exists between the excretory system of both redia and cercaria generations.

Other papers include:

Thompson, "Association of Somatic and Germ Cells in Cestodes" (*Biol. Bull.*, vol. xxxvi, May 1919), and "The Degeneration of Yolk Glands and Cells in Cestodes" (*ibid.*); Drew, "Sexual Activities of the Squid, *Loligo pealii* (Les.): II, The Spermatophore: its Structure, Ejaculation, and Formation" (*Jour. Morph.*, vol. xxxii, June 1919); and Essenberg, "The Pteropod *Desmopterus pacificus* sp. nov." (*Univ. of California Publ. in Zool.*, May 1919).

Much work has been done on the embryology of the Insecta, but it is in the main confined to the stages after the formation of the germ-band. In "The Formation of the Germ-Band in the Egg of the Holly Tortrix Moth, *Eudemis nævana* (Hb.)" (*Proc. Roy. Soc. Edin.*, vol. xxxviii, 1918), Huie describes the earlier stages in this Lepidopteron. It is a particularly fortunate species to take, since the egg is long and flattened and transparent, so that the processes can be followed in the living animal, thus enabling the author to fill in a gap in our knowledge of early insect development.

Other papers include:

Baumberger, "A Nutritional Study of Insects, with Special Reference to Micro-organisms and their Substrata" (*Jour. Exper. Zool.*, vol. xxviii, April 1919); and Bridges, "The Genetics of Purple Eye Colour in *Drosophila melanogaster*" (*ibid.*, May 1919).

Vertebrata.—The papers include:

Day, "The Physiology of the Nervous System of the Tunicate: I, The Relation of the Nerve Ganglion to Sensory Responses" (*Jour. Exp. Zool.*, May 1919).

Thomson has described "The Morphology of the Prosencephalon of *Spinax* as a Type of Elasmobranch Fore-Brain" (*Trans. Roy. Soc. Edin.*, vol. lii, Pt. II, April 1919). The difficulty of this subject is greatly increased by the terminology employed by different workers, and, as the author points out, when in the case of one area five different terms have been employed, it seems time that some clearing up of useless terms was carried out. The present paper is divided into two portions: one deals with the grey matter, in which five areas of grey cells are recognised, and the other treats of the fibre tracts, fourteen in number, and there is also a mention of four fibre complexes.

Other papers include:

Woodward, "On Two New Elasmobranch Fishes (*Crossorhinus jurassicus*, sp. nov., and *Protospinax annectens*) from the Upper Jurassic Lithographic Stone of Bavaria" (*Proc. Zool. Soc.*, Pts. III and IV, March 1919); and Boulenger, "On the Madagascar Frogs of the Genus *Mantidactylus* Blgr." (*ibid.*).

A useful "Review of the Reptilian Fauna of the Karroo System of South Africa" is furnished by Haughton (*Trans. Geol. Soc. of S. Africa*, vol. xxii, 1919). This commences with a general account of the skull of the reptilia found as fossils in South Africa, the classification of the forms, and then a general discussion of the various series of specimens that have been obtained. It ends with an account of the interrelationships of the various forms. Watson writes fully "On *Seymouria*, the most Primitive known Reptile" (*Proc. Zool. Soc.*, Pts. III and IV, March 1919). By means of further preparation of the type material in Munich and from material gathered by himself in Texas, the author has been able to make considerable addition to our knowledge of *Seymouria*. A full description of the skeleton is given, which together with what has been previously written makes the skeleton of this form almost as well known as that of any other reptile. The description is followed by a discussion of the relationship of the animal with the Temnospondyli and Reptilia. It is beyond doubt a form intermediate between Amphibia and Reptilia, and the author concludes: "I hope this study of *Seymouria* will be regarded as placing beyond dispute the origin of the reptiles from the Embolomerous Labyrinthodonts."

Other papers include:

Petronievics, "Comparison between the Lower Jaws of the Cyhodont Reptiles *Gomphognathus* and *Cynognathus*" (*Proc. Zool. Soc.*, Pts. III and IV, March 1919); Taylor, "A Case of Hermaphroditism in a Lizard, *Lacerta viridis*" (*ibid.*); and O'Donoghue and Gowanlock, "Notes on the Caspian Tern (*Sterna caspia*) and the Parasitic Jaeger (*Stercorarius parasiticus*) in Manitoba" (*Canadian Field Nat.*, vol. xxxiii, April 1919).

Hollister has published "East African Mammals in the United States National Museum: Pt. II, Rodentia, Lagomorpha, and Tubulidentata" (*Bull.* 99, United States Natural Museum, 1919). In this 194 species of the three orders enumerated in the title are dealt with. A number of specimens of most species were collected mainly by the expeditions in charge of Theodore Roosevelt and Paul Rainey, and the localities from which they were obtained and various measurements of each are given. Van der Stricht deals with "The Development of the Pillar Cells, Tunnel Space, and Nuël's Spaces in the Organ of Corti" (*Jour. Comp. Neur.*, vol. xxx, April 1919). The tunnel space develops as an intercellular cleft around the spiral nerve bundle, and its fluid contents are elaborated by the vacuolated pillar cells, partly as a result of cytolysis. The first space of Nuël similarly originates as an intercellular cleft between the outer pillars and outer hair cells. The fluid of all the spaces of Nuël is separated from the endolymph of the cochlear duct by the thin roofs of the interspaces. The structure appears to promote the propagation of vibratory waves from the basilar membrane to the membrana tectoria contained in the cochlear canal. Other papers include:

Allen, "Degeneration in the Albino Rat Testis due to a Diet Deficient in the Water-soluble Vitamine, with a Comparison of Similar Rats Differently Treated, and a Consideration of the Sertoli Tissue" (*Anat. Rec.*, vol. i, April 1919); Bate, "On a New Genus of Extinct Muscardine Rodents from the Balearic Islands" (*Proc. Zool. Soc.*, Pts. III and IV, March 1919); Carey, "Teratological Studies: A, On a Phocomelus, with Especial Reference to the Extremities; B, The External Form of an Abnormal Human Embryo of Twenty-three Days; C, The Anomalies of an Encephalic Monster, complete Craniorrhachischisis; and D, A Second Anencephalic Monster, complete Craniorrhachischisis" (*Anat. Rec.*, vol. xvi, April 1919); Donaldson, "The Relative Volumes of the Cortex and Medulla of the Adrenal Gland in the Albino Rat" (*Amer. Jour. Anat.*, vol. xxv, May 1919); Flather, "The Blood-supply of the Areas of Langerhans, a Comparative Study from the Pancreas of Vertebrates (Preliminary Paper)" (*Anat. Rec.*, vol. xvi, April 1919); Hanson, "The Coracoid of *Sus scrofa*" (*ibid.*, May 1919); Hay, "Descriptions of some Mammalian and Fish Remains from Florida of probably Pleistocene Age" (*Proc. United States Nat. Mus.*, vol. lvi, 1919); Hunt, "Vascular Abnormalities in a Domestic Cat (*Felis domestica*)" (*Anat. Rec.*, vol. xvi, April 1919); Jackson, "The Postnatal Development of the Suprarenal Gland, and the Effects of Inanition upon its Growth and Structure in the Albino Rat" (*Amer. Jour. Anat.*, vol. xxv, May 1919); Johnson, "The Development of the Lobule of the Pig's Liver" (*ibid.*); Jordan, "Studies on Striped Muscle Structure: IV, Intercalated Discs in Voluntary Striped Muscle" (*Anat. Rec.*, vol. xvi, 1919); Loder, "Notes on the Beavers at Leonardslee" (*Proc. Zool. Soc.*, Pts. III and IV, March 1919); Marui, "The Effect of Overactivity on the Morphological Structure of the Synapse (*Jour. Comp. Neur.*, vol. xx, April 1919); Meyer, "Uterine, Tubal, and Ovarian Lysis, and Resorption of Conceptuses" (*Biol. Bull.*, vol. xxxvi, April 1919), and "On the Occurrence and Identity of the Plasma Cells of Hofbauer" (*Jour. Morph.*, vol. xxxii, June 1919); Wilder, "An Anomaly in the

Portal Circulation of the Cat" (*Anat. Rec.*, vol. xvi, April 1919); and Witte, "Histogenesis of the Heart Muscle of the Pig in Relation to the Appearance and Development of the Intercalated Discs" (*Amer. Jour. Anat.*, vol. xxv, May 1919).

General.—More has a paper "On the Physiological Properties of the Gonads as Controllers of Somatic and Psychical Characteristics" (*Jour. Exp. Zool.*, vol. xxviii, May 1919). Gonads were removed from young rats and transplanted into other animals of the opposite sex from which the gonads had been previously removed. It is claimed that, while it is difficult to find any somatic modification resulting from this interchange, the alteration in behaviour is quite marked. Young females are, in as far as behaviour is concerned, transformed into typical males and vice versa. In "Precipitation Structures simulating Organic Growths: II, A Contribution to the Physicochemical Analysis of Growth and Heredity" (*Biol. Bull.*, vol. xxxvi, April 1919), Lillie and Johnston have continued work an account of which was published in 1917. The first part consists of a discussion of the theoretical aspect of such phenomena, in which it is suggested that the specific characters of organisms are determined by the specific nature of the structural compounds of protoplasm, or, in general, specific compounds give rise to structures of a correspondingly specific type when deposited from solution in solid form. Development from a germ is constant and specific in character because of the constancy in the physicochemical constitution of the growing system and its surroundings. Part II deals with the experimental side, and describes the forms produced, the electrical, mechanical, osmotic and temperature conditions under which they were produced, and so on.

Other papers include:

Goodale and Macmullen, "The Bearing of Ratios on Theories of the Inheritance of Winter Egg Production" (*Jour. Exp. Zool.*, vol. xxviii, April 1919); Hanson, "On Teaching the Germ Layers" (*Anat. Rec.*, vol. xvi, May 1919); Kirkham, "The Fate of Homozygous Yellow Mice" (*Jour. Exp. Zool.*, vol. xxviii, May 1919); and Roberts, "The Function of Pathological States in Evolution" (*Proc. Zool. Soc.*, Pts. III and IV, March 1919).

ANTHROPOLOGY. By A. G. Thacker, A.R.C.Sc., Public Museum, Gloucester.

Whilst there is an immense literature dealing with the question of the antiquity of man in Europe, there is now also a very considerable literature on the problem of man's origins in America. It will be remembered that the surviving species of man appeared first in Europe in the Aurignacian age, and that this age is most probably to be assigned to the Third (or

Riss-Wurm) Interglacial period. As is well known, the presence of the living species in Europe in Pre-Aurignacian times is credited by many, but is still very doubtful. On the other hand, I think the weight of the evidence is against the other extreme opinion—the ultra-conservative view—that the Aurignacian itself is to be given a post-Wurmian date. There is, however, one point of fundamental importance, which should have been obvious to all from the first, but which has nevertheless been sometimes overlooked. When *Homo sapiens* first appears in Europe he is already thoroughly *Homo sapiens*. We may trace man back into Neolithic times, into the last of the greater ice-ages, and, further yet, into the wonderful warm period which preceded that ice-age, and we find him anatomically what he is now. He shows no sign of merging backwards into another species, another kind of being. We therefore learn nothing of the origin of *Homo sapiens*. It has thus always been obvious that the species either (1) sprang into existence suddenly, or very rapidly (an hypothesis which is less improbable than might have appeared to us ten years ago), or (2) possesses a far greater antiquity elsewhere than in Europe. It is at this point that we turn eagerly to the evidences from America. The supposed proofs brought forward have been looked at askance because they appear to prove too much. The supposed relics of Pleistocene man in America reveal to us a race of barbarians almost identical, not only anatomically but culturally, with the more primitive Red Indians as they were when they were discovered by Europeans. Now, even the more primitive Red Indians were (except in respect of the artistic faculties) in advance of our European races of the later Pleistocene. They are more comparable, culturally, with the very earliest of our Neolithic peoples. Moreover, the most credible of the American evidences carry back these Red Indians (for they were nothing else) much further than the date of the European Aurignacian —as far back as the first of the three great inter-glacial periods. In other words, the supposed proofs give an early Pleistocene antiquity to a primitive Neolithic culture. Now, this is a theory which any anthropologist will be shy of accepting. But we have no right absolutely to rule out the hypothesis on the basis of *a priori* improbability. If the species originated in Asia in early Pleistocene times, the first nomadic tribes would have found America quite as accessible as Europe, for there was then a land-connexion between Asia and America. In early and middle Pleistocene times Europe may well have been merely a "backwater," where the older and much more primitive species of the humanoid family took refuge from the ever-spreading tribes of the abler *H. sapiens*.

The reader will find these matters ably discussed in the recent issue of the *American Anthropologist* (vol. xx. 1918). There is in this volume (p. 1) a paper by Dr. Oliver P. Hay, of the Carnegie Institution, entitled "Further Consideration of the Occurrence of Human Remains in the Pleistocene Deposits at Vero, Florida." These human remains at Vero occur in a stratum which appears to belong to the first inter-glacial epoch. The whole matter is argued at length by Dr. Hay, and he seeks to dispose of the various methods of explaining away this and similar finds. He relies largely on an argument of a general character, which he sums up as follows: "In Pleistocene deposits laid down under conditions which permitted the existence of man, we find numerous evidences of his presence. In Pleistocene deposits originating under conditions forbidding the presence of man, and in older accumulations, no traces of man's existence are met with, even though both those of Pleistocene age and the older ones are adapted to receive adventitious inclusions of artifacts." I have not mentioned in this connexion the relatively few discoveries of remains of the other species of the Hominidæ, because I am fairly confident that they have no direct bearing upon the problem.

There is another paper in the same volume of the *American Anthropologist* which is also of special interest. This is by W. H. Babcock and is entitled "Certain Pre-Columbian Notices of the Inhabitants of the Atlantic Islands." In regard to Iceland the author mentions the reference to trolls in the Saga of Grettir; but he concludes that there was probably no foundation, in fact, for this legendary story of an aboriginal race in Iceland. In this instance, the trolls were doubtless imaginary beings. It may be added that so much is known about early Iceland, that, if Eskimo or any other race of aborigines had existed in the island when the Norsemen discovered it, definite records, placing the matter beyond all possibility of doubt, would almost certainly have been left for us to peruse. The Icelanders, as all the world knows, were a people of marvellous literary gifts. Babcock also deals with the stories of the phantom Atlantis, and with the much later records of European voyages to Madeira, the Azores, and the Canaries. The early descriptions of the inhabitants of the Canaries, of which Babcock gives extracts, make interesting reading. The people were apparently of Berber stock. No aborigines are known to have existed in Madeira or the Azores.

Most of the remaining papers in this volume are scarcely up to the usual standard of the *American Anthropologist*. W. H. Babcock has another paper (p. 406) entitled, "Some Ethnological and National Factors of the War," but his remarks on this subject are somewhat trite. And in places his

sketch of European ethnology is so superficial that it may even be misleading to his American readers, who are for the most part profoundly ignorant of Europe. Mention may also be made of an article by W. D. Wallis under the heading, "Indo-Germanic Relationship Terms as Historical Evidence."

As is well known, in the north of the Japanese Islands, in Saghalien, and in the Kurile Islands, there dwells a very hirsute race of Caucasic character known as the Ainu. The reader's attention must be drawn to an exhaustive monograph on the Ainu of the Kurile Islands. This large monograph is by Dr. R. Tosii, and it constitutes an issue of the *Journal of the College of Science* (University of Tokyo), (vol. xlii. Art. 1, January 1919). The article is in the French language, and is entitled: "Études Archéologiques et Ethnologiques. Les Ainou des Isles Kouriles." The monograph is well illustrated, and, judging from the plates, some of the Ainu are so entirely Caucasic in appearance that they might almost pass as Europeans. How a Caucasic people comes to occupy such an anomalous geographical position is one of the standing problems of anthropology.

The following papers on Social Anthropology may be recorded:

In *Folk-lore*, vol. xxx, No. 2: "Customs Connected with Death and Burial among the Rumanians," by Mrs. A. Murgoci; "The Problem of the Gipsies," by Dr. F. W. Russell; and "The Folk-lore of the Bushmen" (this latter is a reprint of an important article which appeared in the *Cape Monthly* magazine in 1874).

In *Man*: "The Devil's Officers and the Witches' Covens," by Miss M. A. Murray (September); and "Correspondence on 'Anthropology and our Older Histories'," by Fleure, De Guérin, and others (September).

In the *American Anthropologist*, vol. xx: "On Computations for the Maya Calendar," by R. K. Morley; "Early Cheyenne Villages," by G. B. Ginnell; "War-God Shrines of Laguna and Zuni," by Elsie Clews Parsons; and "Kinship Terms of the Kootenay Indians," by E. Sapir.

And the following articles may be selected from the new volume of the *Proceedings of the Prehistoric Society of East Anglia*, vol. iii. Pt. 1, 1918–19, received as we go to press: "Foreign Relations in the Neolithic Period," by Reginald A. Smith; "Excavations at Grime's Graves during 1917," by A. E. Peake; and "The Origin of the Rostro-Carinate Implements, and other Chipped Flints from the Basement Beds of East Anglia," by F. N. Haward.

ARTICLES

EVOLUTION AND IRREVERSIBILITY

By ALFRED J. LOTKA, M.A., D.Sc.

The recent publication in Science Progress of Dr. Petronievics's article, "Sur la loi de l'évolution irréversible," [1] seems to furnish an opportune occasion for a discussion of the relation between irreversibility as understood by the physicist, and evolution as conceived more particularly by the biologist.

Biology leaves us with a not very clearly defined idea of *progression* as one of the fundamental characteristics of those changes which are embraced by the term "evolution." Such phrases as "the passage from lower to higher forms," which are often employed to describe evolution, are vague, and undoubtedly contain an anthropomorphic element.[2] The fact seems to be that all we can predicate positively to-day is that evolution is a "unidirectional" change in time.

Now, the physicist is well acquainted with unidirectional changes. They are precisely those changes which he terms "irreversible." And, for certain cases at least, he is able to define in precise terms the direction of evolution. For example, in an isolated physical system evolution proceeds in such direction that the entropy of the system increases. More generally, if the state of a physical system is defined in terms of certain variables $X_1, X_2 \ldots X_n$ and certain parameters $P_1, P_2 \ldots P_m$, and if evolution is allowed to proceed (*i.e.*, if the X's are allowed to change by a spontaneous process) while keeping the parameters P constant, then certain definite and known functions of the variables X and of the parameters

[1] January 1919, p. 406.

[2] "Evolution is thus almost synonymous with progress, though the latter term is usually confined to processes of development in the moral as distinguished from the physical world. Further, this idea, as Mr. Spencer remarks, has rather a subjective than an objective source, since it points to an *increased value* in existence, *as judged by our feelings*" (*Encyc. Brit.*, 9th edition, vol. viii, pp. 751-2).

P increase toward a maximum, while certain other such functions decrease toward a minimum.

When we pass on to biological systems evolving under the complex conditions presented to us in nature, we may, if we choose, apply the same language in a discussion of their history, and state empirically that their evolution is an irreversible process,[1] or make the guess that here also certain functions increase toward a maximum.[2] But no attempt seems to have been made to give explicitly the form of these functions, or even as much as to indicate just what are the

[1] "The second law (of thermodynamics) is the law of evolution of the world accessible to our observation" (Chwolson, *Lehrbuch der Physik*, 1905, vol. iii, p. 499; *Scientia*, 1910, vol. iii, p. 51.

". . . the second law of the theory of energy is now generally regarded as essentially a statistical law. So viewed, the second law of energy becomes a principle stated wholly in terms of the theory of probability. It is the law that the physical world tends, in each of its parts, to pass from certain less probable to certain more probable configurations of its moving particles. As thus stated the second principle . . . becomes a law of evolution" (Josiah Royce, *Science*, 1914, vol. xxxix, p. 551).

"Un système isolé ne passe jamais deux fois par le même état.

"Le second principe affirme un ordre nécéssaire dans la succession de deux phénomènes, sans retour possible aux états déja traversés.

"C'est pourquoi j'ai cru expressif d'appeler ce principe un principe d'évolution.

"Il se trouve qu'en proposant ce nom je suis fidèle a la pensée de Clausius, car le mot ἐντροπή, d'où il a tiré entropie, signifie précisément évolution" (J. Perrin, *Traité de Chimie Physique*, 1903, vol. i, pp. 142-43).

Compare also:

"Il est hautement improbable qu'un système isolé passe deux fois par le même état; cela est d'autant plus improbable que la complication du système est plus grande, et pratiquement il serait insensé de se placer dans cette hypothèse d'un retour a l'état initial" (J. Perrin, *Traité de Chimie Physique*, 1903, vol. i, pp. 145-46).

The following is also of interest:

"The book of Nature is the book of Fate. She turns the gigantic pages—leaf after leaf, never re-turning one. . . . The face of the planet cools and dries, the races meliorate, and man is born. But when a race has lived its term, it comes no more again" (Emerson, *The Conduct of Life*).

The mere statement that a system "never twice passes through the same state" is, in itself, insufficient to distinguish between evolution as a "progress," or merely a "changeful sequence," to use the apt phrase of Prof. J. A. Thomson in *The Wonder of Life*, 1914. It is insufficient to define the direction of evolution. This direction is indicated by the more definite statement that the system passes from less probable to more probable states. A rigorous proof of this latter principle seems to be given only for certain special kinds of systems.

[2] "Un corps vivant qui reste vivant, suit toujours, quel que soit le dérangement qu'il a subi, la loi qui défini l'équilibre stable. Il est donc en état d'équilibre stable; la stabilité de cet équilibre résulte de ce qu'une certaine fonction de ces éléments est à un maximum" (Le Dantec, *La Stabilité de la Vie*, 1910, p. 25).

variables and the parameters appearing in them.[1] Until this is done the mere statement that such functions exist is of little value, except, perhaps, as pointing a certain direction for inquiry. Needless to say, the statement that such functions, relative to a system in the course of organic evolution, exist, lacks entirely the rigorous basis on which rest the corresponding propositions regarding the evolution of physical systems under certain clearly defined conditions.

It seems, therefore, worth noting that a process of organic evolution, when viewed or comprehended from a certain standpoint, falls naturally within a definite category or class comprising also that group of physical phenomena termed "change of state."[2] The fit is so complete that the same general method of analytical treatment can be applied, without distinction, to the entire category; and certain general conclusions relating equally to "change of state," as ordinarily understood, and to certain phenomena in an evolving organic system, follow in natural course.

This viewpoint is gained by regarding the evolution of the system under consideration as the redistribution of matter among the components of the evolving system.

In the case of an ordinary physico-chemical system, for example, the system may consist of the three components[3] H_2, O_2, H_2O, and in the course of the evolution of the system, matter redistributes itself among these components in accordance with a certain system of differential equations:

$$\left.\begin{aligned}\frac{dX_1}{dt} &= F_1(X_1,\ X_2,\ X_3;\ p,\ T)\\ \frac{dX_2}{dt} &= F_2(X_1,\ X_2,\ X_3;\ p,\ T)\\ \frac{dX_3}{dt} &= F_3(X_1,\ X_2,\ X_3;\ p,\ T)\end{aligned}\right\} \qquad (1)$$

[1] Similar remarks apply to various attempts to apply to biological systems the Principle of Le Chatelier. Inasmuch as this principle itself derives its validity from the second law of thermodynamics, its application can be regarded as established only for those systems and for those variables and parameters which we are competent to-day to treat by thermodynamical methods. The principle is quite meaningless unless the particular variables to which it is applied are clearly stated. Even then, and in the case of simple physical systems, the application of the principle is very liable to errors, of which the literature gives ample illustration. (See Ehrenfest, *Zeitschr. für Phys. Chem.*, 1911, vol. lxxvii, p. 735; Wolchonsky, *Jour. Russian Phys. Chem. Soc.*, 1912, vol. xliv, pp. 305, 310; Chwolson, *Lehrbuch der Phys.*, 1909, vol. iii, p. 547; Bancroft, *Jour. Amer. Chem. Soc.*, 1911, p. 92; Fournier d'Albe, *Contemporary Chemistry*, 1911, p. 38; Löwy, *Kosmos*, 1911, p. 331; Le Dantec, *La Stabilité de la Vie*, 1910, p. 25; Spencer, *First Principles*, chap. xxii, section 173, Burt's Edition, p. 433).

[2] This term is here used in a general sense, to include also change of state by chemical reaction.

[3] Not necessarily independent.

where X_1 = mass of H_2; X_2 = mass of O_2; X_3 = mass of H_2O; p = pressure; T = temperature; t = time.

Among reactions ordinarily considered, the form of the functions F is commonly given by the law of mass action.

In the case of organic evolution, we have a system comprising a number of different species of organisms, and also other components. We may denote the mass of these several species and other components, $S_1, S_2, \ldots S_n$ by $X_1, X_2 \ldots X_n$. The state of the system may be thought of as defined by statement of the values of these variables X; and, further, of certain parameters, $P_1, P_2 \ldots P_l$, which measure climatic conditions, extension-in-area, etc., of the system; lastly, of certain other parameters, $Q_1, Q_2 \ldots Q_k$, which define the character of each species, this character being, in general, variable with time as evolution proceeds.

It is reasonable to make the very broad supposition that the progress of redistribution of matter (evolution) in this system takes place in accordance with a system of differential equations:

$$\left.\begin{aligned} \frac{dX_1}{dt} &= F_1(X_1, X_2, \ldots X_n;\ P_1, P_2, \ldots P_l;\ Q_1, Q_2, \ldots Q_k) \\ \frac{dX_2}{dt} &= F_2(X_1, X_2, \ldots X_n;\ P_1, P_2, \ldots P_l;\ Q_1, Q_2, \ldots Q_k) \\ & \\ \frac{dX_n}{dt} &= F_n(X_1, X_2, \ldots X_n;\ P_1, P_2, \ldots P_l;\ Q_1, Q_2, \ldots Q_k) \end{aligned}\right\} \quad (2)$$

which is merely a more general form of the system (1).

The rôle played in evolution by the changes in the parameters Q—*i.e.*, the changes in the character of the evolving species of organisms—is of such absorbing interest that in common conception this alone has almost come to be regarded as evolution.

In a systematic treatment of the subject from the point of view here set forth, however, it will be seen that this matter of the change of the parameters Q forms only one special subsidiary problem within the broader general problem. Furthermore, this subsidiary problem repeats, within its own borders, the general character of the broader problem.

This becomes apparent when we define the "character" of a species in a statistical way. Such definition takes the following form:

Let the total mass of the species S_i be X_i.

Let $p_1, p_2 \ldots p_r$ be parameters, measuring certain characteristics (*e.g.*, stature, mass, etc.) of individual units (organisms) of the species.

Let $c_i(\omega)$ be such a factor that

$$X_i\, c_i(\omega)\Delta\omega = X_i\, c_i(p_1, p_2, \ldots)\Delta p_1, \Delta p_2 \ldots \Delta p_r \qquad (3)$$

gives the mass of that portion of the species for which the parameters $p_1, p_2 \ldots p_r$ are comprised within the limits

$$\left.\begin{array}{l} p_1 \text{ and } p_1 + \Delta p_1 \\ p_2 \text{ and } p_2 + \Delta p_2 \\ \cdots\cdots\cdots\cdots \\ p_r \text{ and } p_r + \Delta p_r \end{array}\right\} \qquad (4)$$

The increments Δp may be infinitesimals if the conditions of the case commend such choice.

The character of the species being thus defined, it is evident that evolutionary change in character of the species is only one aspect of the redistribution of matter in the system comprising that species—it is that particular phase of the redistribution which occurs within the confines of the species itself. We may speak of this phase or part of organic evolution as intra-species or intra-group evolution.

In contradistinction with this we may speak of the phase of evolution to which the system of equations (2) refers as inter-species or inter-group evolution—*i.e.*, the redistribution of matter among the several species of matter of which the system is composed.

The relations which connect the several factors of intra-group evolution are evidently more complex than those which exist between the factors of inter-group evolution.

It is therefore expedient to consider the latter first.

We are thus led, primarily, to a consideration of the system of differential equations (2).

In physico-chemical dynamics it is customary to study processes taking place at constant pressure and temperature, or at constant volume and temperature.

We shall here introduce a similar restriction, and consider the case of inter-group evolution taking place at constant parameters P (*e.g.*, constant extension-in-area and constant climatic conditions).

Furthermore, a simple case which naturally invites first attention is that in which the changes in the parameters Q (in the character Q of the several species) take place very slowly as compared with the changes in the variables X—*i.e.*, as compared with the changes in the distribution of matter among the several species.

In this case we may, as a first approximation, regard the Q's also as constant.

For the purposes of our present discussion we may, then, write:

$$\left.\begin{aligned}\frac{dX_1}{dt} &= F_1(X_1,\ X_2,\ \ldots\ X_n)\\ \frac{dX_2}{dt} &= F_2(X_1,\ X_2,\ \ldots\ X_n)\\ &\cdots\cdots\cdots\cdots\\ \frac{dX_n}{dt} &= F_n(X_1,\ X_2,\ \ldots\ X_n)\end{aligned}\right\} \tag{5}$$

We define $C_1,\ C_2,\ \ldots\ C_n$ by the relation

$$F_1(C_1,\ C_2,\ \ldots\ C_n) = F_2(C_1,\ C_2,\ \ldots\ C_n) = \ldots = F_n(C_1,\ C_2,\ \ldots\ C_n) = 0 \tag{6}$$

and introduce new variables

$$x_i = X_i - C_i \tag{7}$$

so that

$$\frac{dx_i}{dt} = f_i(x_1,\ x_2,\ \ldots\ x_n) \tag{8}$$

We shall assume that the functions f are analytic, and write

$$\begin{aligned}\frac{dx_i}{dt} = a_{i1}x_1 + a_{i2}x_2 + \ldots + a_{in}x_n\\ + a_{i11}x_1^2 + a_{i22}x_2^2 + \ldots\end{aligned} \tag{9}$$

Omitting the discussion of special cases,[1] a general solution of the system (9) is,

$$x_i = a_{i1}e^{\lambda_1 t} + a_{i2}e^{\lambda_2 t} + \ldots + a_{i11}e^{2\lambda_1 t} + a_{i12}e^{(\lambda_1+\lambda_2)t} + \ldots \tag{10}$$

where the exponential coefficients $\lambda_1,\ \lambda_2,\ \ldots\ \lambda_n$ are the n roots for λ of the equation

$$\Delta(\lambda) = \begin{vmatrix} a_{11}-\lambda & a_{12} \ldots & a_{1n}\\ a_{21} & a_{22}-\lambda \ldots & a_{2n}\\ \cdots & \cdots & \cdots\\ a_{n1} & a_{n2} \ldots & a_{nn}-\lambda\end{vmatrix} = 0 \tag{11}$$

and the coefficients a are determined in well-known manner.[2]

Marks of Irreversibility.—The question now arises, Where in our equations and in their solution do we find the earmarks of irreversibility?

[1] Such as the case when the determinantal equation (11) has multiple roots. This offers no special difficulties, but is here omitted for the sake of brevity. See, for example, Picard, *Traité d'Analyse*, vol. iii, p. 30.

[2] See, for example, Webster's *Dynamics*, 1912, pp. 159, 161.

Considering, first of all, the system of equations (9) we see directly that the velocities $\frac{dx}{dt}$ vanish with the co-ordinates x.

Again, by differentiating (9) repeatedly, it is seen that the second and all the higher derivatives also vanish together with the first (and with the co-ordinates themselves).

This vanishing of the accelerations with the velocities is, of course, typical of an "inertia-free" or "completely damped" system.[1]

On the other hand, the solution (10) appears at first sight very similar in form to that obtained in the treatment of small oscillations according to Lagrange. Such small oscillations are not in general of the completely damped (inertia-free) type; they may not be damped at all, in which case they represent a reversible process.

There is, however, one essential difference which distinguishes the solution (10) here obtained from the solution of the Lagrangian problem. In the latter, with a system requiring for its definition n co-ordinates, there appear in the solution $2n$ arbitrary constants; on the other hand, in the case here considered, with n co-ordinates, there appear only n such constants in the solution. This fact is closely related to the fact that in a system possessing inertia it is, in general, necessary, in order to specify the state of the system, to give the values, not only of the co-ordinates, but of their first derivatives (velocities) also.

On the other hand, in an inertia-free or completely damped system, such as we have here been considering, after the co-ordinates are fixed, the velocities are no longer arbitrary, but are fully determined. This fact finds expression *ab initio* in equation (2) above, in which it is understood that the parameters P, Q do not include initial values of the first derivations of the X's.

Equilibrium defined by Minimum Condition.—In mechanics and thermodynamics the condition for equilibrium commonly takes the form that certain functions (potentials) of the variables defining the state of the system assume a minimum value.

As already remarked, the phraseology thus borrowed from mechanics and thermodynamics has been applied to biological systems, though no attempt has been made to give such expressions the analytical form without which they seem valueless or even meaningless.

As our reflections here have been conducted along analytical lines, the question arises whether they enable us to

[1] See Buckingham, *Thermodynamics*, 1900, p. 33.

formulate the condition for equilibrium in precise terms and in form of a minimum or maximum condition.

We note, first of all, that the condition for equilibrium is given by (6) in the form

$$F_1 = F_2 = \ldots = F_n = 0 \tag{6}$$

i.e., the equilibrium values of the n variables $X_1, X_2 \ldots X_n$ are defined by the vanishing of n given functions of these variables.

But evidently the same result can be achieved by stating that some arbitrary function Φ $(x_1, x_2, \ldots x_n)$ has a minimum or a maximum when $x_1, x_2, \ldots x_n$ have their equilibrium values; for then we have, for these values,

$$\frac{d\Phi}{dx_1} = \frac{d\Phi}{dx_2} = \ldots = \frac{d\Phi}{dx_n} = 0 \tag{12}$$

i.e., as before, the equilibrium values of the variables x are determined by the vanishing of n functions of these variables.

We can go a step further and select the function Φ so that it will have a maximum at the origin if, and only if, the equilibrium at the origin is stable. This will, for example, be the case if

$$\Phi = \mu_1 x_1^2 + \mu_2 x_2^2 + \ldots + \mu_n x_n^2 \tag{13}$$

where $\mu_1, \mu_2, \ldots \mu_n$ are the real parts of the roots λ of the equation (11).

It can be seen by inspection[1] of the solution (10) that a necessary and sufficient condition for stability at the origin is that the real parts of all the roots λ shall be negative—*i.e.*, that the quadratic form Φ shall be definite and negative, so that Φ has a maximum at the origin.

As an illustration we may consider an example taken from Sir Ronald Ross's Quantitative Studies in Epidemiology[2] —namely, the equations representing the history (evolution) of a system comprising the three species: man—anopheles mosquito—malaria parasite.

Here Sir Ronald Ross develops the system of differential equations

$$\left.\begin{aligned} \frac{dz}{dt} &= k'z'(p - z) + qz = F_1(z, z') \\ \frac{dz'}{dt} &= kz(p' - z') + q'z' = F_2(z, z') \end{aligned}\right\} \tag{14}$$

[1] For a rigorous discussion of the conditions of Stability, see Poincaré, *Jour. Mathém.*, ser. 4, vol. ii, chap. xvii; also *Encyc. des Sci. Math.*, vol. ii, pt. 3, fasc. 1

[2] *Nature*, October 5, 1911, p. 466; February 8, 1912, p 497.

Following the procedure set forth in dealing with the general case, we first put

$$F_1(z, z') = F_2(z, z') = 0 \tag{15}$$

One solution of (15) is

$$z = z' = 0 \tag{16}$$

It is not, however, this point of equilibrium in which we are interested, but the one given by the other solution of (15), namely

$$\left.\begin{aligned} z &= \frac{kk'pp' - qq'}{k(k'p' - q)} = C_1 \\ z' &= \frac{kk'pp' - qq'}{k'(kp - q')} = C_2 \end{aligned}\right\} \tag{17}$$

Accordingly we introduce as new variables

$$\begin{aligned} x_1 &= z - C_1 \\ x_2 &= z' - C_2 \end{aligned} \tag{18}$$

and obtain

$$\left.\begin{aligned} \frac{dx_1}{dt} &= (q - k'C_2)x_1 + k'(p - C_1)x_2 - k'x_1x_2 \\ \frac{dx_2}{dt} &= k(p' - C_2)x_1 + (q' - kC_1)x_2 - kx_1x_2 \end{aligned}\right\} \tag{19}$$

or, in the notation of our general discussion,

$$\left.\begin{aligned} \frac{dx_1}{dt} &= a_{11}x_1 + a_{12}x_2 + a_{112}x_1x_2 \\ \frac{dx_2}{dt} &= a_{21}x_1 + a_{22}x_2 + a_{212}x_1x_2 \end{aligned}\right\} \tag{20}$$

Assuming now, for the sake of illustration, that the constants in (19) are capable of assuming all kinds of values, we may distinguish the following cases :

1. Case in which the roots λ_1, λ_2, of (11) are real (and distinct).

The determinantal equation (11) here takes the form

$$\begin{vmatrix} a_{11} - \lambda & a_{12} \\ a_{21} & a_{22} - \lambda \end{vmatrix} = 0 \tag{21}$$

In the immediate neighbourhood of the origin the product terms in (20) are negligible.

By a well-known homogeneous linear transformation[1] we

$$\begin{aligned} \xi_1 &= a_1 x_1 + a_2 x_2 \\ \xi_2 &= \beta_1 x_1 + \beta_2 x_2 \end{aligned} \tag{22}$$

can transform (20), omitting the product terms, into

$$\left.\begin{aligned} \frac{d\xi_1}{dt} &= \lambda_1 \;{}_1 \\ \frac{d\xi_2}{dt} &= \lambda_2 \xi_2 \end{aligned}\right\} \tag{23}$$

where λ_1, λ_2 are the roots of (21).

From (23) it follows, firstly, that

$$\frac{d\xi_1}{d\xi_2} = \frac{\lambda_1}{\lambda_2} \cdot \frac{\xi_1}{\xi_2} \tag{24}$$

$$\xi_2 = K \xi_1^{\frac{\lambda_2}{\lambda_1}} \tag{25}$$

and, secondly, that

$$\left.\begin{aligned} \xi_1 \frac{d\xi_1}{dt} &= \lambda_1 \xi_1 \\ \xi_2 \frac{d\xi_2}{dt} &= \lambda_2 \xi_2^2 \end{aligned}\right\} \tag{26}$$

Hence, by addition,

$$\frac{d}{dt}(\xi_1^2 + \xi_2^2) = \frac{dR^2}{dt} = 2(\lambda_1 \xi_1^2 + \lambda_2 \xi^2) \tag{27}$$

where R is the radius vector of the point $\xi_1 \xi_2$ in a diagram in which these two variables are plotted as rectangular coordinates. We thus obtain a graphic interpretation of the quadratic form $\lambda_1 \xi_1^2 + \lambda_2 \xi_2^2$. The topography[2] of the integral curves (25) near the origin is indicated in fig. 1 for $\lambda_1 < 0$, $\lambda_2 < 0$ (stable equilibrium); in fig. 2 for $\lambda_2 > 0$, $\lambda_2 > 0$ (unstable equilibrium); and in fig. 3 for $\lambda_1 > 0$, $\lambda_1 < 0$ (unstable equilibrium). The arrow heads indicate the direction of travel along the integral curves according to (27).

2. *Case of Complex Roots.*—Here we write

$$\begin{aligned} \lambda_1 &= \mu + i\nu \\ \lambda_2 &= \mu - i\nu \end{aligned} \tag{28}$$

[1] See, for example, Liebmann, *Lehrbuch der Differentialgleichungen*, pp. 99, 131.

[2] Compare Liebmann, *loc. cit.*; also von Dyk, *Akad. Wiss. Munich Sitzungsber.*, 1909, Abh. 15; *Munich Abhandlungen*, 1914, vol. xxvi, Abh. 10; also Sharpe *Ann. of Math.*, ser. 2, vol. ii, 1910, p. 97.

A transformation[1] somewhat similar to (22) then leads to

$$\left.\begin{aligned}\frac{d\xi_1}{dt} &= \mu\xi_1 + \nu\xi_2 \\ \frac{d\xi_2}{dt} &= \mu\xi_1 - \nu\xi_2\end{aligned}\right\} \qquad (29)$$

and we have, in this case,

$$\frac{d\xi_1}{d\xi_2} = \frac{\mu\xi_1 + \nu\xi_2}{-\nu\xi_1 + \mu\xi_2} \qquad (30)$$

$$R = R_0\, e^{\frac{\mu}{\nu}\tan^{-1}\frac{\xi_1}{\xi_2}} \qquad (31)$$

and

$$\frac{d}{dt}(\xi_1^2 + \xi_2^2) = \frac{dR^2}{dt} = 2\mu R^2 \qquad (32)$$

$$R = R_0\, e^{\mu t} \qquad (33)$$

The course of these integral curves is shown in figs. 4 and 5.

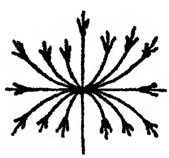

Fig. 1. Roots λ real and both < 0. Stability.

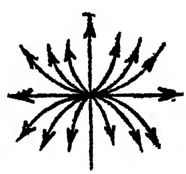

Fig. 2. Roots λ real and both > 0. Instability.

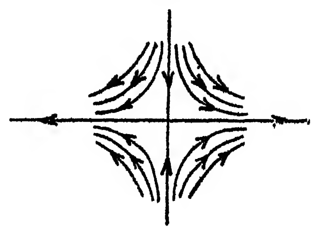

Fig. 3. Roots λ real and of opposite sign. Instability.

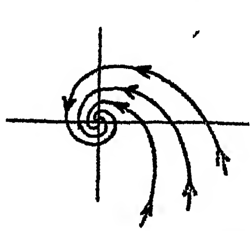

Fig. 4. Roots λ complex; $\mu < 0$. Stability.

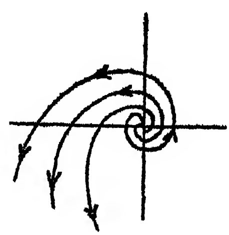

Fig. 5. Roots λ complex; $\mu > 0$. Instability.

Summary.—It has been pointed out by biologists that organic evolution is an irreversible process. Physicists also

[1] See, for example, Liebmann, *Lehrbuch der Differentialgleichungen*, pp. 99, 131.

have spoken of the second law of thermodynamics broadly as the law of evolution.

In inorganic physical systems irreversible processes are attended with a decrease in certain functions of the variables defining the state of the system. Stable equilibrium is characterised by the fact that these functions assume a minimum value.

In the case of organic systems we have not, in general, any such definite criteria for irreversibility or for equilibrium.

In the present contribution a broad formulation of evolution, organic or otherwise, is presented in analytical form. From this it is shown that, for certain cases, functions of the variables X and the parameters P defining the state of the system, and of the coefficients a defining its characteristic properties, can be indicated, which have the property, in the neighbourhood of stable equilibrium, of diminishing in the (irreversible) process of the evolution of the system, and of assuming a minimum when stable equilibrium is established.

In these cases, therefore, it is possible to define in exact terms the *direction* of evolution, whereas the descriptions ordinarily given of this direction (passage from lower to higher, from simpler to more complex forms, etc.) are vague or inaccurate.

By the way of example, the principles thus established are applied to the system of equations given by Sir Ronald Ross to represent the history (evolution) of a system comprising a human population, mosquitoes, and malaria parasites.

RHYTHM IN NATURE

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That so many phenomena in Nature should be of a recurrent or periodic type is not surprising when we consider that the earth on which we live forms part of a rhythmic universe. From the biological point of view, however—and it is with the biological aspects of rhythm that the writer is concerned—these phenomena, though possibly all of the same nature fundamentally, must, for practical purposes, be regarded as of two distinct kinds.

We have, in the first place, the numerous cases of periodic behaviour in response to, or imposed by, external factors of a cosmic nature, such as the alternation of night and day, the regular and ever-recurring sequence of the seasons, the ebb and flow of the tides, and so on. Here the mainspring of the rhythm is obviously external. In contrast to this, however, are the cases of the second type in which the rhythm is not dependent on the environment, but is of an intrinsic or vital nature, and corresponds to something inherent in the organism.

The periodicities of this latter type may all be regarded as identical except as regards the time factor. They are of the nature of *age-cycles*.

In temperate countries, where the difference between the seasons is very marked, the periodical phenomena of plants and animals take place at about the same time in all species. With us, in consequence, the seasons have come to typify the chief stages in the life-cycle of plants and animals. Notwithstanding this, the phenomenon of an age-cycle must be regarded as something intrinsic in the organism and not merely as the result of environment, the synchronising of the stages of the age-cycle with the seasons being of a secondary nature. " In Europe," says Bates,[1] " a woodland scene has its spring, its summer, its autumnal aspects. In the equatorial forests the aspect is the same or nearly so every day in the year : budding, flowering, fruiting, and leaf-shedding are always going on in one species or other. The activity of birds and

[1] Bates, H. W., *The Naturalist on the Amazons.*

insects proceeds without interruption, each species having its own separate time; the colonies of wasps, for instance, do not die off annually, leaving only the queens, as in cold climates; but the succession of generations and colonies goes on incessantly. It is never either spring, summer, or autumn, but each day is a combination of all three."

The age-cycle, with its sequence of birth, growth, maturity, decay, and death, is the most typical and most inevitable phenomenon in Nature. It is not only the normal sequence in the organism as a whole, but the phenomena of senescence and rejuvenescence are continually being repeated *within* the organism. These internal periodicities are of essentially the same character as the age-cycle of the organism, except as regards the time factor. "In cells," says Child,[1] "where function is accompanied by extensive accumulation and discharge of substances, such, for example, as the gland cells, storage cells, etc., the cycles of activity and morphological change are essentially age-cycles—that is to say, the period of loading of the cell is a period of decreasing metabolic activity, of senescence, and the period of discharge one of increasing activity, of rejuvenescence, which makes possible a repetition of the cycle." In the pancreas of the toad, for example, the cells, when ready to secrete, are loaded with granules, and in this condition are only very slightly active metabolically. As the cell secretion is discharged, the granules gradually disappear to a point when they are practically absent. In this condition, the cell is again capable of a high rate of metabolic activity; if nutrition is present, the process of loading occurs once more. This cycle of changes, which may occur within a few hours, and which may be repeated within a single cell, Child believes, is not fundamentally different from the age-cycle of organisms. It exhibits all the essential features, up to a certain point, of senescence and rejuvenescence. The cell undergoes changes similar to those of the age-cycle, though their period is short. At the same time, as Child says, the gland cell may be undergoing senescence in the stricter sense —that is to say, changes in the more stable framework of the protoplasm may be occurring which are not wholly compensated by the functional cycle.

According to Benjamin Moore,[2] the living cell may be regarded, from the physico-chemical point of view, as a peculiar energy transformer: chemical energy in the living cell being converted by the colloidal structure into biotic energy,

[1] Child, C. M., *Senescence and Rejuvenescence.* (University of Chicago Press, 1915.)

[2] Benjamin Moore, *The Origin and Nature of Life.* (London: Williams and Norgate, 1912.)

this latter being convertible into mechanical energy, electrical energy, heat energy, or chemical energy. Like all energy transformers, living cells have their phasic periods or revolution time in which they pass through a cycle or oscillation, the period varying from one type of cell to another.

The contraction of the heart is an example of a vital rhythm which can escape nobody. The series of movements taking place in the heart during one complete beat constitutes what is known as a "cardiac cycle." This, again, is essentially an age-cycle, although the rapid recovery after fatigue tends to obscure its real nature. Disregarding the immediate causes underlying the repetition of the cycle, the point to notice is that rhythm is associated with efficiency. The rhythmic method represents the best means of accomplishing a purpose, and may, in fact, be regarded as an evolutionary goal towards which all life processes are tending. To take a concrete example: It is certain that the circulation in vertebrates is both a more orderly and a more efficient process than the flow of blood in an animal, let us say, like an earthworm. The correctness of this view of coupling rhythm with efficiency is supported by the phenomenon of the disordered and arhythmical action of the heart and lungs in disease. The loud "bourdon" of the engines at a power-station conveys a most distinctly rhythmic impression, and a practised ear can readily detect the smallest of functional troubles by the modification of the normal rhythmic note.

We now turn to cases of periodicity of the external type.

It is well known that not a few diseases, particularly those like malaria, produce markedly periodic symptoms. It is an interesting fact, however, that not only does the malarial parasite itself show regular development cycles within the organism, but the mosquito, the carrier of the parasite, is markedly periodic in its habits. Some interesting observations have been made on the flight of the mosquito in the course of the construction of the Panama Canal.[1] Gatun, about seven miles south of Colon, is one of the largest settlements in the Canal zone. Between January and March 1913, more mosquitoes were found there than in any settlement since the beginning of work on the Canal. The weekly catch of *Anopheles* was from 7,000 to 20,000. Just to the south of Gatun is a lake which seemed a likely breeding-ground for mosquitoes, but it was soon found that they did not have their origin here. To the west of Gatun, across a part of the old French canal, there was some flat land into which sea-water and mud from the American canal were being pumped. This land was so located

[1] Le Prince, J. A. and Orenstein, A. J., *Mosquito Control in Panama*. (London: Putnam, 1916.)

that, to reach the settlement, adult mosquitoes would have to fly from half a mile to a mile across, or at right angles to, the stiff breezes which prevail at Gatun. It was eventually discovered "that a regular purposive flight of mosquitoes towards the town took place in the evenings before nightfall from 6.30 to 7 p.m. After dark the flight was reduced to practically nothing. During the period of flight, the observers were bitten continuously. After the flight ceased, the observers were bitten only once or twice in an hour's time. A return flight began at 6 a.m. and took place with extraordinary rapidity. As daylight became stronger, the speed of the returning *Anopheles* increased. The termination of both forward and return flights was remarkably abrupt. One observer said the flight stopped with almost mechanical precision when there was too much daylight or too much darkness."

Willey [1] records an interesting example of periodic habit occurring among crows and so-called "flying foxes" (really fruit-eating bats) on the coast of Ceylon. At one place, a small lighthouse islet off the coast of Ceylon, they congregate in the palm-trees alternately by night and by day. "At sundown," says Willey, "the passage of immense flocks of crows and flying foxes in opposite directions across the strait which divides the island from the mainland can be witnessed, the former bound for the island to rest for the night, the latter speeding their way to the mainland intent upon their nocturnal forage. . . . The reverse passage—namely, the matutinal flight—takes place towards sunrise, the bats returning from the mainland to rest for the day suspended in rows from the midribs of the palm-leaves, the crows crossing over on their daily quest for garbage." As a result of these markedly periodic habits, the two classes of animals are able to make their homes in the same trees, without in the slightest degree interfering with each other.

The effect of external periodicities on the organism and its behaviour is nowhere better seen than on the seashore. The case of *Convoluta roscoffensis* is perhaps too familiar to need much description. *Convoluta roscoffensis* is a minute, elongated flat-worm covered with cilia, and containing green algal cells living with it symbiotically. Its habitat is a narrow strip of sandy beach on the coasts of Normandy and Brittany situated at the level reached by high-water at the slackest of neap-tides. Though of very small size, the worms occur in such enormous numbers as to form at low tide great patches of green scum. As the tide laps the edges of the colony the green patches disappear, the worms remaining

[1] Willey, A., *Convergence in Evolution.* (London: Murray, 1911.)

beneath the surface till the next ebb-tide. Twice during 24 hours the zone occupied by the colonies is submerged and the animals live in darkness underground, and twice the zone is uncovered and the animals rise to the surface. The burrowing reaction is due to the necessity of avoiding extermination by wave-shock, the upward movement is determined by the presence of the algal cells and their light requirements. The egg-laying of *Convoluta roscoffensis* is also periodic, and is related in a remarkable way to the rhythm of the tides. Egg-laying begins with the onset of the spring tides and continues for a week. The reason for this is as follows: In the summer, the time of year when these observations were made, the low-water of spring tides at Roscoff occurs about midday and midnight.[1] When, however, the zone occupied by the Convolutas is uncovered during the night-time, the animals in the absence of light do not rise to the surface. Hence, during the spring tides, the worm has an uninterrupted period of some 18 hours in which to lay its eggs. Experiments in the laboratory have shown that egg-laying reaches its maximum when the animals are not obliged to come to the surface twice in 24 hours—that is to say, when they can have the longest possible spell of darkness; in other words, the conditions most favourable to egg-laying occur when the moon is full or new—a remarkable example of the effect of the tides on the habits of shore animals.

Shore animals like the common periwinkle, for instance, are submitted to a double periodic influence: the rhythmic ebb and flow of the sea and the alternation of day and night. The existence of periwinkles comprises regularly alternating periods of active life in the water or moist air (at high tides) and periods of suspended animation within their shells. This constant reaction to the tidal rhythm is not without a profound influence on the functions of the organism. For instance, inert periwinkles, even in a dry environment, can be reactivated by shaking; but, according to Bohn, the reactivation occurs much more readily at certain times and hours. Bohn[2] states that if a collection of these molluscs has been isolated for a certain length of time in a laboratory, it is easily demonstrated that, at periods of low tide, one has to shake much longer to produce the reactivation than when the tide is high. That is to say, the periods of inertia in the laboratory correspond to the periods of desiccation on the shore.

The impress of the external rhythm on the organism is,

[1] Keeble, F., *Plant-Animals.* (Cambridge: University Press, 1910.)

[2] Bohn, Georges, *Institut Psychologique: Travail du Groupe d'Étude de Psychologie zoologique. Mémoire I: Attractions et Oscillations des Animaux marins sous l'influence de la lumière.* (Paris, 1905.)

of course, not permanent, but becomes gradually weaker with the passage of time. Numerous similar cases occur, both in plants and animals, but the above are sufficient for our purpose.

It is common knowledge that in many animals the colour is not fixed, but varies according to the hue of their surroundings. This power of colour change has been investigated most minutely in the case of the Æsop prawn (*Hippolyte varians*).[1] The colour of this crustacean is extremely variable, the change in coloration being brought about by the expansion or contraction of masses of pigment. The cells in which the pigment is situated are very irregular in form, with branched processes. On appropriate stimulation, the pigment flows out along these branching spaces in such a way that what was a mere pin-point of pigment becomes spread out over a wide surface. The result is a change in coloration. The Æsop prawn owes its colour to three pigments—red, yellow, and blue. In the daytime, the many changes of colour, in response to varying surroundings, are due entirely to the red and yellow pigments. At nightfall the colour of Hippolyte, whatever it may happen to be at the time, changes to a transparent azure blue, this blue colour being replaced at daybreak by the prawn's diurnal tint. The Æsop prawn thus exhibits rhythmic colour-change corresponding to the transition from light to darkness and vice versa.

So far the cases we have been considering of rhythmical behaviour are all of a clear-cut and simple kind, being directly due to the succession of the seasons, the regular alternation of night and day, the ebb and flow of the tides, etc.; moreover, the nature of these latter phenomena is also well understood. On the other hand, the causes underlying the now well-established cyclical changes of climate are not only themselves decidedly more complex, but there is still a great deal to be learned about their effects in relation both to animals and mankind.

The first type of climatic change, and the best known, is that of the Glacial Period. It is now known that the Glacial Period was not a continuous period of intense cold, but was punctuated by epochs in which the weather was much warmer, and when the retreat of the ice-sheet allowed many animals and plants to regain, temporarily at any rate, the ground they had been obliged to cede. These fluctuations of climate are believed to have affected the whole world simultaneously, and it is certain that the whole of the Northern Hemisphere was affected.

The second type of climatic change is less familiar, having

[1] Keeble, F. and Gamble, F. W., *Phil. Trans. Roy. Soc.*, B, vol. cxcvi. (London, 1904.)

been only recently established. Brückner, Clough, and others consider that the whole world passes through a climatic cycle once in every thirty-six years. At one end of the cycle the climate of continental regions for a period of years is unusually cold and wet, with relatively frequent storms; at the other the climate is warmer and drier, with high barometric pressure and few storms. The extremes of low temperature are ascribed, somewhat paradoxically it may seem, to periods of maximum solar activity as shown by the number of sun-spots and the rapidity with which they are formed.

The Swedish hydrographer, Petersson, has published data showing the importance of these climatic cycles to the study of hydrography, and thence to the study of fish-migrations.[1] Increased solar activity results in oceanic circulation being more intense. As a result of this, the Baltic, the water of which is normally distinctly brackish, receives a greater quantity of salt water from the Atlantic. Owing to the increased oceanic circulation outside, large quantities of water are pumped into the Baltic through the narrow neck between the Skager Rack and the Kattegat. As a result of the increased salinity of the Baltic, the herring shoals are enabled to extend their migrations to this sea, which is normally closed to them owing to their intolerance of water with a low salt content. The times of the appearance of herring in the Baltic thus correspond to the periods of increased solar activity; in other words, the appearance of herring in the Baltic is a regularly periodic phenomenon.

A recent American expedition to Turkestan has brought to light regular cycles of wet and dry climate in this region.[2] One of the most convincing pieces of evidence of these climatic cycles is furnished by the changes of level which can be traced in the waters of the Lop Nor basin, a huge enclosed area about three times as large as the British Isles, situated in the heart of the Asiatic Continent. The periods of drought result in periods of famine which cause the nomadic races, which live on lands too dry for agriculture, to migrate and so come into conflict with the peoples of more favoured regions. The wave so started, say in the centre of Asia, propagates itself in ever-widening ripples, the most remote of which may even be the cause of a commercial crisis as far away as the United States. Anyone who has lived for a few years in Southern Italy cannot fail to marvel how the old Romans could ever have achieved so much if the climatic conditions were as enervating as they are now. But evidence is forthcoming to show that such was not the case.

[1] Petersson, Otto, *Der Fischerbote*, III, Nr 7, 8, 9. (Hamburg, 1911.)
[2] Ellsworth Huntington, *The Pulse of Asia.* (London: Constable, 1907.)

The historian, Gibbon,[1] mentions two remarkable facts which tend to show that the climate of Europe in Roman Imperial times was much colder than it is now. The Rhine and the Danube were frequently frozen over and capable of supporting the most enormous weights, a thing unparalleled in modern times. In the time of Cæsar, the reindeer, now confined to the area around the Poles, was a native of the Hercynian forest which then covered a great part of Germany and Poland. To quote Mr. Huntington: "Apparently the climate of the earth is subject to pulsations of very diverse degrees of intensity and of varying length. The Glacial Period as a whole represents the largest type of pulsation; upon it are superposed the great pulsations known as glacial epochs, each with a length measured probably in tens of thousands of years; their steady progress is in turn interrupted by smaller changes of climate such as those of which evidence has been found during historic times in Central Asia; and, finally, the climate of the world pulsates in cycles of thirty-six years."

In spite of the undoubted influence of climate, it would seem that the growth and decay of successive civilisations is in great part a biological phenomenon analogous to the age-cycles referred to above, although the matter is evidently far too complex to allow one to generalise with safety.

According to Prof. Flinders Petrie,[2] as quoted by Spurrell,[3] "there have been eight distinct periods of civilisation in Europe, from the earliest dawn of civilisation in Egypt, the duration of each period tending to be longer than its predecessor; and the intervals are marked by an inrush of barbarian races and an interlude of destruction and admixture of blood." Prof. Petrie founds his analysis of civilisation on sculpture, on the grounds that sculpture is available over so long a period and is so easily presented to the mind. In the sculpture of every period can be seen the same sequence of growth and decay. The archaic stage, in spite of crudity, is invariably marked by boldness and vigour. Next, the treatment loses its archaic character and becomes more free, the details being more skilfully subordinated to the whole. From this point the period of highest achievement is soon reached: all traces of archaism have disappeared, inspiration is still powerful, and workmanship wellnigh, sometimes entirely, perfect. After this the treatment tends to become over-elaborate, the inspiration is lost, and a period of unintelligent copying ensues, followed by one of degradation and ultimate decay.

[1] Gibbon, E., *Decline and Fall of the Roman Empire*, chap. I.

[2] Flinders Petrie, W. M., *The Revolutions of Civilisation.* (London: Harper, 1912.)

[3] Spurrell, H. G. F., *Modern Man and His Forerunners.* (London: Bell, 1918.)

Civilisation is an intermittent phenomenon, its growth and fall being comparable to summer and winter in Nature. Prof. Petrie shows how this analogy was familiar to the ancients under the guise of the " Great Year," the Etruscans speaking of the Great Year as the period of each race of men that should arise in succession. He makes the following quotation from Plutarch's *Life of Sulla*, which refers to the close of the Etruscans' own period of 1,100 years, in 87 B.C.: " One day, when the sky was serene and clear, there was heard in it the sound of a trumpet, so shrill and mournful that it frightened and astonished the whole city. The Tuscan sages said that it portended a new race of men, and a renovation of the world, for they observed that there were eight several kinds of men, all differing in life and manners; that Heaven had allotted to each its time, which was limited by the circuit of the Great Year; and that when one race came to a period, and another was rising, it was announced by some wonderful sign from either earth or heaven. So that it was evident at once to those who attended to these things, and were versed in them, that a different sort of man was come into the world, with other manners and customs, and more or less the care of the gods than those who had preceded them. . . . Such was the mythology of the most learned and respectable of the Tuscan soothsayers."

From the foregoing examples it becomes evident that life, in its main aspects, is essentially a rhythmic phenomenon. The essence of rhythm being order, it seems, indeed, inevitable that, with the progress of time, all biological phenomena of importance, whether concerned with the inner functioning of the organism or with its behaviour in relation to the outside world, should tend to become increasingly rhythmic in character.

Finally, it should be evident that the sense of rhythm, which forms so large a part of the pleasure conveyed by all the higher forms of art, results from the successful expression by man of his appreciation of the order and measured flow so characteristic of his own nature and of the world about him.

POPULAR SCIENCE

SOME SCIENTIFIC ASPECTS OF COLD STORAGE

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PART III [1]

In our earlier articles we have pointed out that as regards preservation in cold storage, foods fall into two groups, those in which the material is held above the freezing-point, and those in which the food is kept in the frozen condition. As examples of the former we have selected fruit and chilled beef, both of which present their special problems. In this article we shall confine our attention to the special problems presented when food is preserved in the frozen condition.

There are several advantages in keeping produce in the frozen as contrasted with the chilled state. Not only does the lower temperature reduce considerably the rate of chemical reactions which produce deterioration of the material, but the growth of micro-organisms in the solid condition is inhibited or takes place with extreme slowness. In consequence frozen material can be kept in cold storage for a much longer period than produce kept in the unfrozen condition. This may be of the utmost economic importance, for when it becomes possible to substitute freezing for chilling in the cold storage of any food substance it may increase indefinitely the distance over which such food materials may be conveyed. This can be made clear at once by reference to the case of beef. In the chilled

[1] The scope of this article was planned by both authors at the same time as that of the earlier articles in this series. Owing to illness abroad Mr. Jörgensen has, unfortunately, not been able to take part in the actual preparation of this concluding article. For faults of presentation I am, therefore, alone responsible.—W.S.

unfrozen condition this cannot be relied upon to last longer than thirty days without the risk of substantial deterioration, so that importation of chilled beef from places from which the transit takes longer than three weeks is not usually undertaken. If frozen beef of quality equal to the best chilled beef could be obtained the importation of beef from any part of the earth could be brought about as successfully as, for instance, that of frozen mutton.

Other important advantages of freezing are economy of space and convenience in handling. Frozen meat can be much more closely packed than chilled meat and requires much less attention, and the financial advantages accruing from these facts far more than compensate for any extra cost resulting from keeping the produce at a lower temperature.

Certain animal produce has been found suitable for preservation in the frozen condition, among which may be mentioned mutton, rabbits, and some species of fish. In other cases where, for the reason already mentioned, it would be extremely advantageous to preserve the material in the frozen condition, freezing can be used, but produces, as a rule, a decided deterioration.

By applying scientific principles and scientific methods of research there is, however, every reason to believe that methods of freezing can be devised which will prevent deterioration in these food substances. In other cases, as with fruit, freezing has so far proved impossible.

As has already been indicated, the principle involved in storing meat and other commodities in the frozen state is the replacement of the liquid water by solid ice, and in this way removing the liquid medium in which chemical reactions take place and micro-organisms flourish. Thus W. D. Richardson says: "It is the solid state and not any specific temperature (*e.g.* 0° C.) which is the limiting condition for growth and reproduction of micro-organisms."[1] We have already referred at the end of the second part of this series of articles to the principal difficulty encountered in the preservation of organic tissue by freezing. This is, that after thawing, water which was previously bound in the cell contents becomes free and drips from the tissue so that a loss of fluid occurs, and this "drip," as it is sometimes called, may carry with it appreciable quantities of nutritive material. This difficulty is especially noticeable in the case of frozen vegetable tissue. If, for instance, a potato, carrot, or an apple be frozen and subsequently thawed, on squeezing the tissue in the hand, water will run

[1] W. D. Richardson, "The Cold Storage of Beef and Poultry," *Premier Congrès International du Froid, Rapports et Communications*, vol. 2, pp. 261–316, Paris, 1908.

out as it will when a wet sponge is squeezed. On this account freezing of fruit has so far proved quite impracticable. The water loss in the case of beef is not nearly so great, although as already emphasised, it is appreciable enough.

The processes involved in the cold storage of meat and fish by freezing are (1) the freezing of the tissue, (2) its storage at a low temperature, and (3) the thawing (" defrosting ") of the tissue. Each of these processes is capable of a wide range of variation. Thus the freezing and defrosting can be carried out with varying degrees of rapidity, while the tissue can be held in storage at a temperature which can vary over a wide range. In practice there seems no unanimity as to which are the most suitable conditions under which meat or other material should be frozen or thawed, or as to the best conditions for its storage.

Thus in Germany and other parts of Central Europe the general practice appears to be to hang the carcases in cold air at a comparatively high temperature (–4° C. to – 8° C.), whereas in America and Australia and New Zealand the freezing chambers are kept at a much lower temperature (about –15° C.). A similar variation in practice exists in the temperature of storage, this being usually several degrees higher on the continent of Europe than in America. Diversity of opinion also exists as to the best method of thawing.

If a fish, for example, or a piece of meat is frozen in cold air and a thermometer or other temperature-measuring instrument placed in it so that the temperature of the middle of the object is measured, it is found that the temperature falls regularly until it reaches the neighbourhood of – 1° C. At this point the temperature remains stationary for a length of time depending on the dimensions of the object frozen and on other factors, until ultimately the temperature again begins to fall and continues to do so until it approximates to the temperature of the external cooling medium. The form of the curve connecting the temperature of the middle of the frozen object and the time that has elapsed from the commencement of the freezing process is shown in Fig. 1. This form of curve is quite general for all tissues, whether plant or animal. It holds equally well for meat, fish, fruit, vegetables such as carrot or potato, and indeed for any material which consists, from a physical and chemical point of view, chiefly of water with a small quantity of substances dissolved in it. Thus similar curves are obtained in the freezing of gelatine and agar-agar jellies. When frozen tissue is thawed a somewhat similar curve is obtained.

The freezing and thawing curves thus exhibit what may be termed horizontal portions which represent a period during

which the temperature of the object under investigation remains approximately constant at -1° C. This is the period during which the water in the tissue is freezing (or thawing), that is, during which a change in phase is taking place, to use the language of physical chemistry. As the great difficulty experienced in the preservation of beef and other food substances arises from the change in the water relation in the tissues, great interest attaches to the changes which may take place during this change of phase. If these changes in the water relations of the tissue which occur on freezing can be made reversible on thawing, the " drip " is avoided, and

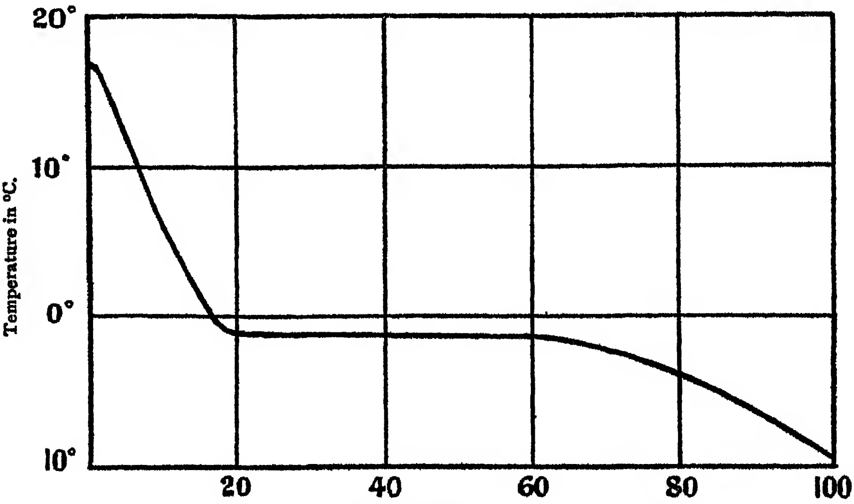


FIG. 1.—The relation between time and the temperature of the middle of a piece of beef, weighing 15 grains, frozen in cold air at -20°C.

one of the great problems of cold storage in the frozen condition is solved.

These changes in the water relation which may take place during the change in phase may now be considered. In the first place there may occur a separation out of the different constituents of the cell; that is, the molecules of water, salts, proteins, etc., which in the unfrozen tissues constitute an apparently homogeneous substance, may during the change in phase undergo a regrouping so that part of the water is no longer held bound in the water-salt-protein complex. In this connection metallurgical work on the solidification of alloys may throw much light on our particular problem, for it is now well known that the rate of solidification of a molten alloy influences profoundly the structure of the solidified mass.

There may also arise mechanical injuries to the tissue, such as may result if the tissue, like pure water, should expand on freezing.

It would not be a matter of much moment in practice if the changes which took place during freezing were absolutely reversed on thawing, for then the last state of the tissue would be the same as the first. As a matter of fact, it is known from everyday experience that this is not the case. Richardson states that with slow thawing the changes are made reversible, but recent German work denies the possibility of this taking place, at any rate in the case of fish.

Since the disturbance in the water relation of the tissues takes place principally during the period represented by the horizontal portion of the freezing curve, it would appear probable that shortening this period would correspondingly lessen the disturbance produced in the water relation, or, stated in other words, it is to be expected that with increased rate of freezing and thawing there will result a corresponding decrease in the disturbance to the water relations in the tissues.

The rate of freezing of a body depends on a number of factors. Naturally it depends on the size of the body frozen and on its shape; the larger the body the longer the time taken to freeze it. And here it may be pointed out that when a body is frozen by immersion in a colder medium, every part of the body does not have the same temperature at the same time. The outer parts of the cooled object fall rapidly in temperature and soon become frozen, whereas, further inside the body, the time taken for the freezing process to take place increases, being longest in the middle of the thickest part. Thus the rate at which the inner parts of the cooled object freeze depends on the " heat conductivity " of the body, *i.e.* on the rate at which heat can pass through and so be drawn from the inner regions.

It will also depend on the specific heat of the cooled object, that is, on the quantity of heat which must be withdrawn to lower the temperature of unit mass of the substance through one degree. Among the different food substances which it is desirable to preserve by freezing, there is not much variation either in specific heat or conductivity, for the tissues in question are so largely water that both the specific heat and conductivity cannot be far removed from those of water. There are, however, three other factors on which the rates of cooling and freezing depend; these are the temperature of the cooling medium, the extent to which the latter is kept in motion, and its heat-conductivity. As regards the temperature of the cooling medium the lower this is the more rapid will be the rate of cooling. This is the fact summarised in Newton's well-known

Law of Cooling, that the rate of cooling is proportional to the difference in temperature between the cooling body and the external medium. Stirring, by removing warmer gas or liquid from the surface of the cooling body, also increases the rate of freezing. As regards conductivity for heat different substances vary greatly, but in practice naturally the choice of a freezing medium is limited to those which can be used without damaging the cooled material. The only materials which have so far been employed at all on a commercial scale are air, water, and brine. The great advantage of air as a freezing medium obviously lies in the simplicity of the procedure involved in its use. It is no wonder that the vast majority of food materials kept in the frozen condition are frozen in cold air. But there is a great difference between air on the one hand and water or brine on the other in regard to heat conductivity. The conductivity of water and brine is about twenty-eight times as great as that of air. Consequently their capacity for removing heat from the surface of the cooled body is very much greater, and so freezing will be accomplished much quicker in a bath of brine than in air, other conditions being the same. Generally brine has to be used, as it can be obtained in the liquid condition at a temperature of $-21°$ C., whereas water freezes into a solid mass at $0°$ C. Nevertheless for some food produce freezing in ice-block has been successfully employed.

It is thus possible to vary the rapidity with which a particular food material is frozen, and so we are in a position to examine how far differences in the freezing velocity affect the loss of liquid or "drip" which occurs on thawing the frozen tissue. For this purpose some experiments made with gelatine and agar-agar are very instructive. If a block of gelatine jelly is frozen slowly in air at a temperature only a few degrees below $0°$ C., after thawing the water can be squeezed out of it as the water can be squeezed out from a wet sponge. If, however, a thin piece of gelatine gel is frozen rapidly, as, for example, by freezing in mercury (which has a particularly high conductivity) at say $-20°$ C., then, after thawing, the water loss is no greater than that which takes place from similar material unfrozen. The same phenomenon can be observed with agar-agar gels if these are concentrated enough and the freezing is carried out sufficiently rapidly.

These experiences lead us to expect that the application of more rapid freezing to the preservation of food materials will overcome the difficulties resulting from changes in the water relations of the tissues brought about by slower freezing methods. Accounts of work in which a more rapid method of freezing is employed are now available and fully bear out this

expectation. Although, as we pointed out in our last article, the method of freezing tissue in cold brine was a British invention, it is from Germany that published results, in which this more rapid freezing of food has been undertaken, are so far available. It is found, for example, that most fish are suitable for preservation by freezing, but that the usual method of freezing slowly in air is much less satisfactory than freezing in a brine solution because by the former method large ice-crystals are produced in the fish outside the muscle fibres, which damage the fibres, whereas by rapid freezing in brine the muscle liquid solidifies inside the fibres themselves and hence does not exert any external pressure on the fibres. As a consequence fish frozen in brine is of a considerably higher quality as regards nutritive value, appearance, and taste than fish frozen in air.[1] Similar results are obtained with meat,[2] but at present no details have apparently been published in regard to scientific experiments carried out with meat. An attempt has however been made to calculate the relation between the rate of freezing and the condition of the frozen material.[3] These calculations, however, include certain assumptions which render the data obtained only approximate. In the results given, a quantity called the " freezing velocity " is utilised. When a block of tissue is frozen the outer part is first frozen, and the limit of frozen tissue continually marches inwards. The rate at which this march proceeds is called the " freezing velocity." It is calculated that when the freezing velocity proceeds at a rate of from 1 to 2 cm. per hour or quicker, the release of water from the muscle fibres is prevented, and consequently the " drip " from the meat on thawing does not take place.

Contrary to expectation it is stated by the German investigators that the rate of thawing is without influence on the water relations of the tissue. This is a question to which no doubt further attention will have to be given, and the same applies to the changes taking place in frozen food material during storage, about which very little definite information is available.

In these articles we have selected for consideration some of the scientific aspects presented by the cold storage of three

[1] R. Plank, E. Ehrenbaum, and K. Reuter, " Die Konservierung von Fischen durch das Gefrierverfahren," Berlin, 1916.

[2] R. Plank and E. Kallert, " Ueber die Behandlung und Verarbeitung von gefrorenem Rindfleisch," Berlin 1916.

[3] R. Plank, " Über den Einfluss der Gefriergeschwindigkeit auf die histologischen Veranderungen tierischer Gewebe," *Zeitsch. f. allg. Physiol.* **17**, 221-38, 1918.

kinds of food, each of which presents its own scientific problems, namely, fruit, chilled meat, and frozen meat and fish. We have not aimed at giving a complete account of the scientific problems that arise in connection with these matters, but rather we have tried to show that the application of the principles of pure science to these industrial questions will bring about improvement in methods of food-preservation which must be of the greatest value to the nation and the world at large. At the same time we would emphasise that each food material presents its own particular problems, and it seems not improbable that the science of food-investigation, of so much importance to the human race, will at no distant date take as prominent a place as other branches of science of obvious economic importance.

CORRESPONDENCE

To the Editor of "Science Progress"

LATIN OR IDO?

From Gilbert H. Richardson

Dear Sir,—The function and purpose of language is to communicate thought from man to man. We may liken it to a telephone. Telephones may be more or may be less efficient. Languages likewise may be compared as regards their efficiency. We may separate this into two factors :

i. Ease of acquirement and use.
ii. Expressiveness.

The most efficient language would be the one capable of being acquired and used with the least expenditure of labour, and at the same time capable of the greatest power of expression.

i. *Ease.*—Command of Latin for scientific purposes would, I presume, usually be attained only at University grade; study extending over some six or seven years would be required.

This labour would be required partly to acquire the vocabulary, and partly to master the grammar and syntax.

The writers in Science Progress for January suggest that Latin might be to some degree simplified. And to the impartial observer it must be obvious that a good deal of the complexity of Latin grammar is theoretically quite needless. Why genders? Why more than one declension? Why more than one conjugation?

But the simplifying of Latin has already been done for us on the great stage of history. The Goths, Vandals, Burgundians and Franks have simplified Latin into the Romance languages. They have abolished the declensions, but they have broken up the linguistic unity of Western Europe.

The international language Ido is the highest common factor of the modern languages of Europe, and it turns out to be, to a high percentage, Romance.

A man who knows only French knows 91 per cent. of the roots, one who knows only Italian knows 83 per cent. of them. One who knows only Spanish or only English will recognise at sight 79 per cent. of them, and even the mere German speaker will know 60 per cent., and the mere Russian speaker 52 per cent. (see *Complete Manual of Ido*, Isaac Pitman & Sons, 1919).

Can Latin hope to compete with a vocabulary so international as this? To make it adequate to the needs of modern science, it must be flooded with new words, some coined from Latin and Greek, some barbarian. Lucretius (I, 136–9) in his day had to deplore the poverty of Latin for scientific purposes compared to Greek.

As to grammar, Ido is marvellous. (*See* Elementary Grammar, Isaac Pitman & Sons, 6*d*.) No genders, no declensions, and for all the Latin paradigms of verbs 14 syllables.

	Present	Past	Future	
for both numbers and all persons	-as	-is	-os	insertion of -ab- forms pluperf. and fut. perf.
infinitives	-ar	-ir	-or	
participles active	-anta	-inta	-onta	
,, passive	-ata	-ita	-ota	
imperative	-ez			

And this for all verbs. None are irregular.

Why have the successors of Newton and Linnæus dropped Latin? Presumably because they have found French and other modern languages more convenient.

And here is the experience of a contemporary, Canon J. Gross, of the Great St. Bernard, Switzerland:

"After studying Latin for eight years, I now read Latin every day and I can't speak it fluently, and this has been my custom for thirty years."

It is proper to schoolmasters, I suppose, to aim at getting from their pupils as much work as possible; manufacturers aim at getting the work done with the minimum expenditure of labour and energy. Is there not in this matter of mental communication a case for adopting a labour-saving device?

But mere ease is no sufficient criterion of the efficiency of a language. A few gesticulations might be acquired easily, but would be worth very little as means of expression.

ii. *Expressiveness.*—The richness of expression of a language depends on the wealth of its vocabulary and on its

ability to avoid ambiguity, to discriminate meanings and shades of meaning.

French is probably more capable of clear expression than English. In English many words can be noun, adjective, or verb, without change of form, and the reader may hesitate which way to take them.

Latin, in its grammatical arrangements, may be fairly clear for those who are well at home with it. But its terminations are largely ambiguous: -a is fem. nom. sing. and nom. and acc. neut. pl., and if length marks are omitted, also abl. fem. sing.; -um may be acc. sing. masc. or nom. or acc. neut. sing. or gen. pl.; -ae may be gen. or dat. sing. fem. or nom. pl. fem.; -i may be gen. sing. or dat. sing. or nom. pl. These ambiguities can hardly count as merits in efficiency. The detached observer might be inclined to remark that any child might have arranged the terminations better.

In Ido—o means noun sing., -i noun pl., -a adj., -e adv.

And as to Latin vocabulary, let anyone turn the pages of the Latin dictionary and notice how numerous are the words, and what common ones, which have six, eight, or ten meanings, and meanings seriously wide apart from each other. Ratio, tollo, fero. Fero means among other things to carry, plunder, rush, tolerate, direct, show, vote. (See *The Auxiliary Language Ido*, by L. de Beaufront. Isaac Pitman.)

Does it seem likely that science, which above all things requires clear thought, would be well served by such a vehicle?

Ido approximates to the ideal of "one word, one meaning." Of course the words of one language frequently include more or less than the nearest corresponding word in another language. In Russian the word for "hand" includes also the meaning "arm." Two or more meanings are occasionally allowed in Ido, but only when they are unlikely to clash.

But Ido is often even more precise than "one word, one meaning." What in English or French is one word is separated out into two or more forms to distinguish different meanings—*e.g.*, "number" as ordinal is "nombro," as cardinal is "numero."

Ido technical terminology is still to complete, but a mathematical and a biological vocabulary have already been published: "organisation" as a condition is organ-iz-es-o; as an action, organ-iz-o; as a product, organ-iz-uro (obtainable from Ido-Edileyo Lüsslingen, Solothurn, Switzerland).

Examine the French-Ido dictionary (Dictionnaire Français-Ido, par L. de Beaufront et L. Couturat. Paris Imprinerie Chaix, 1915), and you will find abundant examples of shades of meaning distinguished with greater accuracy in Ido than in French.

Latin is now taught, not for use in communication, but for mental discipline. I should like to know what experts in education would say of Ido as a means of mind-training. My impression is that it has high value, it is so clear and logical.

Latin is also taught because of its literature. That is a purpose that may well be distinguished from the discipline of its grammar. The vogue of Ido is only beginning, its literature may be in the future. Let me conclude with a specimen. It is a passage translated from Bergson :

"Ni serchas nur la preciza senco donata da nia koncio a la vorto, 'existar' e ni trovas ke, por koncioza ento, existar esas chanjar, chanjar esas maturigar su, maturigar su esas krear su ipsa senfine. Kad on pocas dicar samo tote generale pri l'existo."

Yours faithfully,
GILBERT H. RICHARDSON.

July 28, 1919

THE GHOST-HYPOTHESIS

I.—FROM C. A. RICHARDSON, M.A.

DEAR SIR,—An essay-review appeared in your October number on a book on Spiritualism by the late Dr. Charles Mercier, which calls, I think, for some protest. The name of the reviewer does not appear to be mentioned, perhaps wisely. I am not here concerned to take up any definite attitude as to the truth or falsity of spiritualism; but I should like to urge that criticism of assertions which have received the support of such well-known and rigorous investigators as Crookes, Flammarion, Lodge, and Barrett (to name but four), should only be entrusted to one who is at least moderately instructed in the matters he raises as relevant to the point at issue. I will illustrate from the review mentioned. The reviewer speaks first of what he calls the "*a priori* improbability" of spiritualistic phenomena. Now, strictly speaking, *a priori* improbability has no application to experienced events, for these are unique and particular. For this reason, the first example given, that of the three sides of a triangle, is quite off the mark. A geometrical proposition is a *general* proposition, deduced by means of principles of reasoning which are themselves general, from a certain body of postulates which are also general propositions. In the question under discussion, however, we are concerned with what are purely *particular* matters of fact. The point may be passed over, however, for apparently what the reviewer really means here is that it is extremely improbable that events will be experienced in the future of a different type from those experienced in the past. This, of course, assumes some such principle as the Uniformity of Nature, for which there is no ultimate guarantee. But, in any case, the argument from improbability is a risky one to play with, and apt to lead to unpleasant surprises. One need only mention the case of the alchemists with their theory of transmutation. They were subjected to precisely the same kind of criticism from certain quarters as are the spiritualists. Yet now that

the alchemists are at last utterly discredited, it suddenly turns out, as a consequence of the discovery of radio-activity, that they were right after all in their essential ideas. There *is* such a thing as the discovery of facts of an entirely new type, and science would be quite stagnant if all scientists adopted the "improbability" attitude. But apart from all this, the argument breaks down in this case, for it could only hold if the alleged phenomena were of a kind never experienced before, whereas every age teems with accounts of similar happenings.

The next objection urged is that the existence of "ghosts" would violate the invariable association of sound- and light-vibrations with matter. But why are these invariably associated? To any one who has studied modern philosophy of science the answer is clear. It is because all such are abstract conceptions consisting in logical constructions built up out of *common* elements—namely, sense-data, the ultimate data of knowledge. A visual apparition is on precisely the same level as the experience of any other group of sense-data, the only difference being that here the correlations among the sense-data may be somewhat unusual, a fact which would simply bring us back to the "improbability" argument dealt with above. As for the ancient difficulty of the "ghosts of the clothes," this disappears when it is realised that apparitions, if they exist, are probably more akin to vivid images telepathically originated than to sensations proper. With regard to the assertion made by the reviewer that ghosts do not "appear to us in broad daylight, and let us examine them thoroughly, and take photographs of them," it may be pointed out that Crookes stated that all these phenomena had actually occurred.

The next point raised is as follows: "Then there is the whole body of evidence suggesting that mind is only 'the secretion of the brain'—*evidence which has not been even damaged* by the modern pseudo-philosophers, who are out to prove themselves above the order of nature" (italics mine). From this we can only conclude one of two things: Either the reviewer classes nearly all modern philosophers as "pseudo-philosophers" (if so, comment is needless!), or else he is profoundly ignorant of the whole trend of recent philosophic thought. In the first place, it is now generally recognised that a phrase such as "mind is the secretion of the brain" is quite meaningless as it stands. One might just as well, and with equal significance, say, for example, that "digestion is the secretion of the stomach." However, we may recognise that some such catch-phrase as this is simply the symbolic slogan of the materialistic school of thinkers. Now it is nothing less than ridiculous in the face of obvious facts to state that materialism has not been even damaged by the results of modern inquiry. The whole tendency of the latter is to set more and more strongly against it. As for the concomitance of brain-events and mind-events on which the reviewer insists, it is susceptible of quite other explanation than the materialistic—an explanation far more satisfactory in the light of *all* the facts. Here it need only be said that we know about brains through perception alone. So far as we are concerned, a brain is simply a group of sense-data, and the perception of it *presupposes* the existence of a perceiving mind. Materialism is thus a gigantic *ὕστερον πρότερον*. Consequently there is no final reason against the hypothesis that a mind temporarily disabled by association with a damaged brain may entirely recover when that association no longer exists. It is not "Mr. Jones," but *his body*, that is built up of bones, flesh, and blood.

The reviewer next goes on to remark that people who have read the evidence for spiritualistic phenomena will probably come to certain conclusions. The first

is that these occurrences are no more wonderful than the tricks of a professional conjuror, which are later referred to in the review as "Prestigiatory (!) Phenomena." It is doubtful whether this is true; but even if it were, one must not forget the difference in the conditions under which a conjuror works and those arranged for the testing of a medium under the rigorous supervision of men such as those mentioned at the beginning of this letter. Also one can hardly attach much weight to a criticism which would lead to such results as, for example, that all rabbits are produced out of hats. Then we have the old arguments as to the discovery of trickery on the part of mediums and the greed for money. The first is certainly the saddest passage in the history of spiritualism; but to conclude from it that all mediums are probably tricksters is obviously absurd on the face of it. For trickery occurs in nearly every walk of life, but so, fortunately, does genuine honesty also. The onus of proving the general proposition, "All mediums are tricksters," is on the reviewer. Exactly similar remarks apply to the question of money payments; but the fact that a considerable number of mediums make a special point of refusing to accept payment of any kind is altogether ignored. As for the "poor Indian juggler" referred to, it need only be pointed out that the Indian fakirs are but the lowest order of a priesthood whose superior members claim to perform far more wonderful things, *avowedly by the aid of spirits.*

After all this, the condescending crumb of praise to the "ghost-hypothesis" as "an anodyne for bereavement and a stimulant for noble effort" is nothing less than an insult, especially at the present time. I have expressed my opinions somewhat strongly: but surely it is high time that this sort of thing should stop. The dispute is about certain matters of fact, and therefore can only be settled by direct empirical observation. Jeering criticism is quite futile, and merely reflects unfavourably on the critic. It is the noblest ideal of science to face all the facts, to give a fair field and no favour, and consequently to dismiss no statements which have reasonable backing from men of science without careful and scrupulously rigid investigation.

The reviewer says that "we (busy men) would decline to waste our time over such a foolish undertaking" as (by implication) the investigation of spiritualism. Perhaps he is too busy to do so; then clearly he ought also to be too busy to indulge in jibes and what he rightly calls "amateur" criticism at the expense of the spiritualists. Certainly he has been too busy to trouble to acquaint himself more than very superficially with the real nature of the issues involved.

Yours faithfully,
C. A. Richardson.

October 25, 1919

II. A PLAIN STATEMENT.

Dear Sir,—Studies on Mind Theory may be conveniently grouped for reference as follows:

A. A vast mass of observations, experiments, and thoughts connecting psychological processes with the nervous system or bearing on the evolution, development, measurement, variations, and diseases of mind in men and animals—all leading to the cumulative judgment that Mind is a product of Body.

B. A smaller mass of introspective studies leading to the speculation that Mind and Body are distinct, or even to the speculation that Body is a product of Mind.

C. Certain alleged observations and experiments purporting to confirm B by

showing that Mind apart from Body may sometimes be or become apparent to our living senses.

Dr. Mercier's little book suggested that the observations under C do not really confirm B because they may be explained by the ordinary methods of common sense as being due merely to deception or legerdemain. My review of his book suggested in addition, (1) that the case under A is so strong and the case under B so weak that the evidence under C requires the most rigorous analysis before it is accepted; and (2) that the fact that B and C originated in the natural fear of death, and are, therefore, agreeable to the mass of mankind, is an additional reason why we should hesitate in accepting the speculations which they attempt to support.

Regarding C, the majority of people reject the alleged manifestations for the following sufficient reasons. Most countries contain certain persons, who, by practice and instruction, are able to perform a number of clever tricks, commonly called conjuring or juggling. These are performed by sleight of hand, distraction of attention, previous preparation of the room, collusion of members of the audience, and other artifices. The conjurers are usually (except in card-sharping and thimblerig), quite honest persons, who play the tricks to amuse themselves and their friends or in order to make a living, but who openly repudiate supernatural assistance. Most of us can do some of the simple tricks ourselves for the sake of pleasing children, and so on; but the more wonderful exhibitions given by professional conjurors are almost always so clever that the most astute onlooker fails to detect where the deception lies, even after frequent attempts to do so. Now the spiritualistic exhibitions are of just the same class, being held in prepared and darkened rooms, with all the modern resources of science to assist, before audiences always capable of collusion and often desiring untruth rather than truth (for the reason given above). The only difference is that the juggler now calls himself or herself a medium, and pretends that his or her tricks are done by supernatural agencies. To this claim the man of ordinary common sense replies, "I do not know in the least how your tricks are done; but they are not a whit more wonderful than those of a conjurer; and I therefore see no reason for believing that you have used any but the natural agencies which he employs. Also, I find it easier to think that you are an impostor than to agree that you can at will summon supernatural potencies with which I, personally, have never had any acquaintance." And the man of science adds, "The admission of your claim involves the abandonment of all the conclusions obtained under A. Not only, then, can I find no reasons for admitting your claim, but I do find a vast number of reasons against doing so."

To this the spiritualist always replies, "The manifestations of the conjurer and the medium may be of the same class, but this does not necessarily prove that they are always produced by the same agencies. I will admit that some manifestations of some mediums have, sadly enough, sometimes been shown to have been done by trickery; but this does not prove that all the manifestations of all mediums are always due to trickery. You must therefore admit that some of the manifestations are genuine." Now, this is a glaring example of the *sit-ergo-est* fallacy—it may be, therefore it is. We cannot prove that all the manifestations are not genuine; therefore some of them are genuine: *ergo*, spirits exist. We cannot prove that there are no elephants in the moon! But, of course, there is an infinity of possibilities between *may be* and *is*.

The extent to which this sophistry is used by spiritualists is astonishing. Thus, they talk of "bad séances" and "good séances"—namely, those which are, and

which are not, obviously due to trickery; and suggest that the former throw no discredit on the latter. They even pretend that the manifestations of a medium who has been actually exposed are not necessarily always fraudulent—he was sick or drunk when he perpetrated the frauds, but he is usually quite honest! Lastly, they try to throw *on us* the onus of proving that all mediums are tricksters—as your correspondent, Mr. C. A. Richardson, does in his letter. Not at all. On them lies the onus of proving that all mediums are not tricksters.

We readily admit with Huxley that absolute impossibilities can rarely be established. In the vast majority of cases we are obliged to come to a decision on the sum of probabilities, one way or another—often after large random sampling. When a man has been once convicted of lying or stealing, that is good presumptive evidence (used as such by the law) that he may lie or steal again; and, if he is found again in suspicious circumstances, on him will rest the burden of proving that this time he was guiltless. Still more difficult will it be to clear himself if he tries to do so by pleading the occurrence of a miracle; and this is precisely the case of a medium who has once been detected in fraud when he pretends to "manifest" a ghost or "telepath" a message on a subsequent occasion. When one alleged miracle is proved to be due to a trick, that is good presumptive evidence that all alleged miracles of the same kind have been really due to the same trick. And the whole of the case under C now lies under the same suspicion: the presumptive evidence is against it; and on it lies the onus of clearing itself. Still more generally—if we see a tree in a field we believe that it grew there from the seed or the sapling; if you assert that it fell from heaven it is for you to convince us. Our explanation is a natural and ordinary one, yours an unnatural and an extraordinary one. On him who asserts the miracle lies the burden of the proof.

But our spiritualists claim that their exhibitions have often been tested by "rigorous investigators," who have failed to find any trick or flaw. We ask at once whether these persons would have been able to discover the artifices employed by any ordinary professional conjurer? Certainly not. I have myself watched card-tricks and many kinds of jugglery done in broad daylight a yard or two away from me, without being able to detect the tricks employed. Why then should we suppose that the "rigorous investigators" will be more successful at séances, with the additional security of darkness and an atmosphere of "religious" faith and sanctity? Their failure to do so may as rightly be interpreted as evidence of one of two things, either of the genuineness of the manifestation, or of the cleverness of the deception; and the former involves a miracle, the latter only such a common thing as a fraud. Then again, are the investigators really "rigorous"? They may pretend to be so, while they are secretly or sub-consciously desirous of overlooking any small doings behind the scenes. It is idle to adduce the integrity or eminence of such persons, because we know that the most upright and able men (especially old men) often make mistakes, or fall into fads, or even lose money on the Stock Exchange! Then what of the equally upright and scientific gentlemen who *have* detected frauds at séances or who do not believe in spiritualism? Your correspondent mentions four "rigorous investigators," who, he says, have supported the assertions of spiritualism, but he omits to refer to the hundreds or thousands of men of science who laugh at them. Such a dialectic may be permissible in metaphysics—but not in science. In fact, the argument is worthless both ways.

The pose of the detached scientist who sets out to investigate "psychic manifestations" in the cause of pure truth is becoming rather worn and thin. To

judge from his frequent and obvious credulities he must often be really a man who starts with an *a priori* list in favour of the hypothesis which he proposes to test (to use the words in Mr. Richardson's sense). We have not the least objection to such a pursuit, except for the loss of time which will be occasioned by so forlorn a hope; but I think that, owing to this very circumstance, only those who are already biased are likely to undertake the investigation. I remember that just before the war SCIENCE PROGRESS published a much better apology for spiritualism than that of Mr. Richardson, in which the author, after protesting his scientific detachment, recorded his belief that the cast-off clothing of a dead man can retain memories of the wearer and communicate them to mediums after that wearer's death! Such people think that the mere statement of an hypothesis, however wild, is sufficient proof of it. What most "rigorous investigators" are in want of is not evidence but experience in legerdermain and—judgment. I suggest that "psychic phenomena" should be investigated, not by men of science, but by a committee of professional conjurers. Mediums would then quickly resolve themselves into ether, like the ghosts which haunt them.

But there is this final "intuitive" objection against the whole business under C—that if the higher spiritualism were true, the lower spiritualism must be false. We were taught in our childhood that the spirits of the worthy dead are bright and beautiful beings. If they are allowed to communicate with us they should do so in the fields and the woods, and in the sunlight. There is an indescribable meanness even in the mere supposition that they could ever be forced to communicate with us through shady persons, before curious audiences, within closed chambers, and in an atmosphere, if not of trickery, yet of intrigue, art, and compulsion.

Turning now from the case of *empirical spiritualism* under C to that of *metaphysical spiritualism* under B, we maintain that, while the literature of the latter may often be astute, clever, or even occasionally instructive, it really does not weigh a straw against the mass of evidence under A—and most men of science, especially the biologists, will agree. The mind of humanity—composed, as it were, of the minds of innumerable individuals, as their brains are of brain-cells—has grown considerably since the days of Berkeley and of Kant. It has become instantly suspicious of any trace of the desire to prove things—as when the admirable Bishop sets out to confound "sceptics and atheists," and the Prussian psychocrat resolves to create God. It dreads concatenated propositions because it recognises how often the chain breaks at the weakest link. Berkeley, the father of modern idealists, was probably the cleverest of all of them; but his chain broke as early as the sixth link (Principles) when he neglected to extend his reasoning regarding material objects to others' minds. This neglect vitiates all his subsequent matter. Really his reasoning leads simply to solipsism, the last absurdity of metaphysics; and we can scarcely admit that, with all his virtues, he was the Deity. As for Kant, surely no one trained in modern scientific criticism can accept his psychology. Why it never seems even to have occurred to him that his *a priori's* might be instincts, similar to those of animals, of which we possess many; and he ignores the whole of that first period of mental growth from experience, all memory of which is effaced from our adult minds. And their followers are of the same kidney—constantly avoiding the evidence on the other side, proceeding upon the *may-be ergo is* fallacy, mistaking speculations for theorems, and juggling with words.[1] Thus, when I use *a priori* in the proper

[1] See, for example, Mr. Hugh S. R. Elliot's *Modern Science and the Illusions of Professor Bergson* (Longmans, Green & Co.).

sense, your correspondent accuses me of ignorance because I do not use the phrase in the stale Kantian sense—which I do not accept!

When we ask the advocates of B for clear definitions of their meaning we meet only with evasions. I asked in my review whether it was supposed that animals have ghosts, as well as men?—no answer. Where do these multitudes of ghosts usually live?—no answer. Why do they not frequently communicate with all of us, by day as well as by night, in the field as well as in darkened chambers?—no answer. How do they maintain their earthly personality and even physical strength?—no answer. How can they suddenly make themselves visible or invisible, just as they wish?—no answer. The spiritualist will reply that he cannot answer because he has not yet attained the knowledge. But there is another and more probable reason why he cannot answer—because there is not a word of truth in the whole story. We have only to define the details in order to see the folly of the whole speculation. When I asked why photographs of ghosts cannot be taken, your correspondent declares that "Crookes stated that all these phenomena had actually occurred." Yet this same "rigorous investigator" accepted the genuineness of Katie Cook's performances months after she had been exposed by Mr. Volckman.[1] I do not accept the evidence; and why have not similar photographs been taken by now in thousands? The reader himself can deal easily with the other points raised by your correspondent.

But we are obliged to him for the final demolition of the whole Ghost-Hypothesis. When I asked how it is that ghosts of mere material objects such as clothes, daggers, hands, and faces are so often reported, Mr. Richardson replies that this ancient difficulty "disappears when it is realised that apparitions, if they exist, are probably more akin to vivid images telepathically originated than to sensations proper." Oh, but this is a tremendous admission! Ghosts may then be really not seen at all! They may be due merely to some telepathic titillation of the alleged observer's brain from outside. They may be only fancies after all. But if this be true of ghosts it may also be true of everything that has occurred at every séance ever held—the floating mediums, the spirit-rappings, the skipping tables, the awesome messages may be only foolish illusions generated simultaneously in the brains (?) of the audience by some evil-disposed telepathist in the room, or even miles away. We have always been too polite to suggest mendacity or gullibility, and here is another simple and more agreeable explanation. So, if your correspondent is right, away goes the whole business like a witch on a broomstick, and the discussion is closed!

But what a wonderful thing is telepathy! In future we shall need no telegraphs, posts, writing, printing—even speech. There will be no books, newspapers, committees, parliaments, discussions, controversy. We shall all agree, even about ghosts, because each of us will be able to telepath all he thinks or sees or imagines into everyone else's mind, without any means of communication.

I shall believe in this when I see it. Until then the alternative explanation remains sufficient. The truth is that spiritualists possess enough imagination to see things which do not exist, but not enough to understand the logical consequences of their own wild speculations. That is why they so often meet with ridicule.

Yours faithfully,
The Writer of the ESSAY-REVIEW.

November 5, 1919

[1] Mr. Edward Clodd's book *The Question*, pp. 126-7 (Grant Richards).

III. FROM NEVIL MASKELYNE

(In reply to a letter from the Editor asking him if he was able to do anything which is usually done at spiritualistic séances)

DEAR SIR,—In reply to your inquiry of yesterday, I may say that there is no difficulty whatever in reproducing what takes place at so-called spiritualistic séances. The only trouble is that it is impossible to simulate the atmosphere of solemnity and semi-religious formality which is the chief asset of the impostors who make a living out of this particular fraud.

Yours sincerely,

November 8, 1919 NEVIL MASKELYNE.

NOTES

Increase in Length of "Science Progress"

THE reader's attention should be called to the fact that the length of SCIENCE PROGRESS has been increased in this Number to the equivalent of nearly fifteen pages by setting the larger type close together, and it is hoped that this arrangement will be continued in future. The increase amounts to about 9 per cent. of the former length. The demands upon our space are so great that the attention of contributors is especially invited to the Notice at the beginning of the number.

Charles Arthur Mercier, M.D. (Lond.), F.R.C.P., F.R.C.S.

THE death, in September last, of this distinguished physician, philosopher, man of science and rare craftsman in letters, is a loss that will be widely felt by those who know his works, and a cause of sincere grief to his many friends. Only so much of his life as may seem to bear on his character and line of work can be touched on here, and that but slightly. After a few years' study at Merchant Taylor's School, he felt obliged by circumstances to seek active employment, and for a while he acquired much knowledge of various kinds which bore fruit later. He was at last enabled to begin medical study at the London Hospital Medical School, and as soon as he had won his qualifications to practice he freed himself from all obligations. From this time onwards he worked on persistently and strenuously to within a few weeks before his death at the age of sixty-seven, interrupted at intervals only by the effects of the long and often painful disease from which he suffered for over twenty years: of late severely crippled, but ever keen in mind, and brave to the end.

His reputation as an alienist physician, and as a lecturer at three medical schools in London over a period of many years, was of the best. He was, indeed, one of the few authorities on his special subject who have been at once sound thinkers, accurate observers, learned psychologists, precise writers and luminous expounders of what they had to say.

Photo F Downer, Watford

Dr Charles Mercier

Nor had he been rivalled by many in combining a thoroughly scientific knowledge of insanity and its many allied subjects with a wide practical experience of the details of clinical treatment, and of the general management and requirements of institutions for the insane in all stages of their disorder. Dr. Mercier's best-known works on his special subject are *Sanity and Insanity*, the first edition of which appeared about thirty years ago, and the enlarged edition of his *Text-book on Insanity and other Mental Diseases*, published in 1914. These books cannot be reviewed here. They are invaluable guides to students of insanity of all ages.

As a psychologist, he was known far beyond specialist and general medical circles by his work on *Psychology, Normal and Morbid*, and by numerous articles in many publications dealing with this subject. For many years he was a frequent contributor to the *Journal of Medical Science*, and a prominent and active member of the Medico-Psychological Association. He was fully conversant with the varied literature on psychology, both of the past and present day ; but, though he leaned towards what is known as the introspective method of studying psychological phenomena, he could not be rightly classed as an adherent of any special " school." His book on psychology, already mentioned, was largely adapted for the use of students of mental disorders, the author strongly holding the view that, for the correct understanding of insanity, the study of the normal must closely cohere with that of the abnormal. Mercier's other contributions to scientific journals on this subject deal mostly with psychology alone.

From a study of Mercier's writings on psychology as well as on other matters of a biological nature, or on such as are usually described as subjects of philosophical opinion, it may once more be said that he cannot be strictly classed under any of the philosophical sects which are known by more or less familiar names such as Dualist or Monist, Idealist or Materialist, Vitalist or Mechanist. Deeply impressed, in the early period of his student life, with the writings of Herbert Spencer—he had read them all through twice—his own writings, which treated much of Evolution and Heredity in relation to human development, were, at least the earlier of them, distinctly characterised by this influence ; and, like many medical writers on things of the mind, he held the opinion of the so-called " Neo-Lamarckian " school of biologists that definite " characters," as acquired in the life-time of the parent, were transmissible, as such, to the offspring. He was also, especially in his middle years, a strong believer in the " impassable gulf " (though also in the closest association) between the functioning of the nervous system and that of the mind.

In his later years, however, it appeared to some of his readers that his opinions with respect to these and other matters were considerably modified. In his two books, for instance, on *Criminal Responsibility* and on *Crime and Criminals*, as also in other papers, Mercier's quasi-dualism in psychology, and his Spencerian tendencies in biology seem to affect but little his clear discussion of the action of the human "will" or his apparently later opinions on questions of heredity. And to a considerable extent his book on *Causation and Belief* gave evidence of alteration in his philosophical opinions.

Space allows but short mention of a few others of this author's notable works. His *Conduct and its Disorders* is a very original and valuable work, largely complementary to those on psychology on the one hand, and on insanity on the other. His book on *Spiritualism and Sir Oliver Lodge* is a full, strictly scientific, and brilliant exposure of the fallacious reasoning which leads many writers, on various subjects (some of them specialists in certain branches of science), to announce that actually scientific proof has been arrived at of possible communication by unknown means between persons separated by vast distances, and also between the living and the dead. And his acute and stimulating work, entitled the *New Logic*, though it has been variously judged by some professional logicians, is, at the very least, a book which must be welcomed by all men of science who may be interested in its subject. His masterly criticism of Mill's *Canons of Induction* has not been upset, nor, with the present writer's knowledge, even attacked.

Dr. Mercier possessed a very wide knowledge of many departments of English literature, and a rarely retentive memory. He wrote, without a trace of pedantry, undefiled and trenchant English, and was a brilliant talker, singularly interesting from the number of different subjects of which he had command. Eager and powerful controversialist though he was, both in speech and on paper, he was, none the less, the most clubbable, kindly and generous of men.

Philip Edward Bertrand Jourdain—Born October 16, 1879; Died October 1, 1919 (A. E. Heath, M.A., University, Manchester)

The death of Mr. P. E. B. Jourdain, M.A., is more than a personal or even a national loss; for his work occupies a unique place in mathematical thought. Unable to travel, he yet succeeded in making his influence felt in every part of the world—not merely by his own original contributions to knowledge, but also by the help he so generously gave to others.

Photo Hills & Saunders Cambridge

P E B JOURDAIN

In an age of great acquisitiveness he was content to spare an immense amount of time from his own work to the disinterested labour of abstracting for journals like the *Revue Semestrielle*—strong in the belief that it was worth while to do his share, and more than his share, of such unpaid and almost unacknowledged toil for the advancement of mathematics. His voluminous and important correspondence on mathematical subjects with eminent mathematicians of all nations shows, what all those who came into contact with him know, that his capacity in his own chosen fields of work—mathematical logic and the history of mathematical ideas—exerted no narrowing effect on his interests; that his fresh and unacademic yet encyclopædic knowledge of the whole field of mathematical thought, and his vigorous intellect spiced with an ironic but genial humour, made him one of the most stimulating men of his day.

Mr. Jourdain was the youngest son of the late Rev. F. Jourdain, Vicar of Ashbourne, Derbyshire. He was educated at Cheltenham College and Trinity College, Cambridge. He held the Allen (University) Scholarship, and was a Fellow of the London Mathematical Society and a member of the Mathematical Association.

The main lines along which Mr. Jourdain's own contributions to thought were to develop are clearly traceable in one of his early articles in the *Monist* ("On Some Points in the Foundations of Mathematical Physics," vol. xviii, 1908). In this paper he attempts the exact formulation of certain fundamental questions of mathematical physics, such as causality, by the application of results he had previously reached in the mathematical theory of aggregates (*Journ. für Math.*, cxxviii., "On the General Theory of Functions"). Out of these considerations there arose the reciprocal interest in mathematical logic and in the philosophical foundations of science which is reflected in all his later work. That double interest explains how Mr. Jourdain, whilst an enthusiastic supporter of the "logisticians," could yet criticise Mr. Russell's scorn for the historical origins of mathematical ideas; and how, whilst approving of Poincaré's insistence on the synthetic and intuitional mode by which mathematical discovery advances, he could yet claim that the final result is part of a logical objective, untainted by its psychological origins. For as a logician he could see that "history is irrelevant to logic, that the truth or falsity of a proposition is independent of the way in which so-and-so discovered it"; and, as an acute observer of the origin and development of scientific conceptions, he could claim that it was "as great a mistake to banish from teaching a discussion of the growth of ideas as to try to build a house

without scaffolding on the ground that the scaffolding is not part of the building" (*Mind*, 25, 1916, p. 526).

In both his chosen fields of endeavour Mr. Jourdain had built up for himself a well-deserved reputation by his contributions to an exceptionally wide range of learned journals. His death came at a moment when he was at the height of his power: he had just claimed to have discovered a proof of the very first importance in mathematical logic, that any aggregate can be well-ordered; he was publishing in *Mind* important papers on "Causality, Induction and Probability"; and he was engaged on a monumental work on *The History of Mathematical Thought*. From what has been already fragmentarily published of this latter work, and from the mass of material found amongst his papers, it is clear that this was to be no mere anecdotage of mathematicians but a true history of the conceptions of mathematics.

But Mr. Jourdain's death was untimely also in a less personal, and therefore in what he himself would have regarded as a more important, sense. For he had been working for some time towards the realisation of two great schemes. The first was the foundation of a "Literary Entente" whereby the whole of the contributions of all nations to the advancement of science was to be translated into English and French; and the other was for the publication of what might stand for a national edition of Newton. He considered it to be nothing short of a disgrace that, while France and Italy had paid due respect to their great men in the national editions of Pascal and Galileo, the man whose genius was greater even than these should have been neglected. Mr. Jourdain was the greatest living authority on Newton; he had already done a vast amount of research himself, and it is hoped that some of this—together with his other unpublished work—will be issued posthumously.

In 1912 the late Dr. Paul Carus, whilst on a visit to the International Congress of Mathematicians at Cambridge, appointed Mr. Jourdain English Editor of the *Monist*. By this happy choice Dr. Carus was able to open up an extended sphere of influence for that Journal, and for the philosophical and scientific publications of the company of which he was the head. A few years later Mr. Jourdain became English Editor also of the *International Journal of Ethics*.

The series of valuable summaries of "Recent Advances in Mathematics," which Mr. Jourdain contributed to Science Progress, began in January 1916. The task of presenting a complete, and yet not too thin, abstract of the mass of new work—scattered over the widely diverse and specialised fields into which modern mathematics has split up—is one of ex-

treme difficulty. Mr. Jourdain's use of the classification adopted in the *Revue Semestrielle* enabled him to give to the expert at a glance the references likely to be useful to him; and at the same time it allowed him to indicate, by cross-references, what a more "popular" presentation could not have done—namely, the interconnectedness of all branches of mathematics, and the importance (even to technical mathematicians) of work on the foundations and development of mathematics. All those who have made use of this part of Mr. Jourdain's work will always owe him a debt of gratitude for its completeness and scholarly accuracy.

In addition to books on the work of other mathematicians, Mr. Jourdain published *The Nature of Mathematics*, 1912 (of which a new and revised edition has just been issued); *The Principle of Least Action*, 1913; and *The Philosophy of Mr. B*tr*nd R*ss*ll*, 1919.

Sir Oliver Lodge and Einstein's Theory (H. S. J.)

In a lecture to a small but distinguished gathering at Lord Glenconner's residence on November 24 last, Sir Oliver Lodge, after explaining the result obtained from the photographs taken by the recent Eclipse Expeditions, and showing that the observed deflection was twice that which would be anticipated, assuming light to possess inertia and Newton's law of gravitation to be rigidly true, proceeded to put forward some tentative hypotheses to account for the double displacement. Stated very briefly these were: (i) Light is a wave-motion, and the energy is half kinetic and half potential. It might be conceivable, therefore, that a light-wave would have only half the inertia but the whole weight when compared with a material particle. The deflection of a light-ray passing near the sun would then be double that which would be experienced by a material particle moving past the sun with the velocity of light. (ii) In a material body, the velocity of light is modified by interaction between the body and the æther. It is conceivable that the body may even modify the æther in some way for a certain distance outside it. Its specific inductive capacity, and hence its refractive index, would then be modified. This gravitational "aura" may only become appreciable outside a large mass, such as the sun. The deflection would then be a refraction effect. (iii) The deflection obtained from a distance r from the sun's centre may be put in the form $2v^2/c^2$, where v is the velocity in falling from infinity to the point r under the sun's gravitational field and c is the velocity of light. Light starts from infinity with velocity c, which cannot be increased. Gravitation is striving to increase the longitudinal energy, but is unable to do so. If it is supposed that by a sort of gyrostatic effect the additional energy $\frac{1}{2}mv^2$ is added in the transverse direction instead, it can easily be shown that the observed deflection is obtained.

Sir Oliver Lodge then proceeded to deal with some of the bearings of Einstein's theory, and explained how it revolutionised our conceptions of space and time.

Prof. Schuster, in proposing a vote of thanks, said that, as a student of the history of science, he thought that the safest way to deal with the new theory was not to seek for alternative explanations, but to adopt it and follow it to its logical

consequences, to modify it if necessary, and to abandon it should observation disprove it. He recalled the hostility aroused at first by Maxwell's electromagnetic theory of light, and mentioned how loth Lord Kelvin had been to accept it: he still continued to attempt to explain the æther by attributing to it properties similar to those of ordinary matter. As in that case, so now it must be left to the younger generation to discuss and analyse the new theory.

Einstein on his Theory (H. S. J.)

The *Times* of November 28 contains an article by Einstein on his theory. The following extracts contain the more salient points:

"The laws according to which material bodies are arranged in space do not exactly agree with the laws of space prescribed by the Euclidean geometry of solids. This is what is meant by the phrase 'a warp in space.' The fundamental concepts 'straight,' 'plane,' etc., accordingly lose their exact meaning in physics.

"In the generalised theory of relativity, the doctrine of space and time, kinematics, is no longer one of the absolute foundations of general physics. The geometrical states of bodies and the rates of clocks depend in the first place on their gravitational fields, which again are produced by the material systems concerned.

"Thus the new theory of gravitation diverges widely from that of Newton with respect to its basic principle. But in practical application the two agree so closely that it has been difficult to find cases in which the actual difference could be subjected to observation. . . .

"The great attraction of the theory is its logical consistency. If any deduction from it should prove untenable, it must be given up. A modification of it seems impossible without destruction of the whole."

The last paragraph is of particular importance as giving Einstein's own view as to the inter-relationship of the various predictions of the theory. In particular it indicates that, should the negative spectroscopic result be definitely established, the theory—in spite of its other successes—could not be maintained.

In conflict with this view, Sir Joseph Larmor, in a paper presented to the Royal Society on November 20, states that no spectral displacements should be expected. This paper is not yet published, and the writer has only seen the meeting abstract, so that the ground on which this conclusion is based cannot be examined.

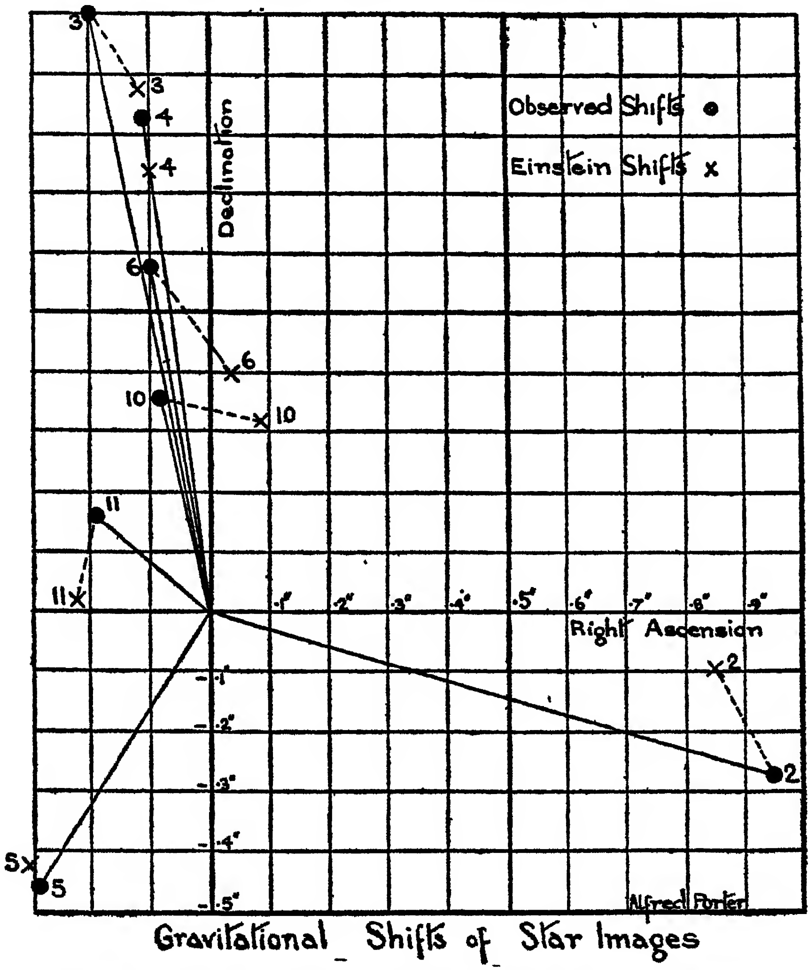
The Solar Eclipse Expeditions and their Results (G. B. J.)

A joint meeting of the Royal Society and the Royal Astronomical Society was held on November 6 to discuss the results of the observations obtained at the total eclipse of the sun on May 6, 1919.

It will be remembered that, according to Einstein's generalised theory of relativity, the light coming from a distant star is deflected inwards by the gravitational field of the sun by an amount which increases with the proximity of the light ray to the sun's centre. Another theory, which in effect regards light as corpuscles subject to the ordinary Newtonian laws, gives a similar result, but the deflection of the ray is only one-half that predicted by Einstein's theory. In order to decide between these rival theories two expeditions were sent out to observe the total eclipse of the sun on May 29, 1919; one to Sobral in North Brazil, in charge of Dr. Crommelin and Mr. Davidson, and the other

to Principe on the West Coast of Africa, in charge of Prof. Eddington and Mr. Cottingham. The method used was to photograph the field of stars near the sun during the eclipse, and to compare the photographs with others of the

The Bending of Light in passing near the Sun.



The above figure represents the results of the recent observations at Sobral. The lines drawn represent in direction and magnitude the resultant shifts observed in the apparent positions of seven stars in the neighbourhood of the sun. Lines drawn from the origin to the crosses represent the shifts calculated from Einstein's theory. The observed shifts are in every case greater than the calculated ones—on the average by about 14 per cent. The positions of the stars are *not* shown in the diagram.

same field taken with the same lenses in England some months previously. This eclipse was exceptionally favourable owing to the number of bright stars in the neighbourhood of the sun. In the case of the Sobral expedition it was possible to supply a further test by waiting until the sun had moved sufficiently

in the ecliptic to enable the same star-field to be photographed with the same instruments and mountings as were used during the eclipse. In spite of the unfavourable weather conditions at Principe, and some difficulties arising from the heating of the cœlostat mirror of the larger of the two instruments used at Sobral, satisfactory photographs were obtained at both stations. On measurement the plates provided conclusive evidence of a deflection of the star-images away from the centre of the sun's disc. When reduced to its value at the sun's limb the Sobral observations gave a mean deflection of 1·98″, while those at Principe, which, owing to the adverse weather conditions, were regarded as less conclusive, gave 1·64″. The value predicted by Einstein's theory is 1·75″, while the Newtonian theory gives ·875″. It was therefore concluded that the observations confirmed the prediction of the former theory.

After Sir F. W. Dyson, Prof. Eddington, and Dr. Crommelin had described the work of the expedition and the methods adopted for the reduction of the data, a discussion followed. Sir J. J. Thompson emphasised the importance of the results obtained, which should be considered in connection with the growing body of evidence in support of Einstein's theory. The deflection of the ray of light implied a change in the direction of the electric and magnetic vectors which, it seems, must be due to a new type of electric force.

Prof. Newall urged the necessity for caution in the interpretation of the established results of the observations, which might conceivably be due to other causes, as, for instance, refraction in a solar atmosphere.

Prof. Lindemann stated that the observed deflection, if due to refraction, required a density of the solar atmosphere which was inadmissible on other grounds.

Dr. Silberstein contended that, regarded as a consequence of Einstein's theory, the present problem was inseparable from another prediction of the same theory—that the Fraunhofer lines of the solar spectrum will be displaced towards the red. Hitherto observation has failed to establish any such displacement, and so long as this is the case we cannot regard the present results as establishing the basis of Einstein's theory.

Report on the Proceedings of Section A, British Association, Bournemouth, 1919. (Prof. A. O. Rankine, D.Sc.)

In spite of the absence of several eminent physicists who usually attend the meetings, there was much of interest and importance in the proceedings of this Section. A full programme occupied the four mornings from September 9-12, commencing with the Presidential Address. *Prof. A. Gray*, after paying graceful tributes to the memory of Carey Foster and Lord Rayleigh, criticised severely the conduct of the war in its scientific aspect by the Government. That they failed to utilise the special abilities of scientifically trained men from the Universities and Colleges, particularly in the early stages of hostilities, is notorious, and to this failure Prof. Gray attributed largely the protraction of the war. The remedy he suggests is the formation of a record of scientific graduates for special service, and he believes that this should still be done, notwithstanding the cessation of the recent war, in order that we may be prepared against eventualities.

Prof. Gray's next criticism was directed against the procedure with reference to inventions during the war. Inventions which seemed likely to be of practical importance often failed to be adopted by G.H.Q. because they were tested, and

possibly ruined, by officers who did not understand them. Delay, too, arose from the fact that the judges of outside inventions were frequently not unbiassed, for the reason that they themselves were technical officers interested in, and perhaps actually working on, the same projects. Prof. Gray would have a testing board of practical, well-qualified physicists and other experts, none of whom were inventors of appliances of the same kind as those submitted for their judgment. He viewed with alarm the lack of appreciation of scientific education still displayed by the authorities. Scientific eminence is almost a disqualification, and control, even of research, is in the hands of "good business men—the supermen of the present age." In this connection Prof. Gray welcomed the proposal to found an Institute of Physics, for he believes that reform lies to a great extent in the organisation of scientific men themselves. At the same time he uttered a warning against making organisation itself a fetish, and opposed ultra-centralisation. Another danger which he foresees is the emphasis which appears always to be laid on the industrial side of research. Prof. Gray pleaded for pure science as by far the most important thing, and asked that the scientific discoverer may be free, like Faraday, to follow where his researches lead. Then, in the long run, as in Faraday's case, the practical results will surpass all expectation.

The latter part of Prof. Gray's address dealt with the teaching of physics. He deprecated the tendency to regard dynamics, not as a part of physics, but as applied mathematics. At the same time he believes in the thorough study of mathematics, and regards as mischievous the attitude of ultra-simplicity exemplified by the statement of an eminent practical engineer that "all the calculus an engineering student requires could be learned in an hour or two." Prof. Gray also made brief reference to modern developments in molecular physics, particularly to the recent work of Sir E. Rutherford on the collisions of α-particles with atoms of small mass.

The Proceedings of the Section included an interesting discussion on *Thermionic Vacuum Tubes*, which was opened by *Prof. Eccles*, who gave a general description of the very varied uses of these tubes. The three-electrode thermionic vacuum tube has practically revolutionised wireless telegraphy during the war. Used for amplifying continuous wave-signals, magnifications of, perhaps, 100,000-fold are obtained, and transmission across the Atlantic is now possible with a receiving antenna consisting of a coil of wire only 3 or 4 ft. square. Tubes can also be used as producers of high-frequency alternating current, and can be made of sufficient size to become powerful wireless transmitters. The action of the tube depends on the electronic emission from hot bodies, and it is possible that it may have a great future in that we may be able to derive electricity direct from fuel, without machinery. Prof. Eccles described the construction of the tube, and pointed out that, used as an amplifier, a single tube will give a faithful reproduction in the proportion 20 : 1, while several, used in cascade, will give much greater ratios. There is almost complete absence of lag and of reaction on the original circuit. As a detector the tube, if nearly but not quite oscillating, readily responds to incoming signals of the same frequency, and is thus most efficient. Or, if oscillating with supersonic frequency, the stimulus of incoming signals of nearly the same frequency gives rise to a "beat" note which can, by adjustment, be rendered audible.

Prof. Fortescue drew attention to the lines upon which it might be hoped still further to improve the tubes. At present only 4½ per cent. of the power used in heating the filament is available. Filaments coated with lime are temporarily more efficient, but deteriorate. Thermal production of electrons is really

objectionable, because they are emitted with large random velocities. There is considerable trouble, also, arising from gases occluded in the various parts of the tube.

Mr. B. S. Gossling elaborated the theory of what happens in the tube, and mentioned the remarkable fact that, in spite of the many difficulties, it was now possible to rely on reproducing the tubes having efficiencies between plus and minus 10 per cent. of the mean.

Major Whiddington drew attention to the extraordinary sensitivity of the beat note in heterodyne reception to variations of inductance, capacity, and even of the resistance of the circuit. The beat note changes appreciably for a very small percentage change in the supersonic frequency. This renders possible a large number of physical measurements to a very high degree of accuracy. Such data as the permeability of a gas (by its effect on the inductance), or its specific inductive capacity (by its effect on the condenser) are now easily measurable. The device brings a very powerful new method into the field of physical research.

Sir O. Lodge expressed the opinion that the thermionic emission method of producing electrons, being of a random character, was probably bad. He asked if a radioactive electrode, or the photo-electric production of electrons, had ever been tried.

Dr. Makower replied that radioactive polonium could easily be used, but that, although probably more efficient than heat, the actual output would be exceedingly small.

Several papers on subjects related to thermionic tubes were also read. These were: "*On Self-Oscillation in Valves*," by *Major Whiddington*; "*The Production of Luminosity in Helium by Electron Collisions*," by *Prof. Horton* and *Miss D. Bailey*; and "*The Ionisation of Argon and Helium by Electron Collisions*," by *Prof. Horton* and *Miss A. C. Davies*. In the latter paper the measurement of the ionisation potentials was described, the values obtained corresponding approximately to Planck's quantum hypothesis of the production of the spectrum. Both *Dr. Goucher* and *Mr. Stead*, however, stated that in similar measurements they had found in helium *two* distinct ionisation potentials, the existence of which had not yet been accounted for satisfactorily.

The Report of the Committee on *Radiotelegraphic Investigations* was read by Prof. Eccles, who gave a preliminary statement on the results of the observations on the strengths of wireless signals during the recent solar eclipse. The results are not yet completely worked out, but it is already established that there was great strengthening of the signals attributable to the passage of the shadow between sending and receiving stations. Thus the eclipse simulated the effect of ordinary darkness, although it should be noted that the effect lasted several hours, indicating that the penumbral part of the shadow was almost as efficient as the umbra.

In connection with wireless telegraphy *Prof. G. N. Watson* read an important paper on "*The Diffraction of Electric Waves*." With the assumptions that the earth is a perfect conductor, and that it is surrounded by a dielectric of height h, followed by an imperfect conductor of conductivity σ, Prof. Watson calculates that theory agrees with experiment if $h^3\sigma = 1{\cdot}67 \times 10^{11}$, expressed in electrostatic units. Taking $h = 100$ kilometres, the estimated height of the ionised region of the atmosphere, the value of σ is about that of pure water.

A paper by *Major Vincent Smith* on "*Wireless Telegraphy during the War*," was also of considerable interest.

Preceding the discussion on *Relativity* on September 12, *Prof. Eddington*

exhibited some magnificent photographs taken by him and *Mr. E. E. Cottingham* at Principe during the total eclipse of the sun, May 29. The main purpose of the expedition was to make use of a splendid opportunity of testing whether light is subject to gravitation—*i.e.*, whether a ray of light passing near the limb of the sun suffers bending. There were thirteen bright stars suitably placed. Unfortunately at Principe, at the critical period, there were hazy clouds. Of sixteen photographs taken during the five minutes available, no stars appeared on the first ten. Towards the end of totality the sky cleared, and the images of five stars were obtained on the remaining plates. These will not be sufficient to test the effect of gravitation by comparing the positions with those in the absence of the sun. Fortunately, however, the expedition to Brazil had excellent conditions, and, although it is not yet possible to indicate the magnitude of the deflection, Prof. Eddington is convinced that some, in the correct sense, exists.[1] On the other hand, according to *Father Cortie*, who took part in the discussion, the fact remains that in the 1917 eclipse no shift was observed in forty-three stars appearing on the same plate.

The Principe photographs have an additional interest, actually enhanced by the intervening mist, in that they show a magnificent arched prominence in the corona of the sun, 100,000 miles high, and four times as broad.

In the *Discussion on Relativity*, opened by *Prof. Eddington*, *Dr. Silberstein*, *Sir O. Lodge*, and *Prof. Lindeman* took part. There were also related papers "*On the Limitations of Relativity*," by *Mr. W. J. Johnston* and *Sir J. Larmor*, and "*On the Æther and the Perihelion of Mercury*," by *Dr. Houstoun.* It is to be feared that the discussion did not advance matters much. It was a pity, too, that the eclipse calculations were not complete ; consequently it was not possible to use these results as a guide to the correct theory. Few people are able to follow Einstein, but there is no doubt that the main difficulty arises from the inability of many minds to dispense with what Sir O. Lodge called "an inexorable progress of time, independent of particular observers." Sir J. Larmor, too, thinks that it is wrong to regard time as constituting a fourth dimension. "Local time" is still the root of the difficulty, and the objection applies equally to the classical relativity of 1905 and Einstein's more recent development of it. Prof. Eddington outlined the Einstein view. Dr. Silberstein professed himself still a relativist of the 1905 school, and expected negative results from the eclipse observations. He claimed that relativity had recently obtained splendid verification, notably in Somerfield's adaptation to Bohr's theory of atoms. He was, however, as much opposed to Einstein's theory as to the non-relativistic view. Sir O. Lodge is still unconvinced. He answered Prof. Eddington's objection to the pretence of measuring lengths although no invariable scale exists, by pointing out that we can and do measure absolute temperatures by means of *real imperfect* gases, although there exist no *ideal* ones.

In addition to the Principe photographs there were two papers on astronomical subjects: "*On Nova Geminorum*," by *Lieut.-Col. Stratton*, and "*On Nova Aquilæ*," by *Father Cortie.* The behaviour of these two novæ—and, indeed, Nova Persei also—has been, apparently, very similar, and there is evidence of prodigious velocities in the materials constituting the outer layers. Shifts of absorption bands equivalent to velocities of 2,400 kilometres per second have been observed.

[1] By the time this report appears it is probable that the definite result will have been announced, enabling us, perhaps, to decide between the non-relativistic view which suggests a deflection of 0·87″, and the Einstein theory which anticipates double this amount.

Meteorology was not well represented at the meeting, but *Sir F. Stupart* gave an interesting description of the weather in Alberta (Canada), and pointed out the remarkable fact that within the last few years the mean January temperature has varied between $-15°$F. and $+26°$F.—an extraordinary range.

The *Seismology Committee* reported that, owing to Mrs. Milne's decision to return to Japan, the observatory at Shide had to be closed, and temporary accommodation has been secured at Oxford.

Two papers on pure mathematical subjects were read, one by *Prof. H. Hilton* on "*Certain Types of Plane Algebraic Curves*," the other by *Prof. A. R. Forsyth* "*On Gauss' Theorem for Quadrature and the Approximate Evaluation of Definite Integrals with Finite Limits.*" It was claimed that the latter paper may be of considerable use in estimating the areas of curves, for by suitably choosing the ordinates, instead of taking them equidistant, equal accuracy can be obtained with only about *half* the number of points.

Papers were also read by *Dr. Houstoun* on "*A Contribution to the Theory of Quanta*," and by *Dr. T. E. Stanton* on "*The Viscosity of Liquids at High Pressures.*"

An important contribution on *Spectra*, by *Dr. L. Silberstein*, will, fortunately, appear in full later in the *Philosophical Magazine*. Dr. Silberstein's theory is an extension of Bohr's, in that he supposes that the atomic nucleus is not a sphere, but unsymmetrical. In this way it is possible to account, not only for Balmer's spectral series, but also for the shifts and multiplications of lines in spectra which increased resolving power continues to reveal.

Sir O. Lodge propounded a *Theory of Vision* in which he supposed the black pigment of the retina, or the visual purple, to contain certain elements which are stimulated into electron emission by light of definite frequencies. A three-colour vision would require three distinct elements. Calculations on Bohr's theory showed that in heavy atoms electrons of the necessary natural frequencies existed. *Prof. Porter* pointed out that this was somewhat like the accepted photoelectric explanation of both vision and photography. If there were three types of electrons in the visual purple, their natural frequencies would need to be in the proportion 2 : 3 : 4 roughly.

The meetings of the Section terminated with a vote of thanks to the President, proposed by Sir O. Lodge, and seconded by Lord Rayleigh.

Experimental Farming (G. W. Harris, Salthams Farm, Chichester)

There is a marked activity at present on the part of various official bodies who have extemporised a life-long enthusiasm for agriculture, in spite of the ominous fact that the Government have declared up to now no agrarian policy. This omission is all the more serious when we note that County Councils are being empowered and urged to purchase farms for small holdings and for experimental purposes.

It would seem that before many thousands are spent in this way (the cost of some farms being so great that nothing under £4 per acre will produce reasonable interest on the money), the first requisite is a Government policy. It is quite possible that the Government have realised that some thought is necessary before making an announcement, and that there are a few difficulties to be surmounted. Hence the delay.

The first question refers to the small holding. As we are not concerned in this article with this part of the problem, we need only say that it has not yet been

established that the small holding is economically a success, and that all the evidence points the other way.

The purchase of farms for experimental purposes requires even more careful thought. The use of the epithet "scientific" in connection with agriculture has not been very happy. For the ordinary person it seems to acquire a mysterious meaning, and men are ready to welcome a return to the age of miracles in agriculture which they would indignantly repudiate in their own particular profession. Huxley's opinion of what science is, and what the term "scientific" must connote, seems to have been forgotten—perhaps it was never known. "Science is, I believe, nothing but trained and organised commonsense." This is eminently true in agriculture.

With what object and on what lines are these experimental farms to be run? Who is to control them, and what qualification is necessary for the officials who will be responsible for these farms?

It must be stated at the outset that, unless the Government have a definite agrarian policy, the multiplication of experimental farms will not only be a futile but also a criminal waste of public money. Assuming that there is a policy, and that it will shortly be announced, the most important problem is the selection of the staff of these farms, and the precise determination of their sphere of activity. At the present time the control of agriculture is in the hands of those who know but little about it. The composition of the Board of Agriculture is not above suspicion: some of its executive were, or are, land agents merely; few, if any, have had to make a living out of farming. Among agricultural scientists the percentage of those who have had any practical connection with farming is clearly exiguous. Thus, those who have, or wish to have, control in agriculture are at present slightly handicapped by ignorance.

Sir A. D. Hall appreciates this want of continuity between farming practice and research. He writes on page 6 of his preface to *Soil* (1912 edition): "But even if the best farming practice is still a step beyond its complete explanation by science, yet the most practical man will find his perception stimulated, and his power of dealing with an emergency quickened, by an appreciation of the reasons underlying the tradition in which he has been trained."

While expressing our doubt as to the validity of our author's argument we would emphasise the fact that it is precisely the best farming practice that should be the object of investigation and of attainment for the experimental farm.

But the farmer is too much concerned with making a living, and the scientist has too little regard for economic questions for this consummation to be realised. Prof. L. P. Jacks's *Farmer Jeremy* may be taken as a true and accurate picture of the best farming practice. The best farmers very rarely divulge their knowledge, and still more rarely hand it on. What their experience has taught them, and the methods they have employed, are known only to themselves, and the scientist who starts out on stilts is hardly likely to succeed in extracting that information which his own experience cannot supply. As an example, we may mention the case of a farmer who is highly successful in his business, and comes of a long line of farmers. He found that no matter what treatment he gave before or after, a crop of potatoes was invariably followed by bad crops, and that this was noticeable even in the third following crop. He, therefore, satisfied with the fact, gave up growing potatoes on this land (which is a brick-earth, and very good land indeed). Where the farmer left off the scientist might begin; but he is usually lacking in experience, and rarely comes up against these practical problems. There are many cases like this where the farmer finds certain results in practice, and has not the time to concern himself with the explanation thereof.

It is essential that some means should be found by which the scientific worker can meet the practical farmer. It is hardly to be imagined that any one but a government official would advocate a system of experimental farms where the subjects for experiment are evolved out of the official inner consciousness.

Nor can it be said that the present position of science in agriculture is notably satisfactory. The problems connected with the destruction of various fungoid and insect pests have not yet approached solution. A book on this subject by Mr. F. R. Petherbridge, published in 1916 (C.U.P.), is very melancholy reading. Any one who cares to read the various paragraphs entitled "Remedial Measures" cannot fail to be struck by the entirely inadequate treatment advocated in most cases. For anything really definite and conclusive, save in very few cases, the inquirer may search in vain. It is quite true that under the ægis of the Board of Agriculture a number of instructional leaflets are published, but the Board of Agriculture has been so conspicuous in its failures in other directions that, logically or otherwise, it has ceased to be above suspicion.

Consider the question of milk-testing. The present state of affairs is almost a burlesque. The local magistrates convict, the farmer appeals, and the conviction is quashed if it is proved that the milk is as it came from the cow. Who was originally responsible for this test? What other work has been done subsequently to cause this modification? If, for example, no better method has been followed than was the case with Thury in 1863, who based his conclusions with regard to the determination of sex on experiments with twenty-nine cows, little wonder that there are continual exceptions in the matter of a fat standard.

Again, fertilisers have been the subject of much experiment. The results are published, and can be obtained, by those who wish to see them, cheaply enough. Agricultural journalists in their answers to correspondents are lavish in their recommendation of fertilisers; but who will pretend that there is as yet any certainty that a fertiliser will behave in the way it is supposed to do? Plenty of farmers will say that they have been unable to notice the slightest difference; and although the agricultural journalist doubtless performs the useful function of disseminating such scientific knowledge as he can lay hold of, the procedure seems to savour of the quack medicine purveyor who has no personal knowledge of the victims whom he treats.

The working of the land is a subject that should afford ample scope for investigation. If as good results are sometimes obtained when the land is very indifferently worked as when there is a really good tilth, is it not possible that the farmer spends time and trouble all to no purpose? Has deep ploughing any advantage over shallow ploughing, or must the decision be governed entirely by local conditions?

Much has been said during the war about baby beef. Have sufficient experiments been conducted to elucidate this question?

It is easy to multiply examples, but we would mention in conclusion the case of silage. What has been done in England on this subject? The information on the feeding value of silage is usually derived from Kellner or some derivative of Kellner. Where ignorance is so widespread, except perhaps in the case of Mr. Wibberly, who expostulates convincingly thereon, how is the farmer to be expected to take up a thing which may result in a complete loss? Has our science any light to throw on the subject?

Lastly, the economic side of farming can never be too much emphasised where experimental farms are contemplated. The purists in agriculture are little better than their teetotal brethren. Farming is a means of livelihood, and if agricultural

scientists wish to assist the farmer (they seem to have no other *raison d'être*) they must publish accurate accounts. It is not necessary that the experiments should be profitable. All that is required is to show exactly what the cost has been, and the farmer can draw his own conclusions. Wherever these experimental stations are set up the best farmers in the district should be consulted, and the direction of affairs should not be left in the hands of an irresponsible minority who happen to enjoy the temporary authority and splendour attached to the membership of a County Council. It is curious to notice that where democracy is a catchword on every one's lips, there, and there more than anywhere else, exists the unbridled tyranny of self-elected minorities.

The Origin of Life: The Work of the late Dr. Charlton Bastian, F.R.S. (Surgeon-Commander W. Bastian, R.N.)

The question of the Origin of Life is one of never-failing interest, and many and varied are the speculations and theories that have been indulged in on this subject by scientific men and by others not so scientific during past ages. It may be said, however, that with one exception very little actual experimental work has been done with a view to seeing whether it is or is not possible to bring about a genesis of living matter by laboratory experiments.

Whilst admitting that there must have been a *de novo* origin of living matter at some time in the remote geological past, most scientific men of to-day pin their faith to the doctrine "Omne vivum ex vivo." They believe with Huxley that that doctrine is, as he said, "victorious along the whole line" (British Association Presidential Address, 1870).

The question then clearly arises whether a life-giving process apart from any supernatural or miraculous agency occurred only once, or, at all events, only in the very early days of the earth's history, or whether it is one that has ever been taking place since the period when it first began.

Mere speculations and theories do not seem to lead us any further, so that to get any idea of the real truth of this matter Nature has to be questioned by the institution of proper experiments.

My father, the late Dr. Charlton Bastian, F.R.S., believed that living matter is constantly coming into being in suitable media and surroundings, or, in other words, in Nature's laboratory, by a process of what he termed "archebiosis,' previously described by Huxley as "abiogenesis." His views were based partly on theoretical grounds, but largely on the basis of his own extensive series of experiments performed during the ten or fifteen years preceding his death, and previously, with certain colloidal saline solutions enclosed within hermetically-sealed and sterilised glass tubes. He claimed, by these means, to have brought about the *de novo* origin of definite well-known living organisms, which grew and which reproduced themselves, and could be cultivated in suitable nutrient media.

Dr. Bastian died in November 1915. In May of this year a letter appeared in the *Lancet* from me, calling attention to the fact that my father's views had met with very little other than unsympathetic and hostile criticism, and that his alleged facts had never been actually and finally disproved by experimental research.

The writer of the letter was well aware that somewhat similar experiments had been performed by others, but that these having produced nothing but negative results it seemed clear that either Dr. Bastian throughout his long life was totally in error as regards his facts and conclusions, or that there might possibly be some

flaw in the work and methods of experiment of those who sought to prove finally whether in fact Dr. Bastian was right or wrong.

Several most interesting letters followed in the *Lancet*, but only a few from persons who had themselves done any experimental work on the subject, on the same lines as those of Dr. Bastian. Two of these had confirmed some of Dr. Bastian's experiments—namely, Albert Mary, of Paris, and Dr. O. C. Gruner, of Leeds—the latter only in regard to Dr. Bastian's work on heterogenesis an allied subject.

Two other observers—namely, Dr. Sydney G. Paine and the Hon. H. Onslow—declared that their experiments clearly indicated that in Dr. Bastian's experiments there was no evolution of life from inorganic materials, and they refer the readers of the *Lancet* to their published reports of their own experiments, wherein they explain their reasons for thinking that Dr. Bastian's facts, and the necessary conclusions arising therefrom, were erroneous.

There, it would seem, the matter has been allowed to rest, though it would appear desirable to institute some further experiments on the exact lines as described in Dr. Bastian's book, *The Origin of Life* (Watts & Co., 2nd edition, 1913, price 3*s*. 6*d*.).

It has always been urged by Dr. Bastian's opponents in this controversy that his experiments are invalidated for the following reasons :

Some critics—mostly not biologists, and presumably not thoroughly acquainted with bacteria and torulæ, have supposed that what were taken from Dr. Bastian's tubes were mere pseudo-organisms such as Leduc, Herrera, Jules Félix, the brothers Mary, and in this country Prof. Benjamin Moore, F.R.S., and Dr. Sydney G. Paine are able to produce from various saline solutions—bodies which undoubtedly simulate organic forms with great exactness. Others, as, for instance, Prof. Hewlett and Dr. Jonathan Wright, of New York, have expressed themselves as quite satisfied that the bodies found and photographed have been real organisms, but cannot believe that they have really been engendered within the tubes ; nor can they believe that they are alive. They presume that they must have been introduced in some way during the sterilising process—which, of course, is quite possible, if not probable. But these organisms would have been killed by the sterilising process, and could not, therefore, go on reproducing themselves as they have been found to do in Bastian's experiments—nor would they have been found in such enormous numbers as was often the case. In this connection see Dr. Jonathan Wright's letter in the *Lancet* of August 2, 1919, and Prof. Hewlett's Introduction to Dr. Bastian's paper in *Nature* of January 22, 1914, p. 579.

The Fiftieth Year of Sir Ray Lankester's Editorship of the "Quarterly Journal of Microscopical Science" (J. B. G.)

The 253rd number of the *Quarterly Journal of Microscopical Science* marks the fiftieth year of Sir Ray Lankester's editorship. During this period the *Quarterly Journal* has included among its pages many important memoirs, written by authors not only of this country, but from abroad. Sir Ray Lankester himself has led the van as a writer of very valuable papers, which have stimulated biological research in several important branches. Without noticing the not unprominent part which Oxford zoologists have taken in contributing to the high standard of this valuable periodical, one will observe that Sir Ray Lankester has succeeded in attracting contributors from every University in the country.

Those younger men who have had the privilege of publishing their first papers

with Sir Ray Lankester's journal, will have noted how kindly he advised and criticised, and with what ease he commanded many branches of biology.

We congratulate Sir Ray Lankester on his splendid work, and we express the hope that he may continue for many years longer as editor of the *Quarterly Journal*.

Gustav Retzius (J. B. G.)

Recently we noted with regret the death of Professor Gustav Retzius, the distinguished Swedish biologist. Retzius was well known for his studies on many biological subjects of a widely different nature. He personally published a journal known as the *Biologische Untersuchungen*, and apparently no cost was spared in the production of this work. While Retzius observed many natural objects, it cannot be said that his knowledge of them was very deep; his work on the brain, and on gametogenesis, are examples of this. His best labours were in the preparation of materials which were beautifully drawn by his artists, and then shortly described by Retzius himself.

Gustav Retzius and his wife were extremely hospitable and charming people, and his death will be lamented by many friends, and by admirers of his work.

Awards for Medical Discovery

The Conjoint Committee of the British Science Guild and the British Medical Association, to which we referred in our last issue, met on November 4, Sir Alfred Keogh taking the Chair. The Committee decided to call itself the Conjoint Committee on Awards for Medical Discovery, and issued various letters asking for further information. It also decided to consider recent medical discoveries which merit public recognition, and Colonel Sir Ronald Ross was asked to draw up a Report on the subject containing information given by the various members.

Readers of SCIENCE PROGRESS will remember that just before the war Sir Ronald Ross submitted a Petition to Parliament asking for compensation for losses on professional emoluments due to continuous work for many years on the subject of malaria. Such petitions have to pass through the Chancellor of the Exchequer and are always held up at first. He took no further action in the matter during the war, but is now preparing to submit the Petition again. If accepted, it is likely to have the widest influence on medical investigation in the future. The Petition was framed on the precedent of Edward Jenner, who was awarded similar compensation by Parliament early in last century.

Real-Politics

The world has heard a great deal of various kinds of politics. For example, there is World-Politics—which has recently cost the lives of about seven million men. Then there is Class-Politics—which has ruined the great country which used to be called Russia. Thirdly, there is Party-Politics—which, by its mismanagement of the early part of the war has created a debt of about eight thousand million pounds for this country. But, in addition, there is a form of politics which may be called Real-Politics—a purely scientific kind of politics, the object of which is to organise prosperity. Nobody thinks of this poor Cinderella, except a few men here and there. Lord Leverhulme is one of these,

and he has attempted to organise prosperity for those two islands owned by him and called Lewis and Harris. It appears that the inhabitants have long been under the impression that they would become perfectly happy if each of them were to be given a small croft with a cottage and a few cows. But Lord Leverhulme, who is perhaps the most able and one of the richest business men in Britain, has come to the conclusion, after scientific thought, that such crofts would really not pay; and so we suppose. Instead of such a programme, he wishes the people of these islands to develop the magnificent harvest of fish which remains, so to speak, uncut at their doors; and he asks for ten years in which to develop the business. Of course the people affected were surprised at first. Any new thing appeals to human beings much in the same way as would a demonstration of the binomial theorem to a herd of cows in a field, but after a time they begin to understand; and we hope and believe that Lord Leverhulme will ultimately succeed. If we talked less about our rights, our privileges, our constitutional powers and the various methods of government, and really tried to think out what we should do to improve our happiness in the world, we should be wise—wiser perhaps than human beings can be.

Shide Hill House

In the summer of 1895 Prof. Milne returned to England after his twenty years' residence in Japan. The seismographs which he brought with him, improved and added to in the following years, were erected at Shide Hill House, near Newport, Isle of Wight, which then became the centre of the operations of the British Association Seismological Committee, and the most important and well-known earthquake observatory in the world. After Prof. Milne's death six years ago (see SCIENCE PROGRESS, April 1914, pp. 713–20), the house became the property of his widow, who is a native of Japan; but the instruments were allowed to remain there and the work with them was carried on as before under the direction of Prof. H. H. Turner, of Oxford, the chairman of the Seismological Committee. Owing to Mrs. Milne's decision to return to Japan, the house was sold by auction last autumn, and the seismographs have been transferred to Oxford, where they will be under Prof. Turner's immediate care. It is satisfactory to learn that, except for the brief interruption of the records, there will be no actual loss caused by the change of station.

Toothbrushes—a Warning

The great prevalence of decayed teeth among the Anglo-Saxon peoples, and, to a less extent, among other white races, is notorious, and has been ascribed to many causes, but, in my opinion, without sufficient consideration. White wheat flour seems to be the most popular ascription in the medical press, such flour being specially favourable to the growth of organisms in dental caries. How, then, we ask, do races which live on white flour, such as several peoples of northern India, possess such beautiful teeth? The eating of soft, cooked food is another alleged cause; why, then, do not the people of southern India, who live almost entirely on soft-boiled rice, suffer as we do? National degeneration is yet another supposed cause—which must be dismissed in view of the fact that physically and mentally we are certainly stronger than many races and tribes whose teeth remain almost perfect until old age—and the same may be said of

sugar, alcohol, and toothpicks, which are used not only by peoples with bad teeth. Moreover, the teeth of the British race were not always so much affected as at present—witness the evidence of old skulls. Thus, some years ago, a large heap of skulls, dating from the sixteenth century, was found in London, and the writer was assured by an anatomist who examined the bones that the teeth were little affected. Apparently, then, the cause, or, at least, the principal cause, of dental caries should be something which has been introduced only recently. Why not (for a working hypothesis) the toothbrush?—which is chiefly employed by the Anglo-Saxons, less by some other white races, and not at all by most Africans, Indians, etc.—who clean their teeth by rubbing them with chalk or dust disposed on the forefinger, or with bits of green and soft stick.

The following case is known to the writer. A man, Mr. X, has used toothbrushes all his life, and began to lose his teeth in childhood; but all his front teeth remained perfectly sound until at the age of sixty he commenced to use a hard brush. At that age the gums retract, leaving a small surface of soft bone between the gum and the enamelled surfaces of the teeth; and in a few months three of his incisors, namely, the three on the left side, specially liable to the friction of the brush, decayed simultaneously, the decay commencing in the soft bone just where the brush was likely to abrade it.

Of course, there is a large literature on dental caries; but the writer has not seen the toothbrush accused in it. It is an unnatural instrument, likely to damage the teeth, and not very effective for cleaning the interstices. When was it introduced? We read in Agnes Strickland's *Life of Queen Elizabeth* (reprinted by J. M. Dent & Sons, p. 380) that "Mistress Twist, the Court Laundress, presented Her Majesty with four 'tooth-cloths' of coarse Holland, wrought with black silk, and edged with lace, apparently for cleaning the teeth!" Hence brushes were probably not used at that time.

The following is possibly a sufficient process for the toilet. The gums and teeth are massaged with the forefinger, on which a little Vinolia tooth-paste or similar substance has been smeared, and the teeth are then rubbed thoroughly but lightly with dry camphorated-chalk powder taken up on the moistened pulp of the middle finger, the mouth being well rinsed, of course, with that cheap disinfectant, water.

Whatever may be the cause of caries, the discovery of the cause of it would be an enormous boon to humanity. It is to be hoped that our new Ministry of Health will continue and extend the investigation of the subject.—R. R.

Astronomical Divinity

Mr. R. G. Durrant tells the following amusing story in the *Schoo Science Review* for October:

There are more things in heaven and earth than are dreamed of in our philosophy. One evening, a junior master living "in-college" was visited by an eminent divine, parent of one of the boys. E. D.: "I understand, sir, that you are president of the school Astronomical Society, and I have come to ask your opinion on a point in astronomy. I understand that the earth moves round the sun in an elliptical orbit." J. M.: "That is so." E. D.: "I am told that an ellipse has two foci, the sun situated at one of them, and that at the other, the 'blind focus,' there is no physical matter." J. M.: "That is the case." E. D.: "I am writing a theological treatise, and I have thought it most probable

that this so-called 'blind focus' is, in reality, the seat of the Almighty. Thus, the sun from the physical, and He from the spiritual focus, govern the universe." J. M.: "Ahem! it may be so; but I think, sir, it might be unwise to publish this when we consider how very small is the whole solar system in comparison with the starry heavens, and He made the stars also." E. D.: "Well, I will consider the matter further. Good-night to you."

Notes and News

The King has been pleased to approve of the following awards this year by the President and Council of the Royal Society:

A Royal Medal to Prof. J. B. Farmer, F.R.S., for his notable work on plant and animal cytology.

A Royal Medal to Mr. J. H. Jeans, F.R.S., for his researches in applied mathematics.

The following awards have also been made by the President and Council:

The Copley Medal to Prof. W. M. Bayliss, F.R.S., for his contributions to general physiology and to bio-physics.

The Davy Medal to Prof. P. F. Frankland, F.R.S., for his distinguished work in chemistry, especially that on optical activity and on fermentation.

The Sylvester Medal to Major P. A. MacMahon for his researches in pure mathematics, especially in connection with the partition of numbers and analysis.

The Hughes Medal to Dr. C. Chree, F.R.S., for his researches on terrestrial magnetism.

Sir J. J. Thomson has been recommended by the Council for election as President during the year 1920, Sir David Prain as Treasurer, and Mr. W. B. Hardy and Mr. J. H. Jeans as Secretaries.

The Prince of Wales has consented to become a Fellow of the Royal Society and an honorary member of the Iron and Steel Institute.

Prof. Theodore W. Richards, of Harvard University, has been elected President of the American Academy of Arts and Sciences.

Prof. A. Fowler, F.R.S., has been awarded a gold medal by the National Academy of Sciences, Washington, in recognition of his contributions to astronomical science.

Dr. S. Russ has been elected President of the Röntgen Society for the session 1919-20.

Sir Henry Miers, Vice-Chancellor of the University of Manchester, has been appointed to be a member of the Advisory Council to the Committee of the Privy Council for Scientific and Industrial Research.

Prof. J. E. Petavel, F.R.S., has been appointed Director of the National Physical Laboratory in succession to Sir R. T. Glazebrook, who retired on September 18 last.

Dr. Pickard, F.R.S., Principal of the Blackburn Technical School, has been appointed Principal of the Battersea Polytechnic.

Dr. Edward Hindle, Assistant to the Quick Professor of Biology, Cambridge, has been elected to the Chair of Biology in the School of Medicine, Cairo, Egypt.

Prof. E. L. Nichols retired from his post as Head of the Physics Department at Cornell University last June. He is succeeded by Prof. E. Merritt.

We have noted with regret the announcement of the death of the following well-known workers in science during the last quarter: Dr. John Aitken, F.R.S.; Mr. S. D. Chalmers, of the Northampton Institute; Dr. A. G. Vernon Harcourt,

F.R S.; Dr. H. C. Greenwood. lately of the Munitions Invention Department; Dr. P. E. B. Jourdain; Prof. A. Macalister, F.R.S., Professor of Anatomy at the University of Cambridge; Dr. Charles A. Mercier; Prof. Trail, Regius Professor of Botany in the University of Aberdeen.

On September 18 last the Aero Club of America observed officially a flight by Mr. Roland Rohlfs to an altitude of 34,610 ft., the previous record being 30,500 ft. The flight was made on a 400 H.P. Curtiss triplane.

The Geological Survey of Great Britain and Museum of Practical Geology, Jermyn Street, was transferred for administrative purposes from the Board of Education to the Department of Scientific and Industrial Research on November 1 last. Correspondence with reference to the work of the Survey should, however, be addressed as heretofore to the Director of the Survey and Museum, Jermyn Street, S.W.

The Conjoint Board of Scientific Societies has issued a useful pamphlet containing a Calendar of Scientific Meetings for the session 1919-20 (i.e., to June 30). Copies may be obtained from the Secretary at Burlington House, price *6d.* The booklet also gives a list of the constituent societies, with their addresses.

Mr. Austen Chamberlain estimates our future normal yearly expenditure as £808,000,000, of which £135,000,000 will be spent on the fighting forces, and £400,000 on scientific investigation and research.

During the past quarter unusually large donations and bequests have been given to scientific and educational institutions outside Great Britain—chiefly, of course, in the United States. Mr. John D. Rockefeller heads the list with another $20,000,000, which the General Education Board is to devote to the improvement of medical education in the States. The entire sum, principal and interest, is to be distributed in fifty years. The John Sterling estate, bequeathed to Yale, as already reported, has realised $18,000,000 and not $15,000,000 as first estimated. Mr. E. F. Searles has given $1,500,000 to the University of California; Barnard and Columbia each receive £284,000 from the estate of the late General Horace W. Carpentier; Smith and Mount Holyoke Colleges $500,000 under the terms of the will of Mr. Charles N. Clark; and Cornell $250,000 from Mr. B. Lœwy. From Australia comes the news that the University of Sydney receives £460,000 and Brisbane £230,000 from the estate of the late Sir Samuel McCaughey. We should not have to mention the English Universities at all were it not for a munificent gift of £30,000 from Mr. and Mrs. Molteno to Cambridge for building and endowing an Institute of Parasitological Research, and a bequest from the late Mrs. M. L. Medley, of Beaworthy, Devon, of £20,000 to the University of Oxford, to establish a scholarship for the promotion of the study of political economy in memory of her husband, George Webb Medley.

During the last few months the Ramsay Memorial Fund has continued to make progress, and the total contributions now amount to a little over £50,000, apart from the funds to be provided by Foreign Governments for the foundation of Fellowships.

The first Foreign Government to conclude arrangements for the foundation of a Ramsay Fellowship is the Royal Hellenic Government. The first Fellow, Dr. V. Papaconstantinou, formerly Assistant Professor to Professor Zenghelis, of the University of Athens, has been duly nominated by the Royal Hellenic Government, and appointed by the Trustees, and has taken up work in this country. He is working under Prof. F. G. Donnan, at University College, London. It is anticipated that further appointments of Foreign Fellows will be made at an early date.

A branch of the Ramsay Memorial Fund has been started in France under the Presidency of Mr. Lloyd George. The Committee include leading French scientists and politicians. A sum of 43,500 francs has already been collected. The French Committee are aiming at founding one or more Fellowships to enable French chemists to pursue research work in one of the British Universities. The appeal in France is being directed specially to the British and American Colonies, who are being asked to contribute to the fund as a testimony of their appreciation of the wonderful scientific achievements of our ally, France, during the war.

The details of several new expeditions have been reported in the Press. Mr. D. B. MacMillan is leading a small party to the Arctic regions to carry out two to three years' work for the National Geographic Society of the United States of America. The expedition has been organised by the alumni of Bowdoin College. The Duke of Abruzzi intends to explore the upper reaches of the Wady Scebel, a stream running into the Fafan River from the outlying spurs of the Abyssinian mountain ranges in north Italian Somaliland. Mr. E. Heller is leading an expedition to collect African animals, plants, etc., for the Smithsonian Institute, especially those of Central and South African origin.

In *Science* (August 8, 1919) Prof. Millikan announces that he has succeeded in extending the known ultra-violet spectrum to wave-lengths as short as 320 Ångstrom units. It will be remembered that, by using a vacuum spectroscope, Schumann extended the spectrum from 1,850 Å.U. to 1,220 Å.U., and that Lyman afterwards worked down to 510 Å.U. Millikan's further success has been obtained by running a zinc arc in a chamber in which the pressure is kept down to 10^{-4} mm. of mercury by means of a new and very efficient diffusion pump. At this low pressure it is possible to use very high potentials (up to several hundred thousand volts) to produce the sparks without obtaining any glow discharge.

Sir Charles Parsons described several very interesting war-time inventions in his presidential address to the British Association last September. Among them was the Leader-Gear used, first by Germany and later by the Allies, to guide their ships through their own mine-fields. An alternating current is passed through an insulated cable laid at the bottom of the sea, and earthed at its further end. Ships, furnished with suitable instruments, are able to follow the track of the cable with great precision at any speed. Cables of as great a length as 50 miles have been used, and the application of the system to assist in the navigation of narrow channels, and as a guide in fogs, should be of very great value.

In his essay on "Adaptation and Adaptability" in the *Eugenics Review* (October 1919) Prof. Doncaster mentions some recent work on the effects of alcohol on the offspring of the guinea-pig and of the fowl. In the former, even when the male parent *alone* has been given a prolonged course of small doses of alcohol, the offspring are defective, and so, too, are the great-grandchildren of the alcoholised parents. On the other hand, Pearl finds in the fowl that fertility is reduced by alcohol, but that the eggs which are fertile produce stronger and larger chicks. In explanation of this apparent contradiction it is considered that, while in the guinea-pig the germ-cells which produce the eggs and sperms may be injured and still develop, in the fowl the susceptible germ-cells are rendered incapable of development, so that only the stronger ones survive.

In the same number of the *Review* Dr. H. H. O'Farrell discusses the statement, first made by Sir Archibald Alison in his *History of Europe*, that, as a result of the Napoleonic wars, the average height of the French population lessened by one or more inches. It is shown quite conclusively that the statement is incorrect.

In a short note to the *Library Association Record* Mr. F. W. Clifford gives an account of the scheme for the foundation of a representative Library of Chemical Literature which has been rendered possible by the co-operation of the Chemical Society with a number of other societies interested in chemistry - namely, the Association of British Chemical Manufacturers, the Biochemical Society, Faraday Society, Institute of Chemistry, Society of Chemical Industry, Society of Dyers and Colourists, Society of Public Analysts. These societies contribute towards the cost of the scheme, and their members have full use of the library both for loan and reference. The generous policy of the Council of the Chemical Society towards their library in pre-war days resulted in a collection of books dealing not merely with the purely scientific aspect of chemistry, but also with industrial processes. As a consequence, many important works of foreign origin were to be found on their shelves which were of the greatest possible value to the various War Departments and manufacturers who were striving to make essential products which had always before been purchased from Germany.

The Lancashire and Cheshire Coal Research Association has issued its first two *Bulletins*. No. 1 contains notes of ten Introductory Lectures on Organic Chemistry, with special reference to coal, and No. 2 deals with the Sampling of Coal. Both are extremely clear and concise, and Captain F. S. Sinnatt, the Director of Research, is to be congratulated on the manner in which his Association is making its work public. It is a pity, however, that the pages of these little pamphlets are of such an unusual size. It is generally agreed that we should aim at standardisation in such matters, and while very large pages may sometimes be desirable, those at the other extreme are not.

The *Radio Review* (Wireless Press, 2*s.* 6*d.*) is the name of a new monthly journal whose sole aim is to record the scientific developments of radiotelegraphy and telephony, and of related branches of applied science. During the war period a vast amount of research has been carried out which is directly or indirectly connected with the subject, and the three-electrode thermionic vacuum-tube has produced a complete revolution in wireless practice. It has become imperative for the research worker that information hitherto scattered through many periodicals should be gathered together in a single one; and it is believed that the number of people interested in this branch of science has become sufficiently large to give the journal every prospect of success. The fact that Prof. G. W. O. Howe has consented to act as editor serves as a sufficient guarantee of the scientific character of the journal. The first number contains, among other articles, the first portion of a paper by Prof. Eccles on the mathematical theory of the Triode valve; an excellent account of the Proceedings of the British Association at Bournemouth which were concerned with wireless work; and a valuable collection of abstracts of the latest radio literature (including patents), which, from their excellence, we suspect to be the work of Mr. P. R. Coursey, the assistant editor.

Major-General George O. Squier, the chief Signals Officer of the U.S. Army, has given some account of the work done during the war on the use of trees as antennæ in wireless practice in the *Journal of the Franklin Institute* (June 1919). He does not explain details very clearly, and avoids giving diagrams of connections, but it would appear that tree-contact should be made by driving a copper nail well into the tree (e.g., 3 in.), while the earth should consist "practically of several short pieces of insulated wire sealed at the outer ends, radiating from a common centre, and buried a few inches beneath the surface of the ground in the neighbourhood of the tree." So complicated an earth is not, however, essential,

for, by using modern amplifiers, it was possible to obtain near Washington signals from the principal European stations by using a small wire net on the ground beneath the tree. A most important fact is that a tree can be used for widely different wave-lengths if the appropriate receiving devices are connected to the tree terminal. General Squiers suggests that the "metallic electrode rigidly driven into the living organism of a tree" may serve, in conjunction with the thermionic valve-tube, as a powerful method for the detailed study of electrical disturbances in the atmosphere and the earth.

Mr. D. Brownlie (of Brownlie & Green, Austin Friars, E.C.) has published another valuable paper on the efficiency of boiler plants, this time dealing with the economy in coal which could be effected by modernising all colliery steam plants. Collieries are by far the worst coal-wasters because, in pre-war days, coal was so cheap as to be hardly worth saving. In some pit-heads a few years ago high-class coal was sold at 5*s.* per ton—it has even been given away to release railway wagons! Now that it costs 25*s.* a ton even colliery managers may become interested in economy. Mr. Brownlie gives details of tests he has actually made on 100 colliery-boiler plants which he considers typical of all those in the country, and finds that their average thermal efficiency is only 55·5 per cent. (the extremes being 71·8 per cent. and 32·5 per cent.) as against an average of 60 per cent. for the whole of the plants used in Great Britain. It is contended that an average efficiency of 70 per cent. could easily be obtained, and, since the collieries consume 18,500,000 tons of coal each year, this would save nearly 4,000,000 tons. Further, if more use were made of refuse coal, a total saving of 6,600,000 tons per annum could be effected, to say nothing of what could be achieved by stopping the enormous waste of exhaust steam, and by using a modern system of steam-pipe lagging.

The Department of Scientific and Industrial Research[1] seems now to have become a firmly-established Government Office, and its Annual Report has become so comprehensive that, in these notes, it will only be possible to deal briefly with some of its more important aspects. Of these not the least important is that the administration of the Geological Survey and Museum is to be transferred to the Department from the Board of Education. The Fuel Research Laboratory is open, and the Main Station (at Greenwich) should by now be completed. The Fuel Research Board is to undertake, in addition to its many other important tasks, a research on Power Alcohol at the request of the Admiralty. The Food Investigation Board has already obtained important results, and will receive a sum of £24,500 to carry on the work this year. It is estimated that between forty and fifty Industrial Research Associations will ultimately be formed, and a sum of at least £373,000 (out of the £1,000,000 available) will be required for those already in existence or in course of formation. The British Scientific Instrument Research Association, which was to be guaranteed £40,000 from the Department, has now been granted an additional £24,000 to £30,000 as a result of the extension of its scope to include the X-ray electromedical industry and electric scientific instrument makers. To balance this the members of the Association have only to raise an additional £6,400 to £7,000, instead of an equal sum, as is usually required. These exceptional terms have been given on the ground that the Association deals with an industry of the "key" class. Similar generosity has for the same reason been shown to the Glass Research Association, which has been

[1] Report of the Committee of the Privy Council for Scientific and Industrial Research for the year 1918-19, to be obtained through any bookseller from H.M. Stationery Office. Price 6*d.* net.

guaranteed £20,000 a year for five years on condition that the members raise £5,000 a year during the same period. Among the other Associations formed, or in course of formation, since last year, are those involving the following trades: silk, jute, motor, sugar, rubber, Portland cement, leather, boots and shoes, cocoa chocolate and jam, bakery, laundry, piano, etc. Up to July 31, however, only nine licences had actually been granted, and eight associations "approved." The conditions of grant have now been modified to give the Associations the sole right to any discoveries they may make. They may not, however, be communicated to foreigners, and must be sold to any other Associations who might desire to benefit by them for reasonable payment.

The Institute of Science and Industry of Australia has sent us the first four numbers of their official journal, entitled *Science and Industry*, and *Bulletin* No. 12, which deals with the prickly pear. As might be expected, the Journal is largely devoted to discussions and articles of an agricultural or pastoral character. This is, however, due only to the fact that such problems are of paramount importance to the Commonwealth. It is intended that the Journal should cover all branches of science of an industrial character. Vol. 1, No. 4, for example, contains instructions for the manufacture of Epsom salts from Australian magnesite, and discusses how the tanning of Australian sole leather may be modified so as to make it equal to that prepared in England. The striking feature of all these publications is the evidence they bring of the serious losses the country has suffered from imported pests. Sheep-scab is a thing of the past; but the losses due to cattle-tick and worm nodules, prickly pear, water hyacinths, and, of course, the rabbit, still continue. The two cattle diseases probably originated from beasts shipped to Port Darwin from Java in 1872. The population of the town, which then consisted of a few officials and the officers of the British-Australian Cable Company, was short of fresh meat, and the nearest supplies in Australia were a month's journey distance. Eight cows and four bulls were therefore obtained from Java. Some of these escaped into the bush, and to them it has been possible to trace the diseases. Prickly pear was first introduced into Sydney about 1788; but it seems probable that the plants then imported subsequently died, and that the plants which now cover 22,000,000 acres owe their origin to one brought to Scone (about 200 miles from Sydney) in 1839 by a Dr. Carlisle. It was propagated purposely as a hedge plant, and as a stand-by for stock in a drought year. It grows very fast, and in one case on record advanced in a solid mass for half a mile on a four-mile front in two years. Prickly pear cannot be employed commercially for any purpose save as a green manure on soils deficient in humus. To eradicate it seems equally difficult, but it is hoped that some harmless insect may be found which will destroy it. With this end in view the Executive Committee of the Institute recommends that a sum of £8,000 per annum should be provided for a period of five years, in order to pay for experiments, and for the services of a biological expert at a salary of £1,200 per annum, and two entomologists at salaries of £750 per annum. The water hyacinth, which is choking up the Clarence and Richmond Rivers, New South Wales, has propagated itself from a specimen thrown into the Swan Creek near Grafton. It seems possible that this plant may ultimately be utilised as a source of potash. The cheapest method of removing it so far devised is by means of a mechanical destructor.

ESSAYS

METHODS IN PSYCHOLOGY: The Enunciation of the Central Problem and the Solution (Arthur Lynch, M.A., C.L., L.R.C.P., M.R.C.S.).

EVERY man of science who lifts his eyes beyond his own special field of study becomes convinced of the importance of the relation that connects it with other domains; he finds this impression of co-ordination deepened in proportion as his own view becomes more extensive, until at length he recognises that the divisions or classifications of the sciences correspond less to the nature of things than to the limitations of our own faculties. Having arrived so far he is in a fair way to becoming a philosopher, even though he may find an aversion to the term induced by the volumes wherein so much reading in abstruse matter is associated with loose argument and sterile result.

Yet even to the man of science, accustomed to test his work by the appeal to Nature, I say that Psychology should be regarded as the "Queen of the Sciences" in a deeper sense than that which influenced Gauss to bestow the title on mathematics.

Through Psychology, properly presented, we should gain a view of co-ordination of the sciences not derived from historical associations nor brought into evidence merely by the assistance that one science may render to another, but in a more profound sense, which may, in part, be thus expressed: when we make abstraction as far as possible of the actual objective character of the matters studied, and consider in a generalised form the methods employed, and the movements of the mind which correspond to these methods, we recognise the similarity in various fields of the underlying schemata of thought.

As an example, I remember a remark of Sir William Gowers, the distinguished neurologist, who told me that his system of note-taking and reasoning thereon reminded him of the work of an accountant. This is only one out of countless instances that could be given, but, with this observation by the way, I seek a deeper basis for these correspondences. As we continue our analysis persistently, disregarding in scientific processes the inessential details, we will eventually find that the similarity of methods has its source in the modes of working of the mind, and these modes will at length be seen to be combinations of certain fundamental processes. It is one of the objects of this paper to lay these bare; and the meaning of what has been said will become clearer and more significant, I believe, in the course even of this brief exposition. The general problem is far wider in scope than that simply dealing with the co-ordination of the sciences, and the corollaries in every field of thought will be found important enough to satisfy the most exacting.

At the threshold, however, it is well to say a few words on the claims of those who regard a biological, or even a physiological treatment of psychology, as the

only one admissible, and to refer to others who declare that there is no science of psychology outside Experimental Psychology. Certainly all these subjects have an important bearing on the study of Psychology, and for my own part I have not neglected their aid; but the assumption that either of these, or any other partial studies, can cover the whole field of Psychology is so absurd as to be in itself a proof of the need of some more extensive discipline.

Briefly, it may be said that the expositions of physiological-psychology, as, for instance, that of the great authority in these matters, Wundt, will be found to be a discussion of physiology followed by a discussion of psychology. The nexus between the two is never well established. The physical correlatives of thoughts are not thoughts, nor do they, studied objectively, give us an inkling of the thoughts to which they may be the precedent condition. The disturbance of the retina by an ethereal wave is not a sensation of red, for instance, nor is any part of the whole nervous stimulus nor of the corresponding physical reactions, nor the sum-total of these, a colour-sensation.

Even if our histology were immeasurably more minute and illuminated than it is now, so that we could trace not only all the nerve-strands in the brain, but form an image of the vibrations of the molecules in the reaction to a stimulus, we still would on that ground alone know nothing of the psychic state corresponding. It is almost painful to insist on this argument, yet that it is not superfluous is plain when one listens to the theories, or consults the works, of certain great physiologists who from the height of their achievements look down on psychology. What is lacking in their own system is precisely such a grasp of psychology as would enable them to interpret the results of their investigations. We will see this later in regard to Broca's Convolution.

With regard to Experimental Psychology, we have learnt to appreciate the value of the results obtained, but to exclude the play of intellect, which seeks to discover the organic cohesion in all these details, would be as absurd as to obtain a great variety of statistics about falling bodies while refusing to consider the mechanical principles applicable to such cases. I have myself employed experimental methods freely, particularly in the studies of Memory, but I look on Experimental Psychology as the handmaiden, and not the mistress, in the house of Thought.

I will not delay to speak of the importance of studies concerning what is called the "Subconscious," nor indeed of the extravagant claims, suggestive of quackery, of those who seek to popularise "psycho-analysis." Nor will I discuss the form of treatment of Psychology usual in the great text-books—they all have a family likeness—nor the authoritative article on Psychology in the *Encyclopædia Britannica*. When I have indicated the true scientific method no argument will be needed to convince the reader how superficial and tentative are all these expositions, and what an enormous mass of the literature of psychology has the value which may be accorded to a discourse in mathematics where the solutions offered are complicated, abstruse, ingenious, and all wide of the mark.

The conditions of a scientific method in Psychology will have been found if we are able to enunciate the central problem in complete generality; if we exhibit a solution resting on a foundation at least as basic as that of Newton's principles in mechanics; if in the demonstration we conduct the arguments in cogent succession and with rigour; and if finally, in possession of our principles, we show the fertility of the system in a series of new and important corollaries. The prospect thus held out is as fresh as the methods of realisation are new; the two circumstances are mutually dependent. . . .

In general form, the central problem may be thus stated: Standing in face of the whole world of mental activity, actual or possible, let us proceed to analyse all complexes of thought with which we meet, so as to exhibit them as composed of simpler forms; these forms again let us reduce to greater simplicity; in the course of this investigation, having observed that the processes, or schemata, of thought in one domain resemble those of another (the variations being found in the differences of the objects referred to the schemata), and having observed, moreover, that the processes most often consist of combinations of simpler processes, or reiterations of such processes, we are at length brought to ask the question, as to whether it be possible to continue the analysis until a limit be reached at which no further analysis is possible, and then, examining with the closest scrutiny the mental processes that occur at that limit, we ask—is it possible to formulate these processes, to define their character with precision, to show that they are necessary to the simplest modes of thought, and that they are sufficient to explain the most complex mental operations; and that consequently, by retracing the steps of the analysis and representing the course of the thoughts in synthetic forms, we may show that by the combination of these fundamental processes the constitution of any possible synthesis in the realm of thought may be exhibited?

Stated in these terms the problem appears abstract and intangible. The abstract character arises from the extreme generality of the expressions, the word "generality" being here used, in the sense usual in mathematics, not of looseness nor vagueness, but as embracing all particular cases. When particular cases are examined, that is to say, when the system involved in the solution is applied to concrete examples, then the fecundity of the method will be evident; we will find here a powerful instrument of thought applicable to all domains.

The intangibility of the question has been such that in the whole literature of psychology there are extremely few instances of formulation of the problem in any manner similar to that here given. Certainly it is possible, once the system is grasped, to trace the correspondences it presents to those of Aristotle and Kant. Neither of these philosophers, however, found a principle of division to guide the research; there is no attempt to prove that the categories are both necessary and sufficient; the analysis in both cases is tentative and weak.

Certainly the task of tracing down all complex syntheses to their elemental forms would be hopeless. Convinced of this, therefore, I sought other means of attack. Certain suggestions came to me which proved to be not independent of each other, and at length I obtained a starting-point. One of these was that since we must trace all forms of thought to the simplest, we should concentrate our gaze upon simple modes, and make a beginning with any of these selected with as much judgment as possible, at the same time looking out at that level for a principle of division. I determined to examine with persistent attention simple movements of the mind, so as to discover the minutest wheelwork of the mechanism, and thus to gain power to trace out the successive steps of the working. This method suggested to me the expression "putting the microscope on the thoughts." Some of these simple forms I found in the study of the axioms of geometry.

Another suggestion that arose was that, as I could trace most algebraical methods to combinations of the familiar processes of addition, subtraction, multiplication, division, and finally to addition and its inverse operation, together with a system of symbolification, it became laudable to scrutinise in the microscopical manner indicated the process of addition. From this I was led to a study, an intensive examination, of the processes involved in counting.

At length I began to see that in this study was to be found the clue to the whole problem. It led, for one thing, to the consideration of a question that for centuries had been asked by psychologists, and to which various answers, some fantastic, had been given by great authorities—viz., as to how many ideas the mind can at one time entertain. Amongst the endeavours to obtain an answer on scientific grounds may be cited those of Bessel and of Weber. The greater part of the writings on the subject are vitiated by that spirit of inconclusive disquisition which has descended to us from the Schoolmen, and which remains as the model of the academic philosophers of to-day.

In regard to this question I recognised its cardinal importance, and, traversing the arguments of Bessel and Weber, I have shown their insufficiency, and have submitted a series of new arguments which, I believe, will be found to be decisive. The mind cannot at any moment entertain more than one object of attention, together with the relation that associates it with the next. The word "object" is here used, not with reference to any meaning that opposes objective to subjective, but as including any sensation, idea, or incident, of mental life, which can be regarded at the moment as the matter of attention. The term I have used in my book is—"Immediate Presentation."

The meaning of counting arises clearly from this—it is the registration, in some form or other, by means of symbols, of the successive acts of attention, or the reception as Units of the series of Immediate Presentations.

The word "successive" is here inevitable; in other words, the recognition of Time in elementary movements of the mind is a condition of our conscious life. Space also forms a condition.

At the threshold then, and it is for this reason that I have concentrated on this part of the field, we have already ascertained certain processes as necessary, or as I have called them, fundamental. The relation of one Immediate Presentation to another implies Association, and the series, even of two, implies Memory. The process of counting already involves Symbolification. Here, however, I have used the term "Generalisation," which I have shown to involve Symbolification and Classification; these are but different aspects of the same process.

The next process to be considered is that of which the inverse is usually discussed under the title "Discrimination." I have called it Agreement, because when, after a certain sequence of mental experiences another similar sequence arises, there is a tendency in the mind to facilitate this second sequence; or there is an expectancy of Agreement, and it is the disappointment of this expectancy that directs the mind to the points of difference, and so gives us the experience of Discrimination.

In these experiences the affection of the mind, when the conditions are established, arises spontaneously and inevitably. The combination of the movement of thought, so that the successive conditions may be produced, depends on an extraordinarily complicated underlying mechanism, the physical correlative to the mental state; and the successive impacts giving rise to new mental states. In this simple form Immediate Presentation manifests itself in consciousness as the result of the process of Impulse.

This process contains the germ of what we know as Will.

The Sense of Effort is not to be confounded with Impulse. The Sense of Effort has the same relation to motor acts affecting the efferent system, as Sensation has to efferent stimulation. This is one of the most recondite questions in the whole range of psychology, and I have devoted to it a special examination.

A quality involving pleasure or pain is obviously present in certain sensations. I submit that an element of Hedonism is present, inseparably, from all sensation.

We have seen that the inverse of Agreement is Discrimination; every process has its inverse. The universality of inversion I have indicated by the term, "Negation."

I have here indicated the processes which are present in all mental acts: Immediate Presentation, Unit, Memory, Association, Agreement, Generalisation, Impulse, Sense of Effort, Hedonism, Negation, Time, Space.

I call these the Fundamental Processes. I prove that they are necessary even in the simplest forms of thought. I prove that they are sufficient to account for the most complex, and that this is the case for all thoughts possible as well as actual.

This, at first sight, presents a problem that by its limitless complexity and delicacy, verging on the intangible, baffles the effort even to find a principle of division. Certainly the solution could not be obtained in any other manner than by the route that I have already indicated, yet a clear understanding of the preceding discussion points the way to the command of the whole field.

If each Immediate Presentation be a Unit, then in all these exists a common ground of Agreement—viz.: all that underlies the limit of our analysis and enables us to speak of Unit. The differences—objects of Discrimination—of Immediate Presentations differ in the quality of the sensations involved. Underlying any plexus of sensations, presented in any manner of succession and in any complexity, there is a schema of processes such that the schema would remain the same if at each Immediate Presentation the quality of the sensation differed from that of the first series of complexes. But making abstraction of this quality in Immediate Presentation, and remembering what has been said in regard to the Unit, we find in each schema an example of simple mathematical processes. Therefore, that schema, and hence all schemata no matter how complex, must be developed in the manner corresponding to that in which all mathematical processes, no matter how complex, are developed from counting. Therefore, we know that if we cover the whole field of which the schemata correspond to the whole field of mathematics, then by adjoining the actual objects of sensation—or more generally, the Immediate Presentations—in each element, we cover the whole field of mental experiences.

I have said that with regard to mathematics I reserved the questions of Infinity and of Imaginaries for special consideration. I have shown after examination that the introduction of these concepts does not make infirm the rigour of the argument which, in its bare lines, I have indicated.[1]

So far we have entered on a course of thought that leads us widely apart from the disquisitions in authoritative works on Psychology. I have not sought originality for the sake of originality, but I have followed step by step the path opened up by my analysis in the search for the underlying verities. We are here dealing with the subject in a manner which is as near an approach to that of an exact science, as for instance, mechanics, or even geometry, as the subject itself permits; and it will be evident to most that even if details in the course of my arguments were faulty, the true method would consist simply in a rectification of such errors.

I have to add one point more, and that of particular interest, before turning to another aspect of this study.

So far I have tacitly assumed the Idealistic standpoint; that is to say, I have

[1] In *Psychology: a New System* (Stephen Swift & Co.).

considered that the mental experiences correspond to the external world; and the question arises as to how we can be sure that the whole range of objective facts may thus be covered, or that there may be no influence from without which may not disconcert the whole system of Psychology obtained on the subjective standpoint. These considerations are delicate, but, if only on account of a sense of vagueness left in my mind as to what was here involved, I felt constrained to hold back for years the publication of my system. Finally the question resolved into this: Starting from the Idealistic standpoint and continuing in consistency with this principle, is it possible to arrive at a complete sense of objectivity such as is assumed by the ordinary man who has never thought of these problems at all? I say, Yes, and I prove that there is no other way of arriving at the sense of objectivity.

The Idealists of the Berkeley School do not claim this result, and the Common Sense School of Reid has held this crux before the Berkeleyans as a sort of *reductio ad absurdum* of their principles; but, bound to neither of these sects, I find that the Berkeleyans have failed, not by reason of their devotion to Idealism, but on account of their lapses into a crude form of Materialism implied in a tacit recognition of the doctrines of localisation. Given complete illumination and rigorous consistency to the true doctrine of Idealism, and depriving Common Sense of its resource of putting forth as good argument its incapacity for analysis, then a reconciliation becomes possible between these schools apparently so opposed.

I had early begun an analysis of Externality, tracing out minutely the successive steps of the development of the notion of objectivity, importing into the question nothing but sensations and their relations, beginning with the simplest, and proceeding to highly developed complexes. One day in Paris, meditating intently on this subject, I saw in a flash of illumination the link that had been missing, and which now completed the union between Idealism and Common Sense. The Berkeleyans themselves had regarded their own processes from an objective point of view, and it was only by a paradox that they could maintain the position of the external world being resolved into ideas in the head, or brain, of the subject.

But the true Idealist has nothing to do with this assumption. Where are the ideas? Precisely where the object is, as conceived in a complex series composed of ideas. Take an external tree, for example, the idea of that tree is where the tree is, as we conceive it amid the complexes of our ideas; and the notion of externality of the tree is reached by the processes, and entirely satisfied by them, involved on the one hand in the successive receptions of the ideas referring to a physical self, and on the other hand in the successive receptions of the sensations, with their relatives intervening, between that notion of self and of the tree. The ideas referring to a physical self are, of course, subject to the same kind of analysis as those referring to the tree.

If it be said that the ordinary uneducated man has the notion of objectivity immediately, and without analysis, I say that the ordinary uneducated man never uses the term "objectivity," nor thinks in such terms; and that processes, of which the counterparts are expressible in forms of analysis, are being carried on and repeated in his experiences continuously throughout his conscious life. The ordinary man acts automatically without the necessity, in this regard, of analysis, just as he walks without knowing anything of the components of the mechanism which enables him to do so, and the study of which enables the anatomist to explain the manner of his walking. If, however, the ordinary man were asked

to describe what he does see, and without the assumptions contained in our habitual forms of speech which have registered the experiences and thoughts of countless generations of men, then the ordinary man would be brought to reproduce in inverse order the analysis which I have indicated from the Idealist's standpoint.

The matters which I have set out here in bare outline form the gist of the exposition of the two volumes of *Psychology: A New System*, and I refer readers to that book for a detailed exposition.

I say that in establishing the Fundamental Processes of the mind, and showing that no other assumption of processes is necessary in the highest development of thought, I have given the solution to the great central problem of Psychology.

Such an analysis is sufficient for those who simply desire to know philosophically the secret of the mechanism of thought; but there are others who are not content until they can see the application of a system in particular examples. They want "results." From this system, however, corollaries of high importance spring up in such fertility as rather to disconcert those who, asking for something new, are themselves anchored to false positions.

Necessarily, as we see from the threshold, the system must affect the whole domain to which the word "Education" is applicable. This study should be regarded as of the same vital importance to education in any form as anatomy is, say, to orthopædics. It is true that generations of educational authorities have issued rules without knowing anything of this analysis, but it is also true that generations of men have performed surgical operations in ignorance of anatomy. They have often bungled without knowing why they bungled. That is true of the educational authorities.

In more definite instances: I will, out of thousands of applications to problems in mental science, indicate two or three of special importance. The Transcendental philosophy of Kant has dominated orthodox circles of thought, and particularly the great Universities, for generations. I deal with that philosophy in a manner analogous to that in which Lavoisier treated the theory of phlogiston. He brought it to a definite test. The pith of the Kantean system consists in the conception of a Transcendental world in which Pure Reason and Will coalesce.

I subject the processes or faculties of Reason and Will to an analysis far more searching and minute than anything Kant has shown—I reduce them to terms of the Fundamental Processes—and I demonstrate that because, viewed in regard to their elemental structure, they are differently composed, then neither in the Transcendental world, nor in any other, can they coalesce.

Another of the sacred doctrines of the Universities, and of the Medical schools the world over, is that of Broca's Convolution, the third left frontal convolution.

I have shown, from consideration of psychology, that here one is less able to decide that the doctrine is right or wrong than to affirm that the enunciation of the doctrine usually given involves misconceptions of the nature of the phenomena in question. Here, it happens, I can cite authority, though I do not wish to have recourse to that in support of my arguments. Dr. Marie, working at Broca's own hospital, the Salpêtrière, and with cases more numerous and better selected for the purpose than those of his predecessor, comes to conclusions, on the clinical and histological side, which bear out what I have set forth with more particularity on the psychological side. . . .

In the field of Association again I have developed the scope, and shown the power, of this system, if only in its adapted use as a means of investigation.

Elsewhere [1] I have published some results of these studies, as for example, in the consideration of the mystery of Shakespeare's sonnets.

In the field of Memory I have instituted a new style of examination, and have established a series of new results.

Among other fields into which I have introduced new considerations may be mentioned those of Dreams, the Sense of Effort, Fechner's Law, Mathematics, and, finally, the establishment of a basis for a scientific treatment of Ethics.

I will not continue to speak of these corollaries of the main principles; I will only say that just as in mathematics the Cartesian system opened the way to the solution of countless problems even in fields never examined by Descartes, so the Fundamental Principles will throw light on problems in every domain of thought, and illuminate the whole field.

The greatest advantage, however, when the problems of the ordinary text-books cease to be stumbling-blocks, will be found in the habitual use of the system on the part of those who have completely mastered it and make it a part of their way of thinking. It will be found to be like a clarification of the medium of vision, and in dealing with questions that arise in various fields, whereas otherwise we must often be content with "symbolical" demonstrations, we are by this system enabled to trace down every argument to its basis, and so to attain at length, even in matters of puzzling complexity, a sort of "intuitive" view.

DALTON'S DEBT TO DEMOCRITUS (Joshua C. Gregory, B.Sc., F.I.C.)

THE Great War has brought the angel with the sable wings to so many doors that the emotions aroused by death, always present and always poignant, have gathered into a great wave to sweep through the hearts of men. This wave of emotion has touched the mind through the heart and turned it, now wistfully, now with eager hope, now doubtfully, now with firm conviction, towards the belief that the dead can remain in communion with the living. This belief has been so long a candidate for recognition in the world; it has been, in turn, so passionately believed and so scornfully rejected, that it is difficult to be sure whether it be merely experiencing one of its frequent recrudescences, or whether it be at last in the act of confirming the claim so frequently allowed to it and then rejected.

Thought has constantly, during human history, produced conceptions as candidates for recognition. Many of them, like the once prevalent belief in metamorphosis of animals into men or of men into animals, have ceased to be candidates at all. Others, like the Greek theory of cosmic cycles that constantly return the round of change to its starting-point, remain permanent candidates with varying fortunes as they are alternately received into favour and displaced from it. Others again cease to be candidates through final acceptance. The prolonged candidatures of many conceptions, such as the theory of organic evolution now firmly associated with the name of Darwin, give rise, from time to time, to fierce controversies over the names of their inventors. Samuel Butler's savage attack on Darwin's right to be regarded as the founder of the evolution theory confused the simple recognition of the evolution theory as a candidate for recognition with the penetrating vision that perceived the full nature and value of its claims. Butler may have been right in supposing that Buffon, beneath apparent deference to the predominant theological view of his time, ironically suggested the evolution of animal species into and out of one another. Sir Walter Raleigh may have

[1] In *The Book Month*, July 1919.

hinted at organic evolution, at transformism, as Butler asserts he did hint, when he compared the cat with the ounce, the dog-fish with the shark, and the Hispagnolian with dogs and wolves. Goethe suggested the formation upon an original of all more perfect types; Erasmus Darwin surmised that an original living filament was excited into action by necessities and became the source of all organic life; the author of *The Vestiges of Creation*, Lamarck, and many others understood transformism to be a candidate for recognition. This candidature had been realised by the Greeks: Anaximander, for instance, supposed that fish deserted the water for dry land, acquired new members and became men. The candidature of transformism dates back virtually to the first "syllable of recorded time" in the universal belief in metamorphosis that dominated the primitive mind. Cinderella's coach, made from a pumpkin, and her horses, transformed mice, still preserve for the nursery an echo from the beginnings of transformism's candidature for recognition. But Charles Darwin first perceived and disclosed the real value and character of the candidate. His originality, his claim to be regarded as the real *founder* (not merely the suggestor) of the conception of organic evolution, rests on the final dominance he secured for this conception. Before the publication of the *Origin of Species* in 1859 it was possible to think without reference to transformism; after its publication it was no longer possible. The conception of organic evolution, the conception that species increase and spread by the transformation of previous species, stepped from its status of mere candidature for recognition into the position of a determining category of thought, still compelling us to think in its terms and to conceive of all life according to the method it imposes. Charles Darwin made the concept of evolution a dominant concept; till he made it dominant it had never been more than an interesting suggestion.

Dalton did for the corpuscular theory of matter what Darwin subsequently did for transformism. Physical speculations had never been able to proceed very far without the assumption that matter consists of small ultimate particles. They had proceeded without it, just as interpretation of animate nature had been possible, and to some degree successful, without coming under the dominance of the concept of evolution. It seems fair to say, however, that the corpuscular theory of matter, possibly because it was more essential to thinking at all than the concept of transformism, was more specifically thought out into the system of thought before Dalton established it firmly and dominantly in the Atomic Theory than the theory of organic evolution was thought out as a primary concept before Darwin impressed its recognition. For this reason, possibly, Darwin's inception of the doctrine of organic evolution evinced more of the character of an *ad hoc* hypothesis than Dalton's introduction of the Atomic Theory. It was thought formerly that Dalton did first propound his hypothesis absolutely *ad hoc*. It was supposed that, to explain such principles as the constancy of proportion discovered in chemical combination, he again invented the idea of the atom, just as the Greeks, long before him, had invented it to explain the phenomena of the material world. It is improbable in the highest degree that Dalton should have been so ignorant or forgetful of the atoms or corpuscles figuring for centuries in physical speculation as to conceive his Atomic Theory in entire independence of previous theories. Roscoe and Harden have shown that he did, in actual fact, found the Atomic Theory as an adaptation of previous corpuscular hypotheses to explain the discoveries of chemistry. Darwin was apparently less dependent on previous versions of transformism than Dalton on previous versions of corpuscular theories. Malthus suggested to him his famous theory of Natural Selection, and by this route he again encountered the

constantly recurrent idea of the transformation of species into one another, that he established, by his masterly analysis, as one of the dominating conceptions of modern thought.

The concept of transformism, by deepening apprehension of the continuity pervading the evolution of the forms of life, has deepened apprehension of the continuity running through the evolution of human thought. This continuity is not the simple persistence of conceptions as they wait patiently through the centuries, now receiving more favour and now less, until they are summarily dismissed or received permanently into thought. The doctrine of transformism was prejudiced by its early association with metamorphosis. All primitive notions of facile transformations had to be cleared out of it. The various doctrines of metempsychosis and transmigration of souls, deriving their schemes and systems from the unorganised multitude of beliefs in metamorphosis scattered through primitive thought, though they might provide a version of the fundamental notion involved in the modern doctrine of the transmutation of species acceptable to some ages, did not provide a version that could result in a rational, permanent, and dominant concept of the evolution of life. No two ages think alike: few concepts, if any, though they retain the permanent individuality implied in the continuity of life between the mature man and the infant in whom he can hardly recognise himself, are the same at the beginning and end of their history. The concept of the transmutation of species has been a candidate for recognition from the far past until now; but it has greatly changed its mode of candidature from its earliest forms, still represented by Australian aborigines who believe that certain sacred stones are transformed, when their times come, into kangaroos, to its more rational claims in modern evolutionary thought.

The corpuscle, or atom, too, has a history behind it. Dalton had a hypothetical material particle placed at his disposal for theoretical interpretation that has given science a grip over her data and a power of deductive insight almost unparalleled in the history of thought. The atom has not ceased to change since Dalton's day, just as it had a history before it. The minute particle of matter, an ultra-microscopic dust-particle, served adequately as a mental image of the Daltonian atom; it is no longer an adequate image to represent modern physical speculation on its nature. It is difficult to-day to conceive the atom in a way adequate to the interpretative demands made upon it: we, who live in the midst of this struggle for adequate conception, readily realise the difficulty. It is much easier to forget or ignore the struggle imposed upon the human mind to secure the conception which finally enabled Dalton to place the corpuscular theory of matter in the position of a dominating, fertile, and marvellously effective concept.

Science has interpreted nature with extraordinary success by applying the concept embodied in the notion of the corpuscle or atom as an infinitely small solid particle controlled entirely by mechanical laws. This is the core of the idea: the notion used by Dalton, and even employed to-day as a working notion by chemistry, though the conception of an indivisible, impenetrable solid has been the stepping-off place for modern speculation rather than a permanent resting stage. Success in one direction usually raises difficulties in another, and the success of the mechanical concept of nature has been no exception to the rule. The resolution of material bodies, their changes, their interactions—all the many and varied phenomena of the outward world, so successfully into motions of particles, has converted the appearance of consciousness, or even of life in its unconscious phases, into an apparently insoluble mystery. The evidence gathered by science since the foundation of modern chemistry by Lavoisier has steadily

converged on the overwhelming probability that the living emerged from the non-living, consciousness from the living and the mind of man from its first more rudimentary forms. Science, in obtaining its conceptual grip on inorganic nature, and even on the physico-chemical processes within living organisms, has steadily resolved nature into a system of particles moving under complete mechanical determination that seems, whatever may be the real truth, to be entirely incompetent to produce either the more highly developed mind of man or the less developed animal mind that appear to have had their source in it. This particular problem is not under discussion here; it serves, however, to emphasise how necessary it was for thought, in its attempt to conceive the universe, to establish the notion of the purely mechanical particle or atom as a dominant concept or constitutive category. The conception of matter as composed of ultimate particles, mechanical all through, seems to the modern mind so inevitable, so natural, so adapted to scientific interpretation, that it finds it difficult to understand how hard-won an achievement it was. Yet the atom's history, as it appears in the earlier candidature of corpuscular theories for recognition, indicates an arduous development before Dalton could draw freely on completely mechanically conceived particles.

There was the minor difficulty of conceiving gross bodies, apparent to sight, touch or hearing, in terms of minute particles. Imagination was needed for the speculative reduction in size of perceived material objects to atomic dimensions. It was not left without hints: material objects vary in magnitude from mountains to pebbles—suggesting the extrapolation of still vaster objects on the one hand and of still smaller on the other. Very small objects suggest still smaller—"motes" in sunbeams gave the Greek mind a conceptual start towards the idea of the atom. Imagination could overcome this primary difficulty and other difficulties connected with it: the names of Leukippus, Democritus, and Lucretius are a sufficient reminder that it did overcome it; but corpuscular theories did not appear in the first instances of Greek speculation. It seems to have been easier, in the beginnings of philosophical and scientific thought, to proceed in the opposite direction to the speculative diminution of perceptible bodies into very small atoms and draw out matter into a vast, extended continuum. Thales, if we can expand the few hints of his thought that tradition has preserved, thought of water as the original, homogeneous matter that differentiated into the varied elements and objects composing the world. Prof. Ward, in his *Psychological Principles*, suggests that primitive men, while fashioning their tools and weapons, would naturally think of a "mother-stuff" or *materies* formed into many things, and remaining more permanent than the varying forms into which it passed or was fashioned. Since it would be natural for reflective thought, as it passed into the more reflective form recognisable as science or philosophy, to proceed at the first by a reflective extension of traditional concepts, Thales is quite the natural successor of the more primitive thinkers who preceded him. Anaximenes identified the "mother-stuff" with air. Anaximander pushed the fundamental concept of a primary formless continuum into its abstract extreme by defining primitive matter as the "eternal, infinite, indefinite ground, from which, in order of time, all arises, and into which all returns."

The notion of matter as an original, formless, ultimate, extended continuum, passing into the varied items of the world by the impression of forms upon it or by differentiation, first delayed the corpuscular theory and then assisted it. The evolution of the primary matter into objects steadily decreasing in size, from the vaster masses, like seas and mountains, to such minute bodies as the "motes" in

sunbeams, would suggest the same speculative extrapolation of smaller bodies still that reflective inspection of the different magnitudes of bodies would suggest. Thus, by a natural, perhaps inevitable, movement of thought, the Greek mind first attempted to conceive the material world by magnifying magnitudes into one vast, original matter, and then comminuted it into smaller parts during the attempt to derive the actual world from its speculative original. Another step, inevitable perhaps, though probably more difficult to take than we can appreciate, took the Greeks straight to the conception of infinitely small particles or atoms as the structural elements of all bodies. Then the atom took the place of the "mother-stuff" as the original constituent of matter. The concrete movement of thought, with its tortuous complexities and continuous persistence against the clog of the past that tends to restrict it, can only be inadequately represented in summary. Nevertheless, the milestones and signposts do describe the journey, though they are silent on the burden and heat of the day. Anaximander derived the universe from a vast continuum of "mother-stuff"—matter was, so to speak, its own ultimate constituent. Empedocles began to break up this single constituent into more elementary constituent parts, though he went no further than the four elements—earth, water, air, and fire. These elements were *divisible* primal matters and thus ready to be broken up into further constituents. The opinion of Anaxagoras that the ultimate constituents were the particles or "seeds" of stone, gold, water, and the like, constituting individual stony, golden, watery and other objects, was logically transitional between the four elements of Empedocles and the atomism of Leukippus and Democritus. It had been impossible to strike out at one stroke the concept of immutable material particles, extended, indivisible, and differing in size, shape, and weight. It had been impossible to begin the interpretation of nature by means of different atoms, their movements and their arrangements. Dalton was indebted to a struggle of thought, occurring more than two thousand years before him, for a concept that he made into such a powerful instrument of scientific interpretation.

This minor difficulty (if it be fair to call it "minor") of conceiving minute, ultimate particles at all was organically connected with another serious hindrance to the conception of *mechanical* atomism. This hindrance can be analytically isolated for discussion, though not isolable in fact. Bergson (in his Introduction to his *Creative Evolution*) says that the intellect is at home among inanimate objects, and most at home among solids. Its logic, he adds, is a logic of solids, and its concepts are modelled on the solid material body. The concept of the corpuscle that has so effectively served the mechanical interpretation of nature, which, in its turn, has demonstrated its power in the mastery it has bestowed upon science over material processes, is obviously modelled on the solid body. The atom that made the Atomic Theory a dominant concept is primarily, and in the first instance, a speculatively attenuated *inanimate* solid. But the *inanimate* solid body was not the first model. A complex process of thought can be summarised, adequately, though under the limitations of all concise summarisation, by identifying it with a speculative attenuation to atomic dimensions of the *animate* object. Before the atom could dominate the *mechanical* interpretation that has given science its grip over phenomena, its psychism had to be squeezed out of it.

It is possible that the dregs of its original psychism have not been squeezed out of it yet. Science is being urged to-day to drop the concepts of "force" and "cause." "Force," it is argued, is the ghostly remnant of the Empedoclean strife and friendship that disunited or united the elements, or a name simply pretending to have abandoned the idea of psychical control over bodies and their movements.

"Cause" conveys the meaning of "effective operation upon," originally derived from the sense of our own efforts and actions on our surroundings. It is difficult to decide whether, in attenuating the material body to atomic dimensions, a little bit of psychism did or did not remain up to the days of Dalton with the little bit of matter; it is not so difficult to perceive that animistic conceptions were included in the first Greek speculations about matter, as about all else.

The backward glance of history along the track of thought notes psychisms disappearing from the concept of matter. The familiar "abhorrence of a vacuum" was a psychism—an endeavour at explanation, through a concept obviously couched in psychical terms, that evoked the caustic comment from Torricelli, when he supplied the mechanical explanation of the mercury column in the barometer, that nature evidently only abhorred a vacuum to the height of twenty-nine inches. Descartes, Prof. Ward remarks, finally cleared matter of animism. The psychisms he expelled were perhaps a considerable residuum, but only a residuum, of the pervasive animism organised into the first Greek conceptions of the material world.

When Prof. Burnett, in his *Greek Philosophers*, says that "the Milesians had reached the conception of what we call 'matter,'" he substitutes the final product, a conception of matter in terms of non-psychical particles controlled solely by mechanical laws, for the animistic original in the minds of the first Greek philosophers. The saying of Thales, fortunately preserved in his sparse tradition, that "All things are full of gods" does not sound like the voice of the modern physicist, who would hardly regard his atoms as containing gods, or, to use a more satisfactory equivalent for the "gods" of Thales, as animated. Prof. Burnett explains this outburst of mystic enthusiasm as "polite atheism." If *all* things are full of gods they are all on the same level: it is as though there were no gods at all—Thales is a materialist through and through. Burnett definitely compares Thales, Anaximenes, and Anaximander to modern materialists who derive everything from lifeless matter mechanically conceived. Thales did place all things on the same level, but it was a psychical level: all things were alive. The primary "water" of Thales, or the "air" of Anaximenes, or the "infinite" of Anaximander are, however, inadequately rendered either by comparing them to our modern "matter" or to our modern "protoplasm." To think of protoplasm as animated matter, a clear, definite distinction must first be drawn between the purely material and the vital or the psychical. Science has prospered by abstracting the material from the vital and the mental to be studied as a separate domain in mechanical terms; but this abstraction was not struck out at a blow. The thought of Thales was more homogeneous and undifferentiated than our conceptual system, with its sharp analytical distinction between the psychical and the material: Thales simply thought of *all* things in one great round of transformation from the primal "mother-stuff" and back to it again.

It is generally admitted that our sharp discrimination between mind and body, between the physical and the psychical, dates from Descartes. Aristotle did not sever them so completely, even in thought, though he realised their distinction and was familiar with the conception of the soul's incorporeality. Anthropology informs us, as Prof. Ward remarks, that for primitive thought the duality of mind and living body is obscured by their close connection. Philosophy starts with the mental outfit provided by the past, and it would be indeed startling to discover that Thales and his immediate successors had leaped straight from animistic concepts, in which the physical and the psychical are organised together, into such purely physical conceptions as the production of "earth" by the condensation of

"air" through the intermediate condensation into "water," just as we regard the condensing of steam and the freezing of water. Prof. Burnett compares this conception of Anaximenes to the modern concepts of the physical states of matter, and regards it as typical of the purely physical conceptions attained by the Milesians. It is possible to pick out statements that imply only physical concepts; it is equally possible to discover what seems to us an indiscriminate use of the physical and the psychical. Cornford (in his *From Religion to Philosophy*) is certainly nearer the truth when he describes the Milesian primary material as a subtle, mobile stuff, *animate* and divine. Anaximander conceived the eternal round of change in three main stages: the elements earth, air, fire and water separated from the primal *materies*; then individual things were engendered from them. This process reversed returned all individual existents to the original continuum from which they came. A "mother-stuff" became four "daughter-stuffs"; four "daughter-stuffs" produced the world of things. "Things," says Anaximander, "give satisfaction to one another for their injustice, as is appointed according to the ordering of time." He seems to regard the combination, say of water and earth into earth-water compounds, as an encroachment by elements upon one another's domains, like attacks upon their neighbours by plundering tribes: "satisfaction" must be given by the return of the elements to their own private regions. "Satisfaction," "reparation," "injustice"—this is not the language of mechanistic physics. Such rampant psychism in Anaximander is a lucid commentary on the "all things are full of gods" of his contemporary, Thales: Thales' primal "water" was animate and psychic. Both were animists, no doubt, without knowing it—they did not realise any necessity to distinguish the psychic or animate from the physical, or, for that matter, the possibility of making the distinction. The Milesians mingled the psychical and the mechanical together.

Atoms did not easily escape from this animism in their ancestry. Empedocles still drives his elements by hate and love. When his elements crumble down into the atoms of Democritus their psychism begins to escape, but it does not escape completely. The Greeks remained long in the relaxing grip of primitive animism because they could not rid themselves of the notion that the soul was the principle of movement. Aristotle remarks, in the *De Anima*, that his predecessors had handed down motion and sensation as "the two characteristics of soul." "Democritus," he writes, "whose view agrees with that of Leukippus, consequently maintained soul to be a sort of fire and heat . . . he declares that those (atoms) which are spherical in shape constitute fire and soul. . . . The reason why they maintain that the spherical atoms constitute the soul, is that atoms of such configuration are best able to penetrate through everything, and to set other things in motion. . . ." Gomperz, in his *Greek Thinkers*, notes that Democritus made the most mobile atoms the vehicle of psychic functions. The atom has begun to part with its animistic outfit—the least mobile atoms are being prepared for Dalton. But the deep impress stamped into the human mind by that impressive model of reality, the animate or psychic object, is not easily erased. Its close connection with movement kept the psychic anchored in matter. Democritus quite failed to de-animise the most mobile atoms, and thought flowed on to Descartes before the final expulsion of animism from matter. By composing the soul of one set of atoms Democritus preserved in the very heart of the Atomic Theory the confusion between psychical and physical status; but he made possible the future victory of science in its long wrestle with animism, though he did not himself achieve that victory. Did Dalton realise the continuous mental effort, prolonged through

centuries, which at last gave him his atom free enough, if not quite free, from the psychism that long prevented the progress of physical science?

The more clearly we understand the course of human thought, the more clearly we understand our debt to the first Greek thinkers. Men had to pass through the first unreflective stages of thought when their concepts were, so to speak, produced spontaneously within them as a reaction to impressions from without. Two concepts, in particular, grew up inevitably within their minds: they thought of the primary constituent of material bodies as a vast continuous "mother-stuff," and they took as their fundamental model, which may be the most valuable and fundamental in the final philosophical synthesis, the solid with a soul or mind in it. The Greeks did pioneer work in preparing the way for the mechanical view of matter that was a necessary scientific instrument for progress, though it may be neither all knowledge nor even the last word in chemistry and physics. They crumbled the "mother-stuff" into atoms, and they expelled most of the psychism from them. This is a short summary of a long story, for the Greeks dealt with more problems, even in their Atomic Theories, than can be counted on the fingers of Briareus; but it is enough to remind us that the Atomic Theory, like most concepts which attain to high status, has grown to maturity from roots deeply imbedded in the past.

POLITICS AND SCIENCE (Frank H. Perrycoste, B.Sc.)

I

FOR more than a generation past scientific men in this country have been warning their fellows that, unless they learned to appreciate science, to acquire the scientific spirit, and to orient themselves by the scientific pole star of to-day instead of by the *ignes fatui* exhaled from a decomposing past, they would be punished by the *débâcle* of our commercial supremacy, and by the compulsory resignation of the sceptre to those nations which, like America and Germany, have long since most thoroughly realised that science must now and henceforth lead and rule the world. Such appeals and warnings were necessarily addressed —on account of the hardness of men's hearts, the grossness of their understandings, and the almost universal idolatry of lucre to the Briton's regard for his wealth and his country's commercial and industrial supremacy: but even so they failed lamentably, because an "education" based on centuries-old traditions had so thoroughly diseducated our political and industrial ruling classes that not even arguments levelled down to their own materialistic criteria could penetrate their mental hides. To the explosively shattering and very nearly fatal lessons of the Great War even such hides have proved at last not quite impermeable: and at last even politicians and capitalists are realising, however inadequately, that science must be accorded a larger share than formerly—it could scarcely have a smaller—in national regard. There is now some real respect for, and tardy recognition of, "applied" science; and naturally scientific men are rubbing in the acid cure where the wound is tenderest and smarts the most; but they know only too well that, unless this nation will learn to love and pursue science in the spirit of true science—viz., from the love of knowledge as the quest of truth, and for the sake of culture—the victory will not have been achieved. Industrial and military success are among the rewards lavished by science: but actually they are, and should always be regarded as, by-products, as results not of the search for themselves, but of the wholehearted disinterested pursuit of science from the

love of knowledge, the desire to fathom—as far as fathomable—the mysteries of existence and consciousness, and the truly Hellenistic culture of "this main miracle," the human self. All this our great men of science most thoroughly understand and teach—at least to those fitted to receive it: though the grossness of the materialistic uneducated herd of politicians and capitalists and commercial men, and the enforced ignorance of the masses, too often compel them to base their arguments for the study of science upon the rewards of "applied" science.

It is to be feared, however, that even many scientific men themselves have suffered from the absence of any really scientific education, of such an education at school and in earlier university days *in general science* as would make them visualise science as one mighty synthesis including not only physics and chemistry, astronomy and geology and biology, but also psychology and ethics and social science—a synthesis all alike to be studied with the same rigorous scientific precision, and giving rise from its respective departments to branches of "applied science" in industry and warfare, and to the "arts"—as in such cases we style "applied science"—of the physician and surgeon, of the alienist, and of the statesman or politician as he should be but usually or almost universally is not. A few outstanding thinkers have fully realised this oneness of science and the universality of its sway from the study of gravitating masses and vibrating molecules to the almost inextricably complicated phenomena of vast social organisms. Comte —whatever one may think of his conclusions and results—realised that sociology is, or should be, a science just as is astronomy, and can be effectively studied only after due preparation in the less concrete sciences. J. S. Mill devoted one book of his *Logic* to the scientific methods of inquiry specially suited to unravel these complicated social phenomena: and Herbert Spencer built up a mighty system of synthetic scientific philosophy, in which ethics and sociology were studied by the help of and were based on the "laws" and training afforded by the physical sciences, by biology, and by psychology. Notwithstanding, however, these great examples of a past generation, it is to be feared that too few, even of our greatest men in science, realise that sociology is an essential part of this vast scientific synthesis: and I do not know whether any of them, even among the few who take any active interest in "politics," ever give evidence of any greater knowledge of or regard for science *in the sociological sphere* than does the ordinary citizen or the common politician. However even epoch-making they may be in their own departments of science, they are not necessarily even scientific in their attitude to politics, simply because they have never received any instruction whatever in social science, which—being in many respects the most vitally important of all departments of science to us social units—has, by common consent, been regarded as quite outside any scheme of general education, even if some of us be so very advanced as to insist that physics and chemistry, and perhaps a very little biology, be taught as regular school-subjects.

Now, it seems to me to be a very peculiar and outstanding merit of the Labour Party's program, as laid down in *Labour and Social Reconstruction*, that all its proposals, present and future, are and are to be based on the *scientific study* of social phenomena. I am not aware that any other political party-program has ever been based on such study—obviously indispensable and essential though it is —or that any leading statesman of the last half century has manifested any practical consciousness of the teachings of such thinkers as Mill and Spencer Indeed, it is doubtful whether 5 or even 2 per cent. of them have ever studied the books of any such thinkers: and so it has been left to the Labour Party to insist that you cannot build without reliable foundations, or steer without a compass.

If the Labour Party acts up to its professions, it will very abundantly justify its claim to be the Party of hand-workers and *head*-workers alike.

II

To have realised that, to be successful, political action and social legislation must be based on rigorous scientific study of sociology, is perhaps half the battle—but probably less: and anyhow the second half or part of the battle is the worse. In other words, it is far easier for a scientifically-educated man to realise that he must be as scientific in his legislation as the engineer and chemist and physician in their arts than for him to determine exactly what he ought to do.

He has to analyse phenomena far more intertangled—often almost hopelessly intertangled—than any presented to the chemist or the biologist. He cannot institute deliberate experiments on human society to determine whether a given procedure will be beneficial or fatal; and he has no substitute for vivisectional experiments on chloroformed animals. Observation of past successes or failures—chiefly failures—is of but very limited availability: and inductions drawn from such observations are often liable to the objection that, since social conditions have meantime changed, the inference from the past no longer holds good. Finally, in sociology more than in any other branch of science, even more, I think, than in psychology, the most scientific and most conscientious worker is liable to be all unconsciously biassed by the personal equation. It unhappily does not follow that two gifted scientific men both trained alike in the sciences especially preliminary to sociology, and both equally single-minded, will draw the same conclusions from the same data. To take an example now a generation old—Huxley and Herbert Spencer, both earnest single-minded lovers of their fellows, both deeply trained in biology and psychology, and personal friends and comrades to boot, arrived at very different conclusions as to the necessity for or ruinous effects of interference by the State in certain departments. Spencer consistently advocated *laissez faire*; while Huxley declaimed against administrative nihilism. If such doctors disagree, how can a plain man dare to act? Yet act he must: for in politics, whether he be a legislator or only a voter, inaction—*i.e.*, abstention from legislation or from voting—is a form of action.

III

Here we confront the principal objective of this article—viz., the examination of Spencer's fundamental political doctrine, which, advanced as a rigorous deduction from well-ascertained biological "laws," cuts at the very root of all that is implied in that somewhat loose term *socialism*. No socialist reformer, maybe, has ever surpassed Spencer in his desire to serve his fellows and to build a new Jerusalem: but in their conceptions of the necessary *methods* Spencer and the socialists were utterly antithetic; for the life-saving tonic of the one was the poison of the other.

Briefly, Spencer's doctrine may be stated thus: that, as regards each individual, it is equally in accord with the demands of rigorous justice and with the vital permanent interests of the community that he should suffer or fail exactly in proportion to his merits—*i.e.*, to his abilities, industry, character, and conduct; and that, as regards his children, it is in accord with natural biological justice, vital to the progressive betterment of society, and in the long-run most merciful

(or least unmerciful), that the children and descendants of capable and worthy men should flourish and propagate their like, and that those of the incapable and degenerate and worthless should fail and succumb and die out. I intentionally state the contention in its hardest outlines, omitting all consideration of the scope afforded to private philanthropy to mitigate the lot of the hapless during their life-process of succumbing—for we are concerned with the legislative and economic attitude of the State or Community to classes of men—and omitting also all qualifications, on which no one could insist more strongly than did Spencer, as to the essential and so hugely neglected duty of the State to sweep away all artificial and State-imposed obstacles and disabilities of every sort that deny to the meritorious the natural rewards of their merits; for I wish to reduce the antithesis between the Spencerian Individualist and the Collectivist-Socialist doctrines to its naked essentials. If either be right, the other is disastrously wrong; any Community which systematically legislates and administers on the wrong doctrine is heading for ultimate disaster: and any Community which hovers and vacillates aimlessly between the two without any fixed principles of scientific action is—well, to paraphrase Shelley—"Alas! how many are the same." Moreover, "right" and "wrong" here mean what they mean to the engineer or chemist or physician when he decides from exact knowledge that a suggested engine or process or cure will succeed or will fail disastrously because it conforms with or runs counter to verified scientific "laws" of nature. Right and wrong in sociology must mean scientifically right or wrong as in any other department of science.

Now, clearly Spencer's doctrine must be examined under two distinct divisions—viz., (1) as regards the meritorious or incapable individual, and (2) as regards the (possibly indifferently average) children of each, who, independently of and additionally to their own merits or demerits, are to receive an initial start or suffer an initial handicap from the merits or demerits of their parents. This is clear enough; but a very little biological and psychological analysis will suffice to demonstrate that (1) must be subdivided into (*a*) the individuals who prosper on account of sobriety, industry, honesty, thrift, forethought, self-denial, etc., and their opposites who fail from neglect of some or all of these virtues; and (*b*) the individuals (and conversely their opposites) who prosper exceedingly—or would do so under social conditions affording full play to merit, and tolerating no artificial obstructions thereto—through inborn genius. It is necessary to consider these two subdivisions separately, although the modern student of heredity and applied psychology very fully realises how largely (*a*) may be subsumed under (*b*), and how completely all character that is really character is inborn [1]: but he also realises that sufficiently powerful motives and systematic real education may do much to educe or induce many of the qualifications of (*a*) to a practically appreciable degree; whereas no human power, no ability however great for taking pains, can implant genius or talent in any man in whom it is not inborn.

IV

The triple problem being thus definitely stated, we now proceed to attempt an ethical and social solution based on biological and psychological science. First, let us deal with (1 *a*), for here the problem is simplest. Here, too, the Individualist seems to be on pretty safe ground; since if of two men one declines to

[1] The psychological argument is discussed in detail in the third chapter of my *Prolegomena* to *The Influence of Religion upon Moral Civilisation.*

practise the industry and thrift and self-denial, etc., which the other systematically does practise, then the latter should enjoy the full benefit of his merits, and the former should suffer all the natural results of his wilful demerits. That if a man will not work, neither shall he eat; that if he insist on wasting he shall be suffered to want; and that in general he shall reap as he will persist in sowing; seems to be both biologically sound and sociologically just. Very obviously, too, it is to the permanent and general interest of the whole Community that the one type shall be encouraged and the other discouraged—and the more so since, on the average, the odds are that the children of each will be brought up to follow their respective parents' desirable or reprehensible procedure. In so far, then, it seems to me that the position of the Individualist is tolerably unassailable and in strict accord with scientific ethics.

It is when we come to (1 *b*) that the main difficulty arises: and here, if anywhere, the Spencerian position can be turned. Consider the case of the men who may make fortunes ranging from comfortable to colossal, not by any exceptional practice of the virtues aforesaid, but by the exercise (in itself a pleasure by the bye) of such inborn genius as that of the inventor and discoverer, the outstanding artist or musical composer, or even very exceptionally, and under very favourable environmental conditions, the poet[1] or author—or by the mere possession and exercise of an unusually beautiful musical voice. I would include with such men also the exceptionally skilled surgeon who occasionally makes a moderate fortune, and the subtle and plausible forensic pleader who more frequently and less worthily achieves a larger fortune; and also, in short, every man who finds a profitable market for the practice of some inborn genius or talent or uncommon natural endowment.

Now, the plain layman, I think, would very strongly affirm that all these men, just as much as the industrious, etc., men in (1 *a*), should reap the full money-harvest of the exercise of their abilities; and he would quite correctly remind us that many such men have started life under an initial heavy handicap of poverty or social inferiority, and by sheer force of genius—and, of course, by hard work—have achieved fame and success. Quite correctly, too, he will point out that, as regards the inventors of steam-engines and telegraphs *et id omne genus*, and as regards the supreme poets and artists and musical composers, any fortunes that they may—occasionally!—have achieved for themselves are only drops in the reservoir of material and æsthetic wealth that they have filled for the benefit of the whole world. Spencerian Individualism, as I understand it, vehemently approves this decision of the plain man, and deduces from biology a complete justification thereof. In a word, what is true of (1 *a*) is held to be equally true of (1 *b*): but here precisely it is that I join issue, and maintain that deductions from that part of biology dealing with heredity exactly invert the conclusion, and force us to admit that those in (1 *b*) are not entitled to the full harvest of their "merits," or to more than sometimes a very small proportion indeed thereof.[2] If there is one thing most conclusively proved by those inquiries into heredity and genius, of which Francis Galton was the great pioneer, it is that as regards every degree of genius and talent and ability of every sort, *Nature*—*i.e.*, inborn capacity—counts for everything, and *Nurture* for nothing. Since, then, not even the man of the greatest genius can claim the very slightest moral merit for his possession of genius—since it is inborn in him, and if not so inborn could not have been

[1] Monetarily, Tennyson did not do so badly.

[2] The key to the kernel of this argument was indicated in a note to p. 96 of my *Towards Utopia*, in 1894.

acquired by the most Herculean efforts on his part—how can it be maintained that he has any ethical or sociological claim to the harvest of that which biology proves he had no finger in sowing? He is in the position of a trustee with huge responsibilities but with no ethical claim to be a beneficiary, except in so far as he is one unit of the beneficiary community. I admit that he is a compulsory trustee—in the very nature of the case he could not have been consulted as to whether he would be endowed with genius and trusteeship, or left unendowed and comparatively irresponsible; but happily to genius the chief joy of life is the exercise of genius; and, therefore, he suffers nothing, but rather gains, by the moral obligation to exercise his gift to the fullest—though for the benefit of the Community rather than of himself. Since neither himself nor any one person among his ancestors endowed him with that genius which has somehow resulted from the combination of countless germplasms of the past, we can only say that to the present Community as a whole ethically and sociologically belongs the harvest of the genius which the Community of the past has unconsciously somehow sown, and which the protective and life-sustaining activities of the Community through a series of generations have enabled at last to develop.

In common graciousness, in common decency, in common policy, any properly organised Community will ensure to the inventor or creator a substantial personal reward, not only equalling but exceeding the income accorded to the most industrious, etc., worker of similar education, and in very full proportion to intellectual and æsthetic as well as to merely material needs: but biology seems conclusively to decide that the bulk of the harvest of genius is ethically the property of the Community and not of the individual. To put it in the concrete—if a great artist can make thousands a year, a special pleader tens of thousands, or an outstanding engineering or chemical inventor a still huger income, by the exercise of his genius, I utterly deny that he is ethically entitled to pocket more than a proportion in the first case, a pretty small proportion in the second, and an almost vanishingly small proportion in the third; and I affirm—basing myself on "laws" of biology—that the bulk of the gain belongs ethically to the Community.[1]

Now, moreover, it must be pointed out that on ultimate analysis—as already hinted—the qualities commended under (1 *a*), or their potentialities, are really also inborn. Thus, after all, biological deductions may seem to cut away the bases of even so much Individualism as we have conceded: but here psychology helps us out. As I have already said, these qualities are very largely educeable, and even induceable, by adequate training and sufficiently strong *motives*; and the possibility of civilisation as distinguished from savagery so largely hinges upon susceptibility to motives and foresight; and the cultivation of the habit of acting on far-sighted motive instead of on each momentary impulse is so absolutely vital to society; that it is sociologically sound policy, and, therefore, eminently right, to risk offering (biologically) far too much reward than a fraction too little, in order to encourage industry, thrift, forethought, etc.

V

We may now seem to have cleared the way a good deal: but really it is doubtful whether to the hottest Collectivist-Socialist the over-rewards of genius and talent—except in such cases of those of a singer or of an advocate making his tens of thousands—cause any serious concern. Numberless men of genius, to

[1] Full allowance of course, being made for, perhaps, many preparatory years during which the individual's reward was small, or nil, or perhaps notably minus.

whom all humanity is eternally indebted, have lived and died in comparative or absolute poverty or even penury, or sometimes have almost starved to death; while men generally, or often consciousless exploiters of their discoveries or creations particularly, have harvested what they sowed in lifelong travail.[1]

Even if the engineering or chemical inventor—although possibly only applying the scientific discoveries of a never-rewarded man--amass a fortune for himself, yet the Collectivist knows that the gain of humanity at large is a vast multiple of the individual's gain. The capitalist exploiter, rather than the occasional self-enriched inventor, is the Collectivist's bugbear.

When, however, we consider the converse of the men of genius, and when we recall that thousands of the failures in life are mentally degenerate, that they are constitutionally insusceptible to motives to industry and thrift and forethought—when we realise that there is here a huge class of human beings who suffer necessarily a lifelong handicap from ineradicable inborn defects, for which they are no more responsible than are the gifted ones for their genius—then we perceive how profoundly the biological "laws," which we have examined, must affect our sociological canons. The sheer Individualist who—ignoring the personal irresponsibility for genius or degeneracy—takes his stand on individual merit and demerit, can only reply that here is an ample field for private voluntary benevolence, but that it would be wrong to mulct by State-action the meritorious and desirable in order to alleviate the lot of the however pitiable undesirable. As high as one goes above, however, so deep must one go below; and if we insist that the man of genius is a trustee for the Community, to whom really belong the creations of his genius, so *per contra* the Community of to-day must take the responsibility for the degeneracy which the Community of the past has handed down, and must supplement the earnings of those constitutionally unfitted to maintain themselves in comfort. As to the control that must accompany this responsibility, if semi-disaster in the present and ultimate race-suicide are to be avoided, we will speak anon. Here it suffices that the Collectivist seems to derive from biological certainties a complete ethical and sociological answer to the Individualist argument for the natural rewards and penalties of merit and demerit in the sense of genius or talent and inborn incapacity.

VI

We can now pass to division (2), and can very speedily dispose of part of the problem of the children. The children of the industrious and thrifty and self-denying members of (1 *a*) will necessarily possess advantages over the children of the converse parents. It would be most unjust that a man who eats all his cake to-day should be allowed to claim half of what his thrifty self-denying neighbour has put by for to-morrow, or that the idle should be as well off as the industrious; and, therefore, in the course of years the industrious thrifty self-denying man must accumulate advantages which his neighbour justly lacks. What, however, a man has *thus* earned and saved is emphatically his own; and if, carrying self-denial still further, he use much of it for the benefit of his children rather than of himself, they cannot, without rank injustice, be deprived of such necessary and natural advantages. Moreover, it would be sociologically foolish as

[1] Authors, and even artists, may or may not starve; and publishers and dealers may or may not subsist on truffles and champagne: but that there is very generally some such sort of antithesis between the lots of creators and exploiters is unhappily too evident.

well as ethically unjust so to deprive them, (1) because the desire to benefit children is often a stronger incentive than the desire to benefit self, and the Community as a whole profits by the sum of its units' industry and thrift; and (2) because the children of such parents are likely, both from inheritance and from training, to prove in their turn similarly desirable members of the Community.

As regards the children of men of genius and talent in (1 *b*) no farther elaboration seems really necessary: but it may be pointed out that, since children of men of genius are over 200 times more likely to be gifted with genius than are those of ordinary men, and since the advance of civilisation is entirely contingent upon a steady stream of impelling genius, the Community as a whole, from sheer self-interest, must wish men of genius to have plenty of children, and such children to have every facility for coming to the front.

All this, however, concerns only the first half of the children question: but the second half, the problem of the children begotten by either the wilfully lazy and thriftless or by the mentally or morally (or physically) degenerate—this is really the crux.

In so far as such children inherit from degenerate parents a degenerate nature, we have already considered them under (1 *b*)—*but in the adult stage.* Now we must consider them in childhood, and must similarly take account of the sometimes average or indifferent, and therefore educable, children of more or less degenerate parents, and of the children of merely lazy, or thriftless, or selfish, parents. Anyhow, in the very nature of things, they cannot have the natural advantages of the desirables' children: anyhow, Nature—that cruel heartless immoral stepmother, against whom we are powerless—has treated them with typically flagrant injustice: but are we acquiescently to leave the stricken by Nature to their fate, or—as a Community—to undo the injustice so far as possible, and incidentally to render them as serviceable members of the Community as may be instead of letting them remain drags on social progress?

To state the question thus, turning on to the sad phenomena the searchlight which the study of heredity has put into our hands, is to answer it—subject to an all-important proviso to be made anon. No other answer seems really possible unless we can disprove such biological certainties as the study of heredity has afforded, or unless we frankly wash our hands of a scientific sociology founded on biology and psychology and inspired by ethics, and elect to "carry on" in the weary old empirical rudderless course.

VII

Here, however, the philosophical Individualist sees his chance; and very fully commiserating the pitiable lot of such children, and very heartily endorsing our indictment against Nature's cruelty, he yet maintains that the "Socialists" in their ethical and humanitarian zeal will, in the long-run, render things worse even than they already are, and that Spencerian *laissez faire* will really prove ultimately the most merciful and least injurious course. The old and very natural and reasonable objection that the industrious and thrifty man who, with much self-denial, is struggling to equip his own children adequately for their course, will be victimised with cruel injustice if he be heavily mulcted to assist the—admittedly luckless—children of his lazy thriftless selfish neighbours—this primary objection is very largely, if not completely, offset by the modern Collectivist proposals to grade income-tax up to at least 75 per cent. on large incomes, to diminish the tax on small incomes, and to nationalise various services, and thereby diminish expenses

to the consumer. The Individualist, however, has a far stronger argument. He reminds us that a Community will truly prosper directly as the percentage of gifted and desirable units, and inversely as the percentage of wasters and degenerates: he and we alike know that even some years ago the percentage of the former was falling in this country because the desirables were having smaller and smaller families, while the undesirables conversely were increasing both proportionately and absolutely: and we all realise that the war has robbed us of our very best, and has cut off—often childless—much of the moral and intellectual flower of our race. If, now, the Community is systematically to foster and paternally to provide for all the degenerate and semi-degenerate, and to see that the children of the idle and thriftless be well provided for—even if their wilfully worthless parents be left to stew in their own juice—what is before us but a tremendous national decline through the swamping increase of low types that should be encouraged to die out? Surely in such case we shall soon have neither an A1 nor a C3 but a Z26 nation! This is what the Individualist means when he —as true a philanthropist as the Socialist—maintains that the most merciful *i.e.*, the ultimately least unmerciful—course is to let the degenerate die out, and to let Nature gradually extinguish Nature's failures.

If we had no *practical* reply to these objections, we should have to admit that, in spite of all the foregoing arguments, we are helpless, and that the Community cannot take actual and efficient responsibility for the degeneracy bequeathed by the past without bequeathing in turn a yet still more terrible and fatal legacy to the future. The key to the answer was, however, given above when I spoke of *control.* Just as many already realise that rights entail duties, and that privileges imply obligations—even Homer's heroes and Milton's friends realised this; and that *noblesse oblige* is proverbial—so we must insist as a fundamental sociological axiom that *responsibility connotes power.*

This is absolutely vital; and its neglect under any "Socialistic" régime starkly suicidal. Power without responsibility means Prussianism and Tsarism: responsibility without power is hell. If the Community is to shoulder half or three-quarters of the burden of sustaining[1] those degenerates who, through no fault of their own, are congenitally incompetent to maintain themselves in decent comfort, and is to render the life-pilgrimage of these unfortunates tolerable instead of a dreary nightmare; if it is to assume paternal charge of all the tens or hundreds of thousands of children whose parents cannot or will not provide adequately for them, and is to guarantee to all such children as much education as they are capable of receiving, and a really fair start in life: then in sheer self preservation the Community must insist on, and rigidly enforce, its absolute claim to secure that no degeneracy or heritable congenital defects shall persist beyond the present generation of degenerates, and that the Community of fifty or seventy years hence shall have no incubus of mentally or morally, or even physically, degenerate members—none but a few occasional sporadic morbid "sports" from the normal, which it, in turn, may effectually prevent from handing on their like. Here we have the really central problem of "Socialism"—a problem that, so far as my limited knowledge goes, though I write subject to correction, Socialistic propagandists have never squarely faced.

Yet, unless it be squarely faced, and most efficaciously dealt with, a permanently successful Collectivist Community is inherently impossible, and national deterioration increasing with something like geometric progression is inevitable. If we agree to be responsible for the degenerates during their individual lives,

[1] Not at mere subsistence-level, but at an agreed standard of comfort.

and for all the children of the nation who may need help, we must ensure that only mentally and morally and physically healthy stocks shall breed and increase, and that all tainted stocks shall be extinguished. To have said this a generation ago would have been equivalent to demanding either that the degenerate should not marry, or that, if they married, they should either systematically practise an unnatural abstinence or unfailingly take certain precautions: whereas the mentally and morally degenerate are precisely the very people who can be confidently relied upon to do nothing of the sort. Now, however, we know that permanent sterilisation is perfectly practicable, and that the sterilised are not deprived of the capacity for the gratification of mankind's strongest natura passion. Individuals who have no degenerate taint, but who, for one reason or another, had decided that they ought to beget no more children, have voluntarily submitted themselves to a simple operation in order to obviate the necessity of repeatedly recurring individual precautions *ad hoc*: and what such individuals have done voluntarily for their own sakes, the Community must absolutely insist that the degenerate shall do compulsorily. We can afford to take no risks whatever in this sphere. Anyhow, and even under the existing régime, the horde of degenerates already involves a great danger and great expense to the Community; but the danger and the expense will become fatal if the Community assume full responsibility for all its units, but exert no power of preventing the multiplication of unfit units. There is no question of caste or class here, but merely of the national extirpation of a national menace. Any individual of any class or rank or occupation whatsoever, who is afflicted with any form of mental or moral or serious physical degeneracy probably heritable by children—no matter whether such individual belong to a class of tramps or paupers, to that of unskilled labour or of skilled labour, to commerce or the professions, to the peerage or to the Royal Family itself, must be sterilised in time, and effectually prevented from handing on the taint to a succeeding generation. The appalling history of the "Jukes" family in America is a standard example and warning of the huge expense of crime, vice, misery, and actual money, caused to the Community by the offspring to a few generations of *one pair* of degenerate units: but we have to contemplate the multiplication of such expense by thousands or tens of thousands if a Collectivist régime safeguard degenerate stocks from extinction by the hard and painful agency of natural processes, but refuse to ensure their painless extinction by sterilisation. Crowded with wrong and injustice and destitution and exploitation as is our present social régime, I am open to maintain that it would be *ultimately* better to carry on in large measure as we are, and to abandon entirely for the present—and until an eugenic conscience be developed by the majority—any attempt to assume Collective responsibility for the comfort and happiness of all the units of the Community, unless we are prepared to assume and efficaciously to discharge the correlated responsibility for ensuring the Community against the propagation and increase of the unfit. This is a condition that should be put in the very forefront of the Labour Party's program, if this Party is to be really the Party of hand-workers *and head-workers*: and what class stands to gain more than that of the hand-workers by the elimination of the unfit?

* * * * *

I have concluded an inquiry that has run to far greater length than I had anticipated. It appears to me that, subject to the proviso for unflinching eugenic control, I have turned—a frontal attack seeming impossible—the Spencerian biological argument against "Socialism," and have shown that the Collectivist

can claim the sanction of science for his régime. The question is, however, or such vital and fundamental importance that the argument should be most critically scrutinised from every side in order that either it may be admitted as conclusive or any lurking fallacy exposed. In two characteristics the Spencerian Individualist and the Collectivist of whom I write are agreed—viz., in the burning desire to promote the abiding good of humanity, and in the profound conviction that, unless we steer by the pole-star of science, we shall make utter shipwreck of society. If my suggested steering be wrong, let some better navigator correct the error.

REVIEWS

MATHEMATICS

History of the Theory of Numbers: Vol. I, Divisibility and Primality. By LEONARD EUGENE DICKSON, Professor of Mathematics in the University of Chicago. [Pp. xii + 486.] (Washington : Carnegie Institution of Washington, 1919.)

ONE of the most encouraging developments of the present day is the return to an interest in the historical development of mathematics which had apparently disappeared since the appearance of such works as Todhunter and Pearson's *History of the Theory of Elasticity*. Recently the revival of this interest has been quite definite, at least as regards the history of mathematics in general. The present volume is notable in that it gives a connected account of the historical development of one special branch of pure mathematics, and that, as it happens, the most progressive one at the present day. Mathematicians now owe a great debt to the Carnegie Institution of Washington—a debt which we may hope will be extended by the production of similar volumes dealing from the historical standpoint with other branches of mathematics which, like the one at present before us, are now scattered throughout scientific periodicals in such a way that their order of development cannot readily be appreciated by a student.

Any attempt to indicate in detail the contents of the present work would hardly be possible here. It is perhaps sufficient to say that it is complete and apparently exhaustive and that the grouping together of related investigations appears throughout to be the most natural one. The most striking developments now taking place are well represented, and in particular comprehensive reference is made to some of the more remarkable theorems obtained quite recently by Landau, Hardy, Littlewood and Tchebychef. The manner in which the further development of the theory of numbers has tended to centre round the properties of the Zeta function will be quite clear to the reader. For an account which is by its nature largely bibliography the work is very pleasant to read, and it should be an indispensable addition to any mathematical library.

DOROTHY WRINCH.

Matrices and Determinoids. Vol. II. By C. E. CULLIS, M.A., Ph.D., Hardinge Professor of Higher Mathematics in the University of Calcutta. [Pp. xxiii + 555.] (Cambridge : at the University Press, 1918. Price 42*s*. net.)

IN view of the far-reaching advantages in the treatment of determinants derived from the use of matrices, it has always been a serious drawback that, in the treatment usually presented in treatises on algebra, the older methods have been

adhered to so strictly. In the present work, which embodies a course of lectures delivered during the tenure of the now well-known Readership in the University of Calcutta, the author confines his attention more especially to rectangular matrices, and treats them in a manner which exhibits their advantages in a comprehensive sense.

It will be remembered that the first volume was devoted to a calculus of matrices in which the only operations were addition, subtraction, and multiplication, together with further sections on the properties of the determinoid of a matrix, and the solution of matrix equations of the first degree. The present work still avoids the special properties of functional matrices, which are to form the subject of a third volume, and is, in essence, concerned with the preparation of the ground for their consideration. A very large part of the treatment is of a geometrical type, which is a great advantage in that some very interesting geometrical applications, of a kind not easily obtained by other methods, can thereby be included. The definitions, it should be remarked, are not geometrical. Chapter XII, which begins the work, is devoted mainly to definitions and notations, and the following chapter exhibits the relations between the elements and the minors of a matrix. The author then considers more special properties of square matrices. These sections are, of course, mainly introductory, and space does not permit of an account of the more considerable parts of the theory developed.

We are very pleased to notice that the author is expecting to be able to publish the third volume at an early date, and so complete the theory in a form suitable for the applications. The present volume worthily maintains the traditions of the Cambridge University Press, and is a most valuable addition to the rapidly growing series of volumes for which the Readership at the University of Calcutta is responsible.

DOROTHY WRINCH.

The Analytical Geometry of the Straight Line and the Circle. By JOHN MILNE, M.A., Senior Mathematical Master, Mackie Academy, Stonehaven. [Pp. xii + 243.] (London: G. Bell & Sons, 1919. Price 5s.)

THIS is the latest volume of "Bell's Mathematical Series for Schools and Colleges," and fulfils its purpose of being a thorough introduction to the formal study of analytical geometry, in which the many illustrative examples and suitable exercises form a very important feature. After a short historical note and an explanation of the idea of co-ordinates, the question of loci is discussed, and then the equation of a straight line, the finding of the angle between two straight lines, the question of gradients, and perpendiculars to a straight line. Then circles and their properties are considered in the case where the origin is the centre. After this, pencils of straight lines, the homogeneous equation of the second degree, the general equation of a circle, tangents and normals of a circle, conjugate points and poles and polars, and orthogonal and coaxial circles are treated.

The historical note is a brief reminder of Ahmes, Thales and a few other ancient Greeks, Kepler, Desargues, and "a Frenchman named Descartes." It is rather misleading to state (p. 2) that "Descartes was the first to perceive that a point could be fixed on a plane by the help of two axes, and that the laws of algebra could then be applied to the solution of geometrical problems." It seems not quite satisfactory to treat geometrical curves as the result of motion under

constraint of a point (pp. 8, 19). This way of proceeding seems to take the mind of the student away from the fact that the essence of analytical geometry is that *many* co-ordinates of points are connected in pairs by *one* fixed equation. Further, it seems rather a debatable point whether the equations of conics should be postponed beyond equations of pencils of lines and systems of circles. Finally, the old-fashioned way of talking of the equation "to" a curve does not sound quite pleasant to modern ears.

PHILIP E. B. JOURDAIN.

ASTRONOMY

Planetary Rotation Periods. By F. A. BLACK, F.R.S.E. [Pp. xiii + 115.] (Edinburgh and London: Gall & Inglis, 1919. Price 3*s*. 6*d*. net.)

THIS small volume bears the sub-heading, "Two Essays on the Relations between the Planets in Diurnal Rotation and in Mass." The author has discovered empirically a relationship between the period of rotation of a planet and the planet's mass and radius, which gives an approximately constant value for the various planets of the solar system. It does not appear that the relationship has any theoretical importance. Bode's empirical law is an example of another somewhat similar relationship. The contents of the volume could have been compressed into some half-dozen pages. Nothing essential is gained, and clarity is lost, by incorporating detailed elementary arithmetical computations for testing the rule; further, the use of symbols and a simple formula would have avoided pages of rules, in which unnecessary distinction is drawn between the larger and the smaller planet. We do not doubt but that the majority of readers will find the book too tedious to wade through it to the end.

H. S J.

PHYSICS

Notes, Problems, and Laboratory Exercises in Mechanics, Sound, Light, Thermo-Mechanics, and Hydraulics. By HALSEY DUNWOODY, Acting Professor of Natural and Experimental Philosophy at the U.S. Military Academy. [Pp. vi + 369, with 241 figures in the text.] (New York: John Wiley & Sons; London: Chapman & Hall, 1917. Price 13*s*. 6*d*. net.)

THIS book would appear to have been compiled rather hurriedly, in order to cope with an inrush of students at the United States Military Academy at the outbreak of war. The compiler states that it is intended "as a reference-book for use with any text on the general subject." No doubt he found it convenient for his special purpose to have such a collection bound up in a single volume, but that hardly justifies its export to this country. The first portion of the book consists of a set of notes on Mechanics, printed by permission of Prof. Cross, of the Massachusetts Institute of Technology. These notes are quite satisfactory as a summary of the contents of a textbook of intermediate character, making use of the notation of the calculus. Next comes a short section dealing in quite ordinary fashion with Graphical Statics; it would appear to be the only part of the book written by Prof. Dunwoody himself. The third section consists of some 900 problems taken

from the stock collections used in various Technical Institutions in the United States. About 600 of these exercises deal with various branches of Mechanics, the remainder with Heat, Light, Sound, and General Physics. They are of all degrees of difficulty, and are arranged under the names of the Institutes from which they are borrowed, and not (primarily) under subject headings. The next section is a reprint from Gordon's Mechanics of the mathematical development of that subject on an energy basis, and finally there is a collection of sixteen laboratory exercises. Thirteen deal with Elementary Mechanics, and are based on the course devised by Prof. Duff for use at Worcester Polytechnic Institute, while the remainder are "original" (?) experiments in Sound and Optics, based on work done at Cornell University.

From this catalogue of contents it will be seen that Prof. Dunwoody's book contains a hotchpotch which is unlikely to obtain a favourable reception at the hands of the English reader.

D. O. W.

CHEMISTRY

Outlines of Theoretical Chemistry. By FREDERICK H. GETMAN, Ph.D., formerly Associate Professor of Chemistry in Bryn Mawr College. Second Edition, revised and enlarged. [Pp. xvii + 539.] (London: Chapman and Hall, Ltd., 1918. Price 16*s.* 6*d.* net.)

THE great difficulty that confronts one, on attempting to summarise the fundamental laws of chemistry, lies in the fact that many of the more important facts of chemistry are incapable of being described in a few words—for instance, the orientation of substituents in the benzene nucleus, the colours, odours, and physiological action of substances, or the real underlying reasons for the differences in the dissociation constants of acids, none of which are at present susceptible of mathematical treatment, so that authors who deal with the subject are of necessity forced to confine themselves to those laws which can be expressed more or less accurately in mathematical form, for which reason it would often be more accurate to describe works, such as that under review, as textbooks or outlines of Theoretical *Physical* Chemistry.

Subject to this reservation, however, we can heartily commend Prof. Getman's work to those teachers seeking for a short and succinct account of the basic physico-chemical laws with which we are at present acquainted. Prof. Getman has taken the opportunity offered by the issue of a second edition of his book to bring it up to date, the chief points of difference from the earlier edition consisting in the introduction of chapters on radio-activity and the modern conception of the atom, the substitution of a chapter on X-rays and crystal structure in place of the chapter on crystallography, the rewriting of the chapter on colloids, the insertion of a short sketch of Perrin's remarkable researches on the Brownian movement and its bearing on molecular reality, the enlargement of the chapter on Electromotive Force, and the addition of a new chapter on Photochemistry.

The work is well and concisely written, and clearly illustrated with numerous diagrams, and the student who has read and understood the book need have no fears of examinations, and can remain assured that he possesses a very real and accurate knowledge of the fundamental physico-chemical laws.

F. A. M.

The Chemistry and Manufacture of Hydrogen. By P. LITHERLAND TEED, A.R.S.M., A.I.M.M., Major R.A.F. [Pp. vii + 152, with 22 figures in the text.] (London: Edward Arnold, 1919. Price 10*s.* 6*d.* net.)

THE unique circumstances of the last four years have stimulated the industrial utilisation of many chemical products which were formerly regarded as merely of theoretical or academic interest, and which, a few years ago, were practically unknown in technological practice. Few of these substances have proved of such value as hydrogen, and probably in no case has the number of applications to industrial processes been greater. The demand for solid fats has been largely met by the production of "unsaturated" oils and fats by means of Sabatier's method of hydrogenation by the aid of catalysts; the Haber process for the synthesis of ammonia from its elements has been utilised in Germany to overcome the shortage of nitrogen compounds required for explosives and fertilisers, while gaseous hydrogen has been largely employed for balloons and airships. Consequently there have been numerous developments in the methods of manufacture of the gas, and a fairly voluminous literature is now to be found in the various technical journals. As this information is very much scattered, and the subject has hitherto received scant treatment in books on chemical technology, the author, in the monograph under review, has endeavoured to provide a remedy by presenting a useful and very readable summary of the present state of knowledge of the subject.

The earlier parts of the book are occupied by chapters dealing with the natural occurrence and chemistry of hydrogen. So far as the former is concerned, it may be pointed out that some of the data given are not altogether reliable owing to the lack of good methods of determining the amount of the gas in rocks and meteorites. Since in most cases complete evolution of the gas is only obtained by heating the solid to high temperatures, and the water, which is almost invariably present, reacts with iron compounds, the percentage of hydrogen found by such analysis does not represent the amount originally present as gas. The chapter on the chemistry of hydrogen, apparently inserted for the sake of completeness, is too short to be of much practical value, and might well have been omitted, as the information contained therein can be obtained from textbooks of general chemistry. Data for certain of the physical constants of the gas are given in an appendix.

The remainder of the book is occupied by an interesting account of the "large scale" methods for the production of the gas. The descriptions of the various processes are well illustrated, and, though concise, are sufficiently detailed to form a valuable source of information. The methods of manufacture are divided into three groups, chemical, chemico-physical, and physical, according to the nature of the principles on which they are based. Since the first of these includes the Silicol, Iron Contact and Badische Catalytic processes, the greatest amount of space is devoted to it. The Linde-Frank-Caro process is the only one of importance in the second group, while physical methods are only used where the required output is relatively small, or where the gas can be obtained as a by-product.

Although the references to the patent and other literature are fairly full, they might have been given in a more systematic form; for example, the page numbers and dates are omitted in many cases. A few misprints occur, particularly in proper names, as, for instance, "Crooks" for "Crookes," and "Fouque" for "Fouqué" on page 7, "Moisson" for "Moissan" on page 19, and "Jacquerod" for "Jaquerod" on page 145. On page 17 the melting-point of copper is errone-

ously given as 1087' C., while on page 75 the "Dellwick-Fleischer" gas producer is described under the heading "English Method."

A. SCOTT.

Fats and Fatty Degeneration: a Physico-Chemical Study of Emulsions and the Normal and Abnormal Distribution of Fat in Protoplasm. By DR. MARTIN H. FISCHER and DR. MARIAN O. HOOKER. [Pp. ix + 155, with illustrations.] (New York: John Wiley & Sons; London: Chapman & Hall, Ltd., 1917. Price 9*s.* 6*d.* net.)

THE title of this book suggests a treatise on pathology, but, as indicated by the subtitle, it contains a critical study of the conditions favouring the production and breaking of emulsions and the application of these conditions to the explanation of the pathological changes observed in fatty degeneration. A large amount of experimental evidence from artificial emulsion is brought forward in support of the argument that fatty degeneration is in reality a physical and not a chemical problem. Thus, for example, the "cloudy swelling" of the pathologists, which is the precursor of fatty degeneration, is explained as the distension resulting from the increased hydration of a lyophilic colloid, while the softening accompanying fatty degeneration is regarded as resulting from the reduction in viscosity following from the breaking of a stiff emulsion. Some of the conclusions arrived at experimentally are applied to explaining the natural formation of milk and also to the production of stable artificial emulsions and milk substitutes. Two sections are devoted to the mimicry of mucoid secretion and anatomical structures respectively. In the latter section several very striking artifacts are described on the lines of Bütschli, Leduc, and others.

In writing this monograph the authors have followed a plan adopted on a previous occasion—namely, of opening with an introductory chapter or "Argument"; it is explained that this contains an abstract of the entire volume, designed for those "who have not time to read the whole book"; this latter phrase may possibly apply to the American reading public, but we doubt whether any serious reader in this country who has perused this chapter will be able to resist the temptation of reading the whole book. We commend the writing of such a summary, but not from the point of view of a time-saver, but rather as a means of whetting the appetite. The whole subject is presented in a most attractive manner, and the book will bear reading more than once, especially as it is not of very large compass. We have only one fault to find; many of the photographic illustrations of glass vessels containing artificial emulsions appear to have been taken with complete disregard for colour contrast, and it looks as though much better results might have been obtained with the use of orthochromatic plates; but this is, after all, only a minor defect in an otherwise most interesting and instructive volume, which should be widely read by all who are in any way interested in the subject of colloids.

P. H.

The Physical Chemistry of the Proteins. By T. BRAILSFORD ROBERTSON, Ph.D., D.Sc., Professor of Biochemistry and Pharmacology in the University of California. [Pp. xv + 483.] (New York: Longmans, Green & Co., 1918. Price 25*s.* net.)

THE key to this book is given in the quotation which the author makes from Sir J. J. Thomson's *Corbuscular Theory of Matter:*

"From the point of view of the physicist, a theory of matter is a policy rather than a creed; its object is to connect or co-ordinate apparently diverse phenomena, and above all to suggest, stimulate, and direct experiment. It ought to furnish a compass which, if followed, will lead the observer farther and farther into previously unexplored regions. Whether these regions will be barren or fertile, experience alone will decide; but, at any rate, one who is guided in this way will travel onward in a definite direction, and will not wander aimlessly to and fro."

Prof. Brailsford Robertson has written a book in which the guiding theory or "policy" is the applicability of the laws of homogeneous solutions (the law of mass action, and the theory of electrolytic dissociation), to the chemical and physical behaviour of that wide and complex group of substances known as the proteins. It is a remarkable attempt at relatively simple generalisation of complex, and indeed apparently conflicting, facts and observations. There is probably no subject which has furnished a greater number of difficulties from the theoretical point of view. Whether Prof. Robertson has unduly simplified the basis of the theoretical treatment has yet to be seen. What he most certainly has done is to bring order out of chaos and regularity out of confusion. That, indeed, is the great and outstanding merit of the treatment which the subject has received at his hands.

Briefly, Prof. Robertson's thesis is that the proteins are essentially electrolytes, and behave like other electrolytes, due regard being paid to the kind of ionisation of which the protein molecule is capable. The fundamental concept has to do with the grouping $-\mathrm{N.HO.C}-$ in the protein molecule. The kind of change which, on Robertson's view, takes place may be illustrated by the action of an alkali and an acid respectively, viz.:

$$-\mathrm{N.HO.C}- \quad + \quad \mathrm{NaOH} \quad = \quad -\overset{\mathrm{H}}{\underset{\mathrm{OH}}{\mathrm{N}''}} \quad + \quad \overset{++}{\mathrm{Na}}.\mathrm{OC}-$$

and $$-\mathrm{N.HO.C}- \quad + \quad \mathrm{HCl} \quad = \quad -\overset{\mathrm{H}}{\underset{\mathrm{Cl}}{\mathrm{N}''}} \quad + \quad \overset{++}{\mathrm{HO}}.\mathrm{C}-$$

That is, in both cases two divalent complex *ions* are produced by the rupture of an internal $-\mathrm{NHOC}-$ group. (Robertson points out that the end amino and carboxyl groups cannot possibly account for the whole physicochemical behaviour of the proteins.)

Starting from this simple concept, we are led to a striking, and indeed convincing, explanation of such problems as the electrochemical behaviour of the proteins in general, the electrochemical equivalent, combining weight as distinct from molecular weight, the magnitude of the velocity of the protein ions in a field of force, the type of union of salts with proteins, *e.g.*, the fact that the compounds of the proteins with an acid or a base are good electrolytes, but do not give the reaction of, say, chlorine ion or sodium ion, the fact that casein, for example, will displace carbonic acid from its union with calcium hydroxide, the osmotic pressure of protein-base compounds, the absence of hydrolytic dissociation of protein salts, and other characteristics of these substances.

A rough idea of the scope of the work is given by an enumeration of the chapter headings: the chemical constitution of the proteins, the preparation of pure proteins, quantitative estimation of the proteins, the compounds of the proteins (four chapters), the formation and dissociation of protein salts, the combining capacity of the proteins, the electrical conductivity of solutions of protein salts, the

electrochemistry of coagulation, phenomena accompanying changes in the state of proteins, physical properties of protein solutions, hydrolysis of polypeptides and of proteins, enzymatic synthesis of proteins. As an appendix there is a useful account of the technique of electromotive force measurements. Throughout the book a concentration cell is, following the physiologists, called a concentration chain, which jars somewhat on the mere physical chemist. There is a misprint on p. 159 and another on p. 367. But these are trivial things. The book is a great one, most stimulating, carrying the reader on. The material is put in such a way as to suggest numerous subjects of research. Without necessarily accepting every one of Prof. Robertson's conclusions, the reader will hardly fail to see that the book stands out as one of the most valuable contributions to applied physical chemistry which has as yet been made.

W. C. McC. Lewis.

The Metals of the Rare Earths. By J. F. Spencer, B.Sc., D.Sc., Ph.D., F.I.C. Monographs of Inorganic and Physical Chemistry. [Pp. x + 279, with diagrams.] (London: Longmans Green & Co, 1919. Price 12*s*. 6*d*. net.)

Although a certain small number of foreign works have been written which deal with the preparation and properties of the Rare Earth metals and their salts, there does not hitherto appear to have been any treatise in the English language on this important section of chemistry.

We are therefore much indebted to Dr. Spencer for his labour and enthusiasm in making available to British chemists the latest work on the subject.

The chapters deal with such matters as the discovery and occurrence of the rare earths in nature, their fractionation and separation, the special properties of the salts and other compounds of the various earths, and the methods adopted for determining their atomic weights, and an interesting section on the practical uses of this group of substances.

A special section is allotted to the consideration of the knotty problem of the position of the rare earths in the periodic system, though one seeks in vain for any mention of Soddy's theory of isotopes, which would seem to be very directly connected with the whole question.

A comprehensive set of indexes is provided, and the fact that the references run into well over 1,000 is an indication of the amount of trouble that has been taken to make the volume really complete. The book should prove of great value to all those interested in the study of the rare earths.

F. A. M.

GEOLOGY

Military Geology and Topography. A Presentation of Certain Phases of Geology, Geography, and Topography for Military Purposes. H. E. Gregory, Editor. Prepared and issued under the auspices of the Division of Geology and Geography, National Research Council, U.S.A. [Pp. xv + 281, with 117 figures.] (New Haven: Yale University Press, 1918. Price $1.25.)

In large measure the Great War has been a contest of brains, and has required the application of all relevant scientific knowledge for the solution of its problems.

In addition to others, geological science made a significant contribution to this end. Although the next war may be fought principally in the air, the main clash of arms in this one took place on the ground, and the geological nature and topographic form of the terrain inevitably assumed importance in the conflict. Knowledge of earth-science was needed for drainage and tunnelling operations; also for the finding and investigation of various raw materials, the demand for which was stimulated by the war. Recognition of these facts by the United States National Research Council led to the production of this textbook. The Germans early recognised the importance of geology in warfare, as shown by the appointment of numerous geologists to their armies for special technical duties, and by the appearance of many articles on "Kriegsgeologie" in their geological journals. The British and French armies followed suit later with the appointment of geologists charged with special duties, but apparently not to the same extent as the Germans.

The book under review is stated to be a preliminary effort with a view to a more complete volume later on; and it shows signs of the haste with which it has been produced in numerous small errors and misprints. Naturally a compilation of this sort tends to produce rather arid reading.

The book contains eight chapters, with the following headings: Rocks and other Earth Materials; Rock Weathering; Streams; Lakes and Swamps; Water Supply; Land Forms; Map Reading and Map Interpretation; Economic Relations and Military Uses of Minerals—a list which gives an idea of the scope of the work. The military applications of each topic are carefully pointed out. These include engineering considerations relating to rocks (i.e. selection of road metals, excavations, etc.), drainage, pollution of soil, water supply, influence of topographic features on warfare, the military uses of maps, and the geology of raw materials necessary to warfare. The concentration of the book on this military end has led to a certain bareness of statement, such as (p. 16) "Coal beds are old buried peat beds," but dogmatism is perhaps unavoidable in books of this kind.

A special word of praise must be given to the illustrations, which consist of well-selected and beautifully reproduced plates, along with special sketches by F. Morris and A. K. Lobeck. The latter are in general excellent and most illustrative; but Fig. 116 (p. 247), alleged to show drumlins and associated swamps, is very slight and uninforming.

On the whole the book is a characteristically American production in the thoroughness and elaboration with which the subject is presented. We believe that military curricula would be strengthened by the study of this book, or by another on the same lines, but more adapted to British military requirements. Finally, we may note the excellent get-up of the book, its clear printing and good paper (as far as war paper goes), combined with a remarkably small price.

G. W. T.

The Analysis of Minerals and Ores of the Rarer Elements. By W. R. SCHOELLER, Ph.D., and A. R. POWELL. [Pp. x + 237.] (London: C. Griffin & Co., 1919. Price 16*s*. net.)

ALTHOUGH the extended use of many of the rarer elements and their compounds in industrial processes has resulted in the publication of several treatises on the chemistry of such substances, no systematic account of the analytical methods used in the examination of their ores has hitherto appeared in English. In the

book under review the authors have endeavoured to fill this gap in the literature by dealing systematically not only with the methods of determination of the forty less common elements but also with the methods of separation of such groups of these as are likely to occur in the various ore-minerals. After a preliminary chapter on mineral-analysis, the elements are considered, in the order of the periodic table, under the following heads: minerals, properties and compounds, quantitative separation, methods of determination, detection and determination in ores, and complete analysis of ores.

The weakest part of the book is the section on mineralogical analysis, despite the fact that the authors emphasize the necessity of a knowledge of mineralogy. The use of heavy liquids, for separation according to specific gravity, and of magnetic separation, together with the application of these in quantitative work, might have been treated in much greater detail. In this section, as well as in the one on radio-active minerals, illustrations of the various types of apparatus would have been useful. No mention is made of microscopical examination, which often affords considerable information in the investigation of "alluvials," and is generally a useful prelude to a chemical analysis. Although lists, showing the minerals in which each element occurs, are given, the information is too scanty to be of much use, since the formulæ given for the minerals are reduced to very simple terms, and there is little indication, either by text or typical analyses, of the nature and proportions of the elements to be expected.

The purely chemical parts of the book, however, are admirable, and should prove very valuable to all chemists engaged in the examination of such materials. In general several methods of determination are given for each element, together with methods of separating the latter from those others likely to occur with it, as well as elaborate and detailed schemes, often in tabular form, for the complete analysis of the various types of minerals. The two latter should be particularly useful, as several of the processes are new, while others are taken from journals not always available in scientific libraries. Numerous references to the literature are given, and the usual tables of "factors" appended. The letterpress is excellent, and misprints are few and unimportant, the chief occurring on pages 48, 62, and 94.

A. SCOTT.

BOTANY AND AGRICULTURE

Science and Fruit Growing. By the DUKE OF BEDFORD, K.G., F.R.S., and SPENCER PICKERING, M.A., F.R.S. [Pp. xxii + 351, with 47 figures and 4 plates.] (London: Macmillan & Co., Ltd., 1919. Price 12*s.* 6*d.* net.)

THE Woburn Experimental Fruit Farm, which was established in 1894, has, under the direction of the authors, attained a more than national reputation. Situated on the Oxford clay with an extent of not more than twenty acres, the achievements have been considerable and form another monument to the debt which the community owes to private enterprise. The writers have, in this, continued the tradition established by their ancestors, Francis Duke of Bedford and Lord Leicester, and in the pages before us present the results of a quarter of a century of patient research. It is always refreshing to study unorthodox views, but when those views are the outcome of carefully controlled experimentation the conclusion is inevitable that the orthodoxy, far from being a real outcome of practical experience, is in fact the cumulative effect of mere tradition that has

degenerated to something akin to superstition. One of the most striking features of the results at the Woburn Fruit Farm is the refutation they provide of many of the cherished doctrines of the professional and amateur pomologist.

The prolonged life of most fruit trees, and the lapse of time before they come into bearing, render them much less amenable to experimental treatment than annual crops with which results are obtained in a single season. For this reason it was not until recent years that in this country or abroad any serious attempts were made to provide scientific justification for the common practices of the fruit-grower. It was in pursuance of this aim that demonstration plots were inaugurated at Woburn to show the deleterious effects of so-called "bad" planting; plots which in the outcome exhibited better growth and gave a better yield than the "controls."

Repetition merely confirmed these results and led to the recognition that not only were many of the niceties of "good" planting a work of supererogation, but that the beneficial effect of "bad" planting was mainly due to the "reprehensible" practice of tightly ramming down the earth immediately around the transplanted tree.

Similarly we learn that pruning tends to diminish both vigour and yield, though mechanical considerations may render pruning essential. Or again, bastard trenching, so much favoured by fruit-farmers, appears to be attended with a very dubious benefit to trees, whilst it is positively harmful to bush fruit.

The results obtained in experiments on the influence of crop on crop and on insecticides and fungicides, if somewhat less sensational, are often at variance with common practice.

It would be impossible, even briefly, to summarise the many aspects of fruit-growing dealt with in these pages. Most of the conclusions are already familiar from having appeared in the Woburn *Reports* or the various periodicals, but the authors have wisely brought these together into what is not only a convenient form but also an extremely fascinating narrative. In future editions the chapter devoted to an elementary account of the structure and functions of plants should be carefully revised. Such statements as that "cortical cells contain granules of green colouring matter, called chlorophyll," or that the so-called cuticular transpiration ceases with the formation of the cuticle, are errors which could be avoided without materially lengthening the description.

With respect to the text as a whole, whatever criticisms may be levelled at the experiments or their interpretation the onus undoubtedly rests on those who uphold time-honoured practices to demonstrate by experiments, on properly controlled lines, that conditions do exist under which those practices are beneficial.

It is with the greatest confidence that we can commend this volume to those interested in fruit-growing either in its theoretical or practical aspects. For the latter especially, its pages will repay the most careful perusal.

E. J. SALISBURY.

The Living Cycads. By Prof. C. J. CHAMBERLAIN. [Pp. xiv + 172, with 91 figures in the text.] (Chicago: University of Chicago Press. Price $1.50 net.)

THERE are probably few groups of plants that can vie with that of the Cycads in their appeal to the imagination either of the biologist or the general reader. Peculiar in their distribution, embracing some of the world's rarest species, and with a geological record that can be traced back to the Palæozoic Pteridosperms

the Cycads stand as a mere remnant of their former glory, endowed with a romance analogous to that which attaches to the concrete remnants of departed civilisations.

Prof. Chamberlain here presents us, not with a textbook, but with a popular account of these fascinating plants, in which he treats of their haunts, their life-history, and the theoretical problems they present. The author can write with more than usual authority on the subject, as having visited Central America, South Africa, and Australia, where he has studied all the living genera in their natural homes. A number of the photographs taken on these travels are here reproduced, representing the entire range of Cycadean habit.

In segregating the subject-matter into three parts, and relegating to the end all questions relating to evolution and phylogeny, we venture to think the author has lost something of the beauty of his theme and an unrivalled opportunity for illustrating in one group several fundamental biological principles. The first section is concerned with the occurrence of the nine genera in relation to their geographical distribution. Apart from the wide area involved, the location of the actual habitats presented some difficulty, and the author relates how the Mexican *Ceratozamia* was located through a specimen in the Park at Vera Cruz, near which police were stationed to question the natives until its source of origin was discovered. After a fortnight this inquisition was successful, and the observant native subsequently acted as Prof. Chamberlain's guide.

The second part of the book constitutes a summary of the more important features of the vegetative and reproductive structures, described with a minimum of terminology and considerable lucidity.

We shall look forward to the extended and more technical consideration of this group upon which the author is now engaged; but in the meantime, as a popular account of a too little known family of plants, the present volume has much to recommend it.

E. J. S.

A Dictionary of the Flowering Plants and Ferns. By J. C. WILLIS, M A, Sc.D., F.R.S. [Pp. lxvii + 712, with 41 figures.] 4th Edition. (Cambridge University Press, 1919. Price 20*s*. net.)

SINCE the first appearance of this work in 1897 it has passed through four editions, and now occupies nearly eight hundred pages of text. Originally published in two parts, the contents of these have been incorporated, in the present edition, into one dictionary, to which is appended a short supplement and keys to the Phanerogamic Families, based on Engler's Classification, and on that of Bentham and Hooker.

The genera omitted in previous editions are now included, so that all those recognised by Linnæus, Bentham and Hooker, Engler and Prantl, as well as those cited in the Index Kewensis and its supplements, find their place in the text. In each case the author is indicated, the family to which the genus belongs, the number of species it embraces, and the general character of the distribution. Some of the more important synonymy is given, as well as familiar vernacular names.

By a happy use of copious cross-references, and numerous abbreviations and signs, a great amount of information is imparted with a minimum expenditure of space. The chief morphological and anatomical characteristics of important

genera and families are indicated, whilst considerable prominence is given to economic uses.

The volume, though forming an invaluable, and, indeed, almost indispensable work of reference for the student and teacher, possesses the inestimable attribute of convenience.

It is especially true of a work of this character that perfection can never be attained, since, even whilst passing through the press, genera are merged and segregated, new facts are brought to light. It is with a full appreciation of these difficulties that we would call attention to one or two passages where revision is desirable. On page 581 we read that the genus *Salicornia* is "leafless," a statement that is not justified even as a "popular" presentation of the peculiar fusion between stem and leaves that here obtains. Again, in describing the Ranunculaceæ the impression is given that extrorse dehiscence of the stamens characterises the group as a whole, whereas, actually, as pointed out long ago by Baillon, there is in this family almost every gradation between the completely extrorse condition exemplified by *Ranunculus* and the completely introrse of *Eranthis* or *Pæonia.*

The statement (p. 544) that the prothallus of *Psilotum* "has not been seen" was perhaps correct when the MS. went to press, though it is now over two years since Lawson's description was published.

In the article on nomenclature there seems to be an implication that the difference between a *form* and a sub-variety or variety is one of degree and not of kind, but this appellation should, of course, be restricted to states known or presumed to be due to habitat conditions.

But the existence of errors is almost inevitable in a work of this character, and it is far easier to criticise than to do adequate justice to its many merits.

The work is one that should be in the hands of every student, for it belongs to the small class of the well-nigh indispensable.

E. J. S.

Vegetable Growing. By J. G. BOYLE, B.S., M.S. [Pp. ix + 334, with 154 figures.] (Philadelphia and New York: Lea and Febiger, 1917.)

THE professed aim of this addition to the numerous works on the culture of vegetables is to show "the connection between principles and practice." There are certainly few occupations where empiricism is more rife, or where the practical man is so often at a loss to explain his success.

The apparently irreconcilable experiences of acknowledged experts often testifies to a high standard of technique divorced from a corresponding theoretical knowledge.

It is particularly with respect to the character of the soil and the use of fertilisers that ignorance of fundamental principles appears most widespread. Unfortunately, in the work before us it is these very subjects which receive but scanty treatment. Artificial manures are dealt with in less than two pages, and the soil, as such, is disposed of in a few paragraphs. The chapter entitled "Insect and Disease Control" occupies seventeen pages, but attempts in this limited space to cover the subjects of animal and plant parasites, and the whole question of sprays and their utilisation.

The earlier part of the text is mainly concerned with cultural methods, whilst

the bulk is occupied by a consideration of the individual types of vegetables and their particular needs.

Those little details which are so helpful to the amateur, and for which even the professional may sometimes consult a text-book, are apt to be omitted. For example, tomato culture, which is treated of at length, contains no directions as to the pinching out of buds, or the desirability, under certain conditions, of reducing the foliage.

A good deal of information is contained between the two covers, but there is an absence of that thoroughness of treatment which is so essential if practice is to be based on a sound knowledge of principles.

E. J. S.

Strawberry Growing. By S. W. FLETCHER, Professor of Horticulture at the Pennsylvania State College. Rural Science Series. [Pp. xxii + 325.] (New York: The Macmillan Company, 1917. Price $1.75 net.)

THIS book is a review of the present position of the strawberry industry and a thorough practical guide to strawberry cultivation as it is practised in North America. The first chapters deal with questions of cultivation, such as planting, rotation, tillage, training, mulching, pollination, etc. Then follow chapters on picking and packing, markets, and cost of production, and finally a discussion of certain cultural questions: propagation, varieties, forcing, and other special methods of culture, and diseases. Statistics confined to North America form the subject of an appendix.

The treatment throughout is thorough, and, with the author's companion on *The Strawberry in North America*, reviewed in the previous number of SCIENCE PROGRESS, the whole field of the strawberry industry is covered. The handling of the soil, the plant and the pickers are all equally well dealt with. It becomes clear from these volumes that the strawberry industry in America differs in many particulars from the industry in Britain. This can undoubtedly be traced back very largely to the greater amount of land available for cultivation in America, with the greater distances between consuming populations, and on the whole less intense cultivation. Some differences can scarcely be explained on this ground, as, for instance, the fact that in England only hermaphrodite varieties of strawberries are employed, while in North America unisexual female plants are also used. The latter varieties appear, however, to be gradually disappearing.

In the matter of pollination the author states, on the authority of Ewert, that "parthenogenesis, or the production of fruit without fertilisation, is common in the strawberry." It is not clear, however, from this statement, whether it is really parthenogenesis or the much commoner phenomenon parthenocarpy, which is meant. The observation that "It cannot be denied that, occasionally, the character of the fruit may be influenced very slightly by the kind of pollen used," is of great importance from a purely scientific point of view, and deserves further investigation, even if instances are rare.

Although the book only aims at describing commercial strawberry practice in North America, it will be read with the highest interest by those in this country who are interested in the strawberry.

W. S.

The Sugar-beet in America. By F. S. HARRIS, Ph.D., Director and Agronomist, Utah Agricultural Experiment Station, and Professor of Agronomy, Utah Agricultural College. Rural Science Series. [Pp. xviii + 342.] (New York: The Macmillan Company, 1919. Price $2.25 net.)

THE annual consumption of sugar throughout the world has increased within the last century from about a million tons to twenty times this quantity. Of this amount somewhat less than half is obtained from the sugar-beet, so that the sugar-beet industry is one of considerable importance in regard to food supply, and this account of it by Prof. Harris will be read with much interest.

The beet-sugar industry consists of three distinct parts: the production of beet seed, beet-growing, and sugar-making. The present volume is chiefly devoted to sugar-beet growing, and a full practical account is given of the agricultural practice of beet raising in North America in all its aspects. Special attention should be directed to the very interesting chapter on the production of beet-seed. There is also a short chapter on sugar-making in which the principles of this part of the beet-sugar industry are set out, and another on sugar-cane. The principles underlying sugar-beet growing differ in many important details from those underlying the raising of most other crops: thus, for instance, in the matter of seed in the case of a cereal all the crop raised from it can be used whether the yield is high or low; but in the case of beet a low yield of sugar in the root may mean that it is impossible to extract the sugar at a profit, and the crop is valueless for the purpose for which it was raised. It is shown how these special considerations which apply to beet raising necessitate particularly good farming, and so the general level of agricultural practice is raised. For this and other reasons, such as the stability it gives to agriculture, its educational value, and the greater degree of national independence it ensures, the author is enthusiastic over the value of the sugar-beet industry for the welfare of the community.

On page 23 the author gives the native habitat of *Beta maritima* as the Mediterranean. As a matter of fact, this plant is not only common on the English coasts but extends as far north as the Baltic. This is an inexactitude which occurs elsewhere in American writings. In Appendix A there is given a bibliography containing a list of some 250 papers relating to the sugar-beet. This is, of course, a valuable collection of references, but it is regrettable that they should all be American, as the greatest amount of work on the sugar-beet has been done in Europe, and most of this will have some bearing on sugar-beets independently of where they are grown. Although a few expressions, such as "Considerable of the remainder came from Russia through Siberia," may not sound very happy to the English ear, the book is brightly written, and maintains the standard of the series to which it belongs.

W. S.

The Carbohydrate Economy of Cacti. By HERMAN AUGUSTUS SPOEHR. [Pp. 79.] (Washington: Carnegie Institution of Washington, 1919.)

THE experimental attack on the problems of carbon assimilation has been made in several well-defined and rather distinct ways. There is, for example, the purely chemical method of Willstätter directed to an understanding of the chemistry of the leaf pigments, the rather physico-chemical method of F. F. Blackman, which aims at investigating quantitatively the influence of the different factors necessary for carbon assimilation, and there is the chemical

examination of the products of assimilation. The work under review falls in the third of the three categories mentioned. In his preface the author states that before a rational discussion of the manner in which sugars are formed in the green leaf is possible a clearer understanding of the conditions governing the equilibria and mutual transformations of the carbohydrates of the leaf is necessary. To this end he has attempted to determine the carbohydrates of cacti and to examine the transformations these carbohydrates undergo under various conditions.

In the first chapter there is a very stimulating discussion on the transformations that sugars undergo *in vitro* under various conditions, and the bearing of the facts examined on the problems in the plant. The author then proceeds to describe his experimental methods. These have evidently been worked out with great care, and, when one remembers the difficulties always associated with the determination of a number of substances of similar properties in presence of one another, one must congratulate the author on the measure of success that has attended his efforts.

The application of these methods has shown the presence in cacti of pentoses, monosaccharide hexoses, disaccharides, pentosans, and polysaccharide hexoses. The various quantities of these substances were determined in cacti at different seasons and under different experimental conditions; a further analysis of the carbohydrates was not attempted. The general conclusions drawn are that low water content and high temperature are associated with relative increase of polysaccharides and pentosans and decrease of monosaccharides, while high water content and lower temperatures are associated with relative increase of monosaccharides and relative decrease of the more complex pentosans and polysaccharides.

Some observations were also made on aerobic and anaerobic respiration of cacti. Under both conditions carbohydrates are consumed. In the course of normal respiration in the dark there is a large production of malic acid as a result of the restricted oxygen supply in these plants; the alcohol production is, however, small. During illumination the malic acid is disintegrated to carbon dioxide and ethyl alcohol, the latter accumulating. Under anaerobic conditions little acid is produced, but, on the other hand, large quantities of alcohol.

The work concludes with a short discussion on the origin and rôle of pentose sugars. The author appears to regard these as probably derived from more complex sugars through glucuronic acid, which is present in cacti and which should undergo photolysis in light into *l*-xylose and carbon dioxide in the same way that malic acid gives ethyl alcohol and carbon dioxide.

This work appears to be a sound and reliable contribution to our knowledge of plant metabolism, and it is to be hoped the author will continue the prosecution of his researches into the chemistry of carbon assimilation.

W. S.

Botany for Agricultural Students. By JOHN N. MARTIN, Professor of Botany at the Iowa State College of Agriculture and Mechanic Arts. [Pp. x + 585.] (New York: John Wiley & Sons; London: Chapman & Hall, 1919. Price 11*s*. 6*d*. net.)

IN this book the fundamental principles of botany are set out and related to subjects of economic importance such as agriculture, horticulture and forestry.

The book is divided into two parts—the first dealing with the morphology and physiology of the flowering plant, the second with the different types of plants met with throughout the vegetable kingdom. Questions of classification and genetics are included in the second part.

It is usual to commence a treatment of the general morphology and physiology of the flowering plant either with a description of the seed or with that of the adult sporophyte. Our author, however, commences with the flower. As this is in many respects the most complex of plant organs, one feels a certain prejudice against commencing the study of the plant at this point in the life cycle, though there is no logical reason why any particular point in the life cycle of the plant should not be used as a starting-point for its study.

The book is not by any means overloaded with anatomy, emphasis being rather laid on the biological and physiological sides of the subject. This is all to the good in an elementary text-book on botany, especially one that is intended for the use of agricultural students.

A few of the statements on physiological matters are not altogether satisfactory, as for instance the statement that "There are various kinds of sugar, but there is considerable evidence that grape sugar, having the formula $C_6H_{12}O_6$, is the chief one formed in leaves. From this sugar as a basis other kinds of sugar, of which cane sugar ($C_{12}H_{22}O_{11}$) is a common one, can be formed by minor chemical changes."

The wealth of illustrations, of which there are 488, adds to the value of the book. If these illustrations are not in all cases of high technical quality they are nearly always clear. The insertion of some of them seems scarcely necessary, as for instance that of a chickadee carrying a fruit in its beak and that of a cow's tail loaded with weeds. Even a first-year student in agricultural botany ought to be capable of visualising these without the aid of illustrations.

W. S.

The Preparation of Substances important in Agriculture: A Laboratory Manual of Synthetic Agricultural Chemistry. By CHARLES A. PETERS, Ph.D. Third Edition. [Pp. vi + 81.] (London: Chapman & Hall; New York: John Wiley & Sons, 1919. 4*s*. net.)

A KNOWLEDGE of the composition of the more important substances employed in agriculture and of their modes of preparation is almost essential both to their intelligent use and a proper recognition of their qualities and commercial value. The volume before us aims at supplying this information to the agricultural student by means of a series of laboratory exercises in the preparation of superphosphate, sulphate of ammonia, potassium salts, lead salts, Bordeaux mixture, etc. For each substance the chemical nature of the steps involved is explained, together with notes on special points and the practical applications.

Questions are added which should help to impress the salient facts upon the student, whilst the practical work itself is so arranged as to furnish an exercise in some of the more important chemical principles.

To English and many American readers the use of simplified spelling will prove a hindrance rather than a help; but the fact that Prof. Peters' work has already reached a third edition is a testimony to its practical utility.

E. J. S.

ZOOLOGY

Upon the Inheritance of Acquired Characters: A Hypothesis of Heredity, Development, and Assimilation. By EUGENIO RIGNANO. Authorised English Translation by BASIL C. H. HARVEY, Assistant Professor of Anatomy, University of Chicago. With an Appendix upon the Mnemonic Origin and Nature of the Affective or Natural Tendencies. [Pp. 413.] (Chicago: The Open Court Publishing Company, 1911. Price $2 net.)

THIS work on a fascinating subject, by the distinguished editor of *Scientia*, should have attracted more attention than it has in this country since its appearance in English eight years ago.

The author's main object is to elaborate a theory of development which he terms "centro-epigenesis," the essential conception in which is that there is a central zone of development which exerts an infinite number of different influences on the rest of the organism "by activating successively a regular series of specific energies, each remaining in a potential state up to the time of its activation." A subsidiary hypothesis to this main one is that each "specific nervous current" from the central zone deposits a definite substance, and that this process is reversible; that is, this substance after deposition can provoke exclusively "the same specificity of current" as that which deposited it. The author brings to the support of these hypotheses a quantity of experimental work on development and regeneration, drawing very largely on the work of Roux.

With these hypotheses as a basis the author explains on the one hand the inheritance of acquired characters, and on the other mnemonic phenomena, "in the widest sense of the word." The book consisting thus of the presentation of hypotheses, the acceptance or rejection of these must depend on the judgment of the reader as to whether known facts support or contradict them, for the questions with which they deal are far too subtle for the hypotheses to be put to direct experimental proof. Probably the difficulties in the way of acceptance of the hypotheses will appear greater to students of plants than to zoologists. Thus, the author is forced to make assumptions in the case of plants such as that "one must regard the leaf as the true individual, and one must attribute to it a centro-epigenesis of its own"—a view which the student of the living plant will most certainly reject. On the whole one feels that the author has been led to put forward his hypotheses from a consideration of animals, and his ideas have been applied to plants as well in order to speak of the organism in general.

But whether the hypotheses are accepted or rejected, the book will be read with great interest, both on account of the ingenuity of the writer in dealing with the facts he cites in support of his hypotheses, and for his shrewd criticism of pre-existing theories of development and heredity. He is, perhaps, at his best in his trenchant criticism of Weismann, and in his discussion of the alleged cases of inheritance of acquired characters, on which question he writes with great fairness. The remarks on what constitutes an acquired character are especially noteworthy.

It must be admitted that the language is sometimes vague. Thus, "phenomena of nervous nature, in the widest sense of the word," might include almost anything. Also, the author shows too much respect for the clever aphorisms of which some writers of the past generation were unduly fond. Thus, Huxley's definition of a plant as "an animal shut up in a wooden box" is a thing to be forgotten—not to be quoted with approval. And to describe Haeckel's dogma that ontogeny is a recapitulation of phylogeny as "the fundamental biogenetic law" is, to put it mildly, an exaggeration. In the plant kingdom it is rather the exception

than the rule for this "law" to hold, and this is reflected in the little use embryology has been in the manufacture of plant phylogenies.

It is a pity that the remarks on Xenia, on p. 74, should have crept into the book. The explanation of this phenomenon, based on double fertilisation, is so convincing and well-known that the explanation of "the heretofore puzzling phenomena of the Xenia" put forward by the author appears very curious.

W. S.

Textbook of Embryology. Vol. II, Vertebrata with the Exception of Mammalia. By J. GRAHAM KERR, Regius Professor of Zoology in the University of Glasgow. [Pp. ix + 591, with 254 illustrations.] (Macmillan & Co., 1919. Price 31*s*. 6*d*. net.)

THIS is the second volume of this "Textbook," following the first by Prof. MacBride. In the first place we must say immediately that this is undoubtedly a great work, and a worthy mate to the previous volume, and this opinion is forced on one even though there may be parts of the volume with which one is obliged to find fault. The Introduction is poor and skimpy; we do not consider that such a book should have been written without a better and fuller treatment of vertebrate gametogenesis. The first thirty pages are taken up with the description of various forms of segmentation in Vertebrata, then follow some pages on gastrulation. The author concludes that the primitive streak of birds represents the line of coalescence of the gastrular lips just as it actually is in reptiles, and that the neurenteric canal which transverses the primitive streak of the tern, duck, goose, etc., represents a persisting portion of the once slit-like gastrular mouth which is otherwise obliterated.

The origin of the mesoderm is then discussed. Prof. Kerr gives what we consider a very clear account of the subject. We cannot, however, agree with him when he claims that the region of continuity of the mesoderm with the endoderm in forms such as Rana is ventral, and not dorsal, as Hertwig and most other embryologists believe. The question is one of some importance, and the reviewer cannot see how Prof. Kerr can uphold his tenets if he compares his own figure 34A with his figure 33A. The chapter on skin and its derivatives is interesting, but surely more space might have been given to the development of the nervous system? The treatment of the subject of the alimentary canal is nicely balanced.

With reference to the renal organs, the author has a peculiar modified view, which seems to us very perverse. Most workers agree that Goodrich showed clearly that in Annelids the nephridial tube and cœlomic funnel were at first distinct and separate structures with separate external openings, and that in certain cases subsequently a more or less intimate interconnexion between the funnel and the tube came about. Prof. Kerr considers that in all probability the funnel and tube were originally connected, and later on tended to separate from one another. We consider that the author does not fully appreciate the evidence adduced by Goodrich. The subject of the migration of germ-cells in early development has been enriched by the splendid work of John Beard, Allen, etc., and one might have expected some sort of proper treatment of the view of the early segregation of the germ-cells. What we do find is that Prof. Kerr has merely stated that the "evidence (for such views) has not as yet, in the present writer's opinion, reached a stage of being convincing." While we ourselves agree with this statement fully, we consider something more might have been given in this volume. Space does not allow us to comment much further on the Professor's

fine book, but we cannot leave the subject without saying that his modified Gegenbaurian view as to the origin of the paired limbs is peculiar. He considers that the paired limbs arose phylogenetically from external gills, and rejects Balfour's lateral fin-fold hypothesis.

The latter part of the book is occupied by a discussion of a number of the problems relating especially to the embryology of the Vertebrata. We do not think the author is very happy in his discussion on recapitulation, especially in his remarks on trochospheres and nauplii on p. 492. The end of the book is formed by a chapter on the practical study of the embryology of the fowl; in such a textbook we consider this telescoped account superfluous. All embryological libraries contain the books of Lillie and Marshall, and it would have been better had this space been used for other purposes. Naturally, the forms Lepidosiren and Protopterus come in for a good deal of attention—we consider, however, with advantage. The illustrations and printing are extremely good.

This is a very fine piece of work, and our congratulations are offered to Prof. Graham Kerr.

J. Bronté Gatenby.

World Power and Evolution. By Ellsworth Huntington, Yale University. [Pp. 9 + 287, with 30 illustrations.] (New Haven: Yale University Press, 1919. Price 10s. 6d. net.)

This is a very interesting work, dealing with Health, Climate, and general environmental conditions in the evolution of Nations and Races. The author has travelled in many of the countries he talks about, and so has first-hand evidence. Here is a good specimen of the author's method of attacking the problem: "The human animal now rides instead of walks; lives in stuffy houses instead of out-of-doors or in caves; wears clothes instead of exposing his body to the weather; and eats soft, cooked, concentrated food instead of that which is raw, tough, and bulky. He preserves the sick and weakly instead of letting them die; he permits an economic and social system which causes the people with the greatest mental power to have fewest children, while the stupid breed like rabbits. . . ." He also gives statistics showing that even in enlightened America, with its advanced legislation, an immense amount of physical defects is found in the population. He gives curves showing how business is connected with, and sensitive to, national health. He brings into his discussions an extensive knowledge of biological problems. In his treatment of the subject of "New Types among Men," he considers that America, with its polyglot "hybrid" population, is more likely to produce genius than a country with a pure population; but he also considers that, while such variability promises men of genius, it also promises an equal number of exceptionally low and dangerous types. His apologia for Turkish brutality does not convince. It is not the climate or stupidity that makes the Turks assassinate Christians—it is pure savagery and unharnessed viciousness. Some may consider that Dr. Huntington has insisted far too much on the effects of environmental conditions such as climate; we do not think the author has erred far in this way, and we feel that he has proven his case very completely. Of the great cities—London, Paris, Berlin, and New York—the author shows that the first-named city has the smallest difference between highest and lowest monthly mean temperatures, and is one of the most favourable in the world for producing a healthy race. In his treatment of the United States he considers that only one-half of the population lives in a highly stimulating climate.

This is undoubtedly a very interesting book, and one which we consider of value to the student of Man and Modern Civilisation. It is daringly written, and is bound to stimulate interest in the various problems the author has discussed.

J. BRONTÉ GATENBY.

Heredity. By Prof. J. ARTHUR THOMSON, M.A., LL.D., Professor of Natural History in the University of Aberdeen. Third Edition. [Pp. ix + 627, with 47 illustrations.] (London: John Murray, 1919. Price 15*s.* net.)

THIS is the third edition of Prof. Thomson's book The author is such a great authority on all aspects of heredity that one hardly needs to comment on this new edition. For one who wishes to have a reliable work on Heredity and Sex, and especially on the philosophy of these subjects, Prof. Thomson's book is recommended. In the present edition a great deal of the newer evidence on Heredity has been introduced, and the Professor has managed to deal with several very bitterly debatable questions with fairness. We consider the cytology rather weak, but we know of no other book of such wonderful value to the lecturer and student. It is a colossal mine of learning in all matters relating to Heredity, Sex, and Mendelism.

J. BRONTÉ GATENBY.

The Elementary Nervous System. Monographs on Experimental Biology, By PROF. G. H. PARKER, Sc.D. [Pp. 229, with 53 illustrations.] (Philadelphia and London: J. B. Lippincott Company, 1919. Price $2.50 net.) [Second Review.]

PROF. PARKER'S papers have already established his reputation as an authority on the nervous system of the lower animals, particularly on that of the Coelenterates and Porifera. The present work, being the second of the Monographs on Experimental Biology issued under the editorship of Jacques Loeb, T. H. Morgan, and W. J. V. Osterhout, makes accessible, in a very readable form, the general conclusions he has reached, and reviews the evidence by which he has reached them.

The subject is one that has received a certain amount of attention previously, but scarcely as much as its interest and importance warranted, from workers who have approached it from either the anatomical or the physiological point of view, but rarely both. Prof. Parker has used both methods side by side with striking success, and so gives another example of the importance of not separating the two methods of approach which should, of course, go hand in hand in all biological inquiry.

Starting with the lowest of the Metazoa (the Sponges), the motor mechanism is described, and we find in them, as in lowly Coelenterates, that the unit is a "primitive type of muscle tissue . . . unaccompanied with nervous elements" This simple tissue is capable of receiving and responding to external stimuli. In some of the Coelenterates this simple mechanism has advanced to the extent of the epithelio-muscular cell being differentiated into two portions. One part of the cell, situated externally, is receptive in function, and the other internal part is contractile. Next, in the higher Coelenterates, we find a receptor element (the sensory cell) and a separate effector element (the muscular cell). Still, in the

same Phylum we encounter the introduction of another element, termed by the author a "protoneurone," interpolated between the receptor and the muscle cells. It is the appearance of this third element that renders possible the development of the central nervous system in both Invertebrate and Chordate types. In this final type we find that the central nervous system serves as an adjustor.

The nervous systems of higher animals are also dealt with to some extent, even including a brief indication of the main characteristics of that of the Chordata.

The book is well written and, as will be seen from the foregoing brief summary of its contents, one of great importance in all work dealing with the experimental investigation of the behaviour of the lower animals. In addition, it is a distinct contribution to our knowledge, both of the function and structure of the lower organisms, and presents another and fresh aspect of our conception of the nature of nervous control in animals. It should find a place in the library of all biologists.

C. H. O'D.

MEDICINE

The Human Skeleton: an Interpretation. By HERBERT EUGENE WALTER, Associate Professor of Biology, Brown University. [Pp. xv + 214, with 175 illustrations.] (New York: The Macmillan Company, 1918. Price 10*s*. net.)

ALTHOUGH its title would seem to indicate that this is merely another book dealing with human osteology, it is in reality both something more and something less than a text-book for medical students. After discussing the evolution of the vertebrate skeleton, the author proceeds to describe, very briefly it must be confessed, the individual bones of the human skeleton and to outline their morphology. Written throughout in an attractive manner, the book contains many delightful and unexpected expressions, such as the summary of the different characters assumed by the tail (p. 78).

It is important, however, that a book of this type, written principally for the lay public, should avoid the possibility of creating misconceptions, and, in this respect, the book cannot be said to be without fault. Such statements as "the human arm is a foreleg which has become emancipated from the work of locomotion and support," and "nor are defensive outriggers, like horns, any longer necessary, because the swinging arms take the place of such organs of defence," can only give to the layman the erroneous impression that some unnamed hoofed and horned monster stands in the direct line of man's ancestry. It may, further, reasonably be feared that the frontispiece, which is described in the preface as graphically expressing the *motif* of the book, will be interpreted as meaning that the human skeleton is an adaptation of the skeleton of the horse or vice versa.

Further, one expected to find more frequent references to palæontological zoology, particularly in the sections dealing with the evolution of the limbs and limb-girdles.

Despite these omissions and blemishes, the book may be heartily recommended to all who, for any reason whatsoever, are interested in human anatomy. It would be especially interesting to the thoughtful medical student after he has completed a year's dissections, for by that time he has forgotten much of his biology, and has acquired an interest in the human skeleton which few books, of convenient size, do anything to satisfy.

T. B. J.

ENGINEERING

A Manual of Machine Design. By FRANK CASTLE, M.I.Mech.E. [Pp. x + 352, with 207 figures and illustrations.] (London: Macmillan & Co., 1919. Price 7*s*. 6*d*.)

THIS book will be hailed with delight by the hundreds of technical teachers throughout the country. It has the distinct merit of being written by a teacher of great technical standing for students who are bound hand and foot by examination results. It is true that the Board of Education has made efforts to break down the examination bogey, but it has not carried its efforts to a logical conclusion. While it abolishes the greater number of its own examinations and encourages committees and teachers to conduct their own affairs, it allows, without at least a public protest, numerous bodies, unions, and associations, to be established, with the one aim and object of conducting examinations.

When the students of technical institutions are examined under conditions that obtain in our teaching universities the uses of Mr. Castle's book will be practically nil, and teachers will fall back once more on those types of books which point clearly to the value of research, to the value of experience, which make students think, and which, more than anything else, indicate that two designs, totally different, may accomplish a set end and both be economical, efficient, and theoretically and practically sound.

Such books also indicate that sound design invariably leads to a certain degree of standardisation chiefly owing to limitations imposed and conditions to be fulfilled.

A student knowing Mr. Castle's book thoroughly would, without any question, pass certain examinations with credit. Therein, as we have said, lies the merit of the book.

The same student would more than likely prove a distinct trouble in the drawing office. Take a few examples.

A young designer is asked to design a knuckle joint as given in Example No. 6, p. 71. The rod works out to 1·78 in. diameter, or $1\frac{13}{16}$ in. There is not a suggestion of the young designer looking up makers' sizes to see if $1\frac{13}{16}$ in. is a stock size of rod, much less looking up the stock-book of the particular firm concerned.

Now $1\frac{13}{16}$ in. is of course rolled, and would, *perhaps*, be in stock at the makers', but the chances are that $1\frac{7}{8}$ in. would prove the nearest local size. Not a suggestion of this important point is given in the book.

Then, again, Example 16, p. 113, is for the design of a rolled steel girder to carry a brick wall over a 20 ft. clear span.

The design finishes with a flange 6 in. wide. Not a hint is given as to the safe pressure per square foot for brickwork, or as to the wisdom, or otherwise, of carrying a brick wall on a surface 6 in. wide.

The last example we propose to take is the question of the design of bolts. Here the product of Mr. Castle's book may be really dangerous. Working stresses in the examples given vary between 3,000 lb. and 5,000 lb. per sq. in.

Not a word of warning is given that for a black $\frac{5}{8}$ in. diameter bolt on a rough joint (a kind frequently used in practice) the lower value (3,000 lb. per sq. in.) would be much too high—indeed, Mr. Castle does not discriminate between rough and finished joints nor between large and small bolts.

It is with relief that we think our serious designers have been guided by Unwin and Seaton, and we suggest that in the revision of the next edition the

author should have the assistance of an engineer who has been responsible for some actual designs.

J. WEMYSS ANDERSON.

Irrigation Engineering. By ARTHUR POWELL DAVIS, D.Sc., and HERBERT M. WILSON, C.E. [Pp. xxiv + 640, with 249 figures and lix tables.) (New York: John Wiley & Sons; London: Chapman & Hall, 1919. Price 21*s*. net.)

So much has been written lately on the "education of the engineer"—in which neither of the terms, "education" nor "engineer," have been understood, much less defined, that it is a positive recreation to read the preface to this volume (seventh edition).

The engineer, it clearly points out, must be capable of grasping the social, political, and economic conditions of the country in which he works.

Irrigation practice differs, and differs very widely in different countries, not only on account of the customs and labour available, but on account of the difference in the soils, plant life, and food, and the duty of water on the varying lands and plants.

Then again, a number of other important points have to be considered, an outstanding example of which is given on p. 185: "It is said that large areas in India, formerly subject to occasional famine caused by failure of crops on account of drouth, were relieved by irrigation works to counteract the drouths, only to find that the mortality from malaria induced by the marshes resulting from irrigation was greater than that formerly due to famine."

The reviser of this edition (Dr. Davis) has succeeded in incorporating in his work all the technical details necessary to the irrigating engineer, together with an ample chemical, geological, and botanical treatment. References and guidance are given to those who wish to dig wider and deeper into any one of the numerous subdivisions into which irrigation engineering is divided.

Now that our men of pure science have realised what a field of investigation the engineer opens out to them, and how much the engineer would be indebted to pure science for better and deeper guidance, we have no hesitation in saying that irrigation forms an ideal example of such a field, and that this volume would form an excellent guide as to where further investigation is needed. Can any one deny that the medical man and the engineer, working together, could not prevent famines, without fear of malaria?

Finally, we venture once more to ask why all the best engineering books are now coming from U.S.A.?

J. WEMYSS ANDERSON.

Applied Mechanics. Vol. II.: Strength of Materials. By CHARLES E. FULLER, S.B., and WILLIAM A. JOHNSTON, S.B. [Pp. xii + 556, with 282 figures.] (New York: John Wiley & Sons; London: Chapman & Hall, 1919. Price 17*s*. 6*d*. net.)

A CASUAL glance at this volume would, no doubt, result in it being classed with the many excellent books on the strength of materials at present on the market. Closer study, however, indicates that the authors' claim that "considerable attention

has been given to the logical development of the subject, and care has been taken to point out the limitations of the different theories; emphasis being laid on the divergence of the conditions met in practice from the ideal conditions under which the theoretical formulas are deduced, and on modifications necessary, or advisable, when the formulas are used under ordinary working conditions"—is more than justified.

Indeed, these points in particular, no less than the general treatment throughout, stamp the volume as being in a class by itself.

With very little addition the work carried out on such sound lines "for the use of students in the Departments of Engineering in the Massachusetts Institute of Technology" would be of the greatest value in the library of the engineer.

The chief addition would be with respect to (*a*) the actual testing of materials, (*b*) working stresses.

Thus, "hardness" is defined on p. 2. No mention, however, is made of the methods of testing hardness. There can be no question that an outline of the chief tests of materials, in a book on "the strength of materials," would be of value to the student as well as to the engineer. In the same way a tabulation of "working stresses" and the methods of determining same would be a great help to the designer in college no less than to the more mature designer in practice. We contend that there are many important points discussed in this volume the full significance of which the mere student could not be expected to realise—more particularly, those modifications of theory so necessary in practice. The engineers, in practice, would welcome the help given them on these points, which form so strong a feature of the book. We have no hesitation in recommending the volume to students and engineers alike.

J. Wemyss Anderson.

MISCELLANEOUS

A Century of Science in America. By Edward Salisbury Dana and other writers. [Pp. xi + 458, with 22 portrait plates.] (New Haven: Yale University Press; London: Oxford University Press, 1918. Price 17*s*. net.)

This volume contains the fourteenth course of the Silliman Memorial Lectures, which were inaugurated in 1901 by the Corporation of Yale University when it came into possession of the legacy of $80,000 given by the children of Mrs. Hepea Ely Silliman, in 1883, as a memorial of their mother. It commemorates the hundredth anniversary of the foundation of the *American Journal of Science* by Benjamin Silliman, who at the time occupied the chair of chemistry and mineralogy at Yale College.

The first chapter of the book contains an account of the history of this *Journal*. It appears that its inception was due to Colonel George Gibbs, whose famous collection of minerals was purchased by Yale in 1825. Benjamin Silliman rather reluctantly consented to act as editor, for he recognised the difficulties which then confronted the holder of such an office and the demands it would make on his time. He had a twofold problem to solve: not only had an audience to be found among a public little interested in science, but also material to fill his pages. The original "Plan of Work" was for these reasons most comprehensive, including not merely all branches of science, but all the applied arts as well. Just at first progress was very slow, and in his preface to the fourth volume the editor, who had by now assumed pecuniary responsibility for his

Journal, stated that "the work has not even yet reimbursed its expenses (we speak not of editorial or business compensation); we intend that it has not paid for the paper, printing, and engraving." The deficit, however, had ceased to increase, and the fifth volume closed with the assurance that the *Journal* would live. Indeed, its prosperity so increased that in 1829 it was found possible to pay for original contributions, a privilege which the editor to-day finds far beyond his means. As the years passed by Silliman received more and more help from his son, and in 1838 added his name as co-editor on the title-page. A little later, in 1846, James Dwight Dana, his son-in-law and father of the present editor, joined the editorial staff, ultimately to continue as editor until 1890.

The preface to the volume beginning in 1847 is so characteristic of the elder Silliman's style and outlook on life that quotation here may perhaps be permitted:

> "In tracing back the associations of many gone-by years, a host of thoughts rush in, and pensive remembrance of the dead who have laboured with us casts deep shadows into the vista with which we view the past. Anticipation of the hour of discharge, when our summons shall arrive, gives sobriety to thought and checks the confidence which health and continued power to act might naturally inspire, were we not reproved, almost every day, by the death of some coeval, co-worker, companion, friend, or patron. This very hour is saddened by such an event, but we will continue to labour on, and strive to be found at our post of duty, until there is nothing more for us to do; trusting our hopes for a future life in the hands of Him Who placed us in the midst of the splendid garniture of this lower world, and Who has made not less ample provision for another and a better."

The reverent sincerity of this passage seems to have been characteristic of all his writings and work.

Perhaps the most remarkable feature in the history of the *Journal* has been its independence of outside subsidies; it has always been self-supporting, the financial responsibility resting with the editor-in-chief. Nevertheless, the monetary difficulties during the present war are said to have been greater than ever before, and a plea is made for the establishment of a fund wherewith to carry on when the present editor and his family have to relinquish their responsibility for the maintenance of the work.

The contents of the book itself may most briefly be indicated by a quotation from the prefatory note. The several lectures which make up the whole "are devoted to the principal branches of science which have been prominent in the pages of the *Journal*. They have been written with a view to showing in each case the position of science in 1818 and the general progress made during the century; special prominence is given to American science, and particularly to the contributions to it to be found in the *Journal*."

Rather more than half the book is concerned with the growth of Geology in its several aspects, successive chapters being written by Charles Schuchert, Hebert E. Gregory, Joseph Barrell, George Otis Smith, Richard S. Lull, Louis V. Pirsson, and William E. Ford. Dr. Sosman contributes a short article on the work of the Geophysical Institute at Washington; Messrs. Horace L. Wells and Harry W. Foote write on Chemistry; Leigh Page on Physics; while Wesley R. Coe and George L. Goodale deal respectively with Zoology and Botany. It remains to add that the collection of lectures forms a most valuable account of the enormous advance that has taken place in our scientific knowledge during the last hundred years—a growth unparalleled in the past, and unlikely, even in the most optimistic view, to be approached in the future.

D. O. W.

The Boy's Own Book of Great Inventions. By FLOYD L. DARROW. [Pp. xii + 385, with 104 figures in the text and 33 plates.] (New York: The Macmillan Co., 1918. Price 12*s.* 6*d.* net.)

IT has become a commonplace in this country that the general science course must be brought into closer touch with everyday phenomena; but, as yet, it is only in the United States that the books so urgently needed for this purpose are being written. *The Boy's Own Book of Great Inventions* is an excellent example of the type of book required. The author is a science teacher in Brooklyn, N.Y., and as a consequence the volume is pleasantly free from the technical blunders which so often mar books of the kind. Moreover, although it is written in a style which will be readily understood and enjoyed by boys learning science at school, it nevertheless contains a considerable proportion of strictly scientific matter: it would not be welcomed as a present by a boy altogether ignorant of Physics! The author's method is admirable. He first gives an account of the history of the subject he is discussing; next, he deals with its principles or theory; and finally, wherever possible, he describes how the reader may himself construct a working model.

The inventions dealt with are chiefly those which mankind owes to the physicist and engineer, an exception being some fifty pages devoted to chemistry, more especially to the metallurgy of iron and steel. The other chapter headings may be summarised as follows: the Gyroscope, the Telegraph and Telephone with and without wires, the Aeroplane and Submarine, Steam, Gas and Petrol Engines, Agricultural Machinery, applications of electricity, especially in relation to Transport, Light, and Heat, and finally the Telescope. Indeed the only notable omissions from the list are the gramophone and X-rays—the inclusion of which would have been more in accord with the general atmosphere of the book than the chapter on the telescope. The diagrams are clear and the photographs of the interesting type with which the modern American textbook is making us familiar. We can recommend Mr. Darrow's book very cordially, particularly for the school library.

D. O. W.

Six Weeks in Russia in 1919. By ARTHUR RANSOME. [Pp. viii + 152.] (London: George Allen & Unwin. Price 2*s.* 6*d.* net.)

IT is something to read a book written by a man who has spent even only six weeks in the heart of Bolshevist Russia, who has spoken—nay, has even dined—with the leaders of the Terror, and who has seen something during his short stay of the present condition of Russia, at any rate in Petrograd and Moscow.

Mr. Ransome seems to have made his journey in order to write his history of the Revolution, and he appears to have had full permission from the Bolshevist authorities to collect material.

According to the writer, food is very scarce in Moscow, and can only be purchased at a ruinous price; but every other commodity, including clothes, is in great demand. When a man requires a new suit, he must first satisfy his House Committee that he needs one.

The speeches of the various members of the All-Russia Soviet on the Prinkipo Proposal are, as described by Mr. Ransome, particularly interesting. The Soviet appears to be composed of every kind of individual—from the wild fanatic to the self-seeking adventurer. From a conversation that the writer had with the

Chinese Workers' delegate, it appears that there are only about two or three thousand Chinamen in the Red Army. He was also fortunate enough to be able to talk with a "bourgeois."

It seems strange to think that so many theatres are open, and, what is more, filled, in Moscow. But at the time of the September Massacres in Paris was it not the same?

The Right Social Revolutionaries seem the most hopeful of the more extreme opposition parties. The Left Social Revolutionaries are hopeless, as are the Menshevists. Mr. Ransome does not mention the Cadets.

C. C. R.

Papers for the Present. Third Series. No. 9, **The Drift to Revolution.** [Pp. iv + 52, with 7 figures.] (London: Headley Bros., Ltd., 1919. Price 1*s*. net.)

"THE Drift to Revolution" is one of a series of papers on contemporary social evolution published by the Sociological Society, and the object of this particular paper appears to be to present a study of present-day problems, and suggestions for many necessary reforms. The first section of the paper presents a decidedly unflattering picture of the modern utilitarian as personified by Messrs. Herbert Spencer and Jeremy Bentham. This class of person seems decidedly unpopular just now; but, all the same, we must give them their due. They went too far, of course, but that is no reason why we should rush the other way and hold up all their ideas to ridicule.

The writer seems to assume that the doctrine of *laissez faire* was ushered in by the Industrial Revolution and the "Wealth of Nations." Hardly so. Both Bolingbroke and Walpole had been stealing along the paths of Free Trade, and it is not improbable that Adam Smith's ideas would have been enunciated even without the stimulus of the industrial revolution. Neither did the creed of "Liberalism" begin then. There has always been a party of reform in Church and State, and is it likely that the motives of Lord Grey in 1832 were any more disinterested than those of Thomas of Gloucester in 1813? The bulk of the manufacturers and aristocratic Whigs supported the first Reform Bill for their own selfish ends, not because they hoped that the millennium would follow, as their poor ignorant dupes did. But it is true enough to say that all classes took part in the Reform agitation, and that the joint efforts of the Liberals (or Whigs) and the Radicals secured Free Trade. On p. 9 occurs a slight mistake: "From these newly-enriched classes" (of the Industrial Revolution) "had arisen political families like the Pitts. . . ." The Pitts were never particularly rich. The younger Pitt had gained much of his popularity from the fact that, although poor, he had refused the remunerative sinecure of the Clerkship of the Pells.

The next section of the paper deals with the relations between Capital and Labour, and most of it is admirable. There is an excellent description of "Production," "Distribution," "Consumption," as Karl Marx saw it.

There follows a section on the growth of Imperialism, and, incidentally, of Bureaucracy. There is a somewhat disparaging account of the latter. With the growth of Empire offices are bound to multiply. If we conquer India, we must expect to find an India Office on the heels of the conquest; but there is no doubt, as the writer says, that the average bureaucrat is not particularly observant. Why should he be, indeed? The writer correctly points out the close relationship between Junkerdom and Social Democracy.

He goes on to speak of the duel between the financier and the anarchist. The writer recognises the need for some sort of compromise, but Bolshevism, which he scarcely mentions, is neither Anarchism, nor Syndicalism, nor Guild Socialism.

The last section presents a series of clever diagrams, showing the parallel, yet opposing, forces in the State. But it is hardly right to call the Radicals, Socialists, and Anarchists, the Party of Progress, at any rate of cautious Progress. Call them rather the Party of Disorder. There is one in every State: there always has been, and there always will be. The writer concludes with an appeal to the nations of Europe to combine to form a real Society of Nations, containing the virtues of every people. But would it not be as well to see what we can do with our own country first?

C. C. R.

An Enquiry Concerning the Principles of Natural Knowledge. By A. N. WHITEHEAD, Sc.D., F.R.S. [Pp. xii + 200, with 19 illustrations.] (Cambridge University Press, 1919. Price 12*s.* 6*d.* net.)

THE recent striking confirmation of Einstein's theory by the observations carried out during the total solar eclipse of May of this year has been heralded as a revolution in science, and as the deposition of the traditional view of physical nature. To the practical scientist Einstein's theory will mean at present very little reconstruction. The main facts and resultant generalisations of physical science remain valid, except for certain corrective terms, generally very minute. Revolution will result rather in the philosophic basis of our view of physical nature, in that the principle of relativity, readily accepted by all thinking people as regards space, will be extended to include time; and this does not in fact take us much further than the "restricted theory" enunciated by Einstein in 1905, and so brilliantly interpreted by Minkowski.

Prof. Whitehead's aim is to raise the problems involved in such a reconstruction, and the question that he asks himself is: "What are the ultimate data of science?" He divides his enquiry into four main branches. He first disposes of the traditional conception of the durationless instant of time, and the consequent implication of an absolute time: what we are aware of in external nature is a duration. The author then identifies the data of science as events and objects. The structure of events provides the framework of the external nature within which objects are located. What we perceive are spatio-temporal relations of material—time, space, and material being in fact the relations between events. Thus time and space are abstractions expressive of certain qualities of the structure of events. Then follows the discussion of events by what Prof. Whitehead calls the method of extensive abstraction, events being classed according to their K-quality, or quality of extension in spatio-temporal relationship. The analytic geometry of motion is investigated by the transformation from one consentient set to another, based on the principle of kinematic symmetry. Three types of kinematics are shown to be possible, differentiated as: (i) elliptic, in which no distinction exists between time and space, so that it is inapplicable to nature; (ii) parabolic, the ordinary Newtonian common-sense system; (iii) hyperbolic, corresponding to the Larmor-Lorentz-Einstein transformation, for which the electromagnetic equations are invariant. The enquiry concludes with the theory of objects. An interesting outcome of Prof. Whitehead's train of ideas is the

distinction between what are ordinarily called dead and living objects. In the dead or physical object there is a permanently rhythmic repetition of events only in their microscopic or molecular aspect; in the living object the rhythm is manifested also in the macroscopic aggregate.

It is possible that Prof Whitehead's reply to his query will not meet with immediate acceptance, and that contemporary progressive thought will give an answer deviating somewhat from his presentation. There can be no doubt, however, that the main lines of the enquiry as conducted in the present volume offer a basis for profitable discussion, and that the author takes us one "step further into the unfathomable mystery."

S. BRODETSKY.

The Philosophy of Conflict, and Other Essays in War-time. Second Series. By HAVELOCK ELLIS. (London: Constable & Co., Ltd., 1919.)

A BOOK by Mr. Havelock Ellis is certain to be interesting, and the present volume fully comes up to expectations. It consists of essays on a great variety of subjects, many connected with the war, and most of them concerned directly or indirectly with the changes of social outlook which have been necessitated by the war. Problems of sex, and the social status of women, receive much attention; and there are some purely literary essays, including one on Justina de Wynne Rosenberg, whose chief title to fame at the present day is the very ambiguous one of having been a friend of Casanova.

Mr. Havelock Ellis is a psychologist, whose forte lies in a remarkable penetration and knowledge of human character. In consequence, his essays convey an impression of depth and exhibit a philosophic spirit, which render the book both pleasant and informing to read, quite apart from questions of agreement or disagreement. Mr. Ellis advocates the principle of Eugenics, but does not meet the main argument of its opponents. If we were certain that the data of the Eugenists were complete, we should assuredly support the policy which they advocate. But are we satisfied that their data are complete? Any action which really did affect the germ-plasm of the human race would be fraught with vast consequences for the future of humanity; and such action can only be justified if based upon a knowledge that is absolute and unquestionable; a false step would be irretrievable and utterly calamitous. Now, when we observe the numerous errors committed by statesmen in more ephemeral matters of smaller importance, we shall surely be well advised to hesitate before entrusting the hereditary constitution of humanity to the control of persons who have neither knowledge of nor regard for scientific principles. A matter of such deep importance is safer left in the hands of Nature than thrown into the cauldron of party-politics, to be determined by the ignorant prejudices of popular opinion. Nor do we think that Mr. Ellis himself has sufficiently considered all aspects of the matter. He adopts without apparent question the popular opinion that war destroys those most fit to carry on the race. But is it really so? We believe that a very large percentage of those who on medical grounds were prevented from taking an active part in the war owed their disability to physical causes of a non-hereditary type—as, for instance, heart disease, rupture, and so on, due rather to the accidents of individual life than to any hereditary weakness. On the other hand, large numbers of constitutionally inferior persons were swept into the Army, where their weakness too often became manifest. Persons medically disabled from the front line, in short, were those whose disabilities were obvious, and to a very large extent did not diminish their

competency for breeding or affect their constitutional germ-plasm. Wounds inflicted in war similarly leave no hereditary taint.

Here, then, is an illustration of a deficiency in our knowledge, sufficient to preclude action until it has been made good. And if so discerning a thinker as Mr. Ellis has neglected this point, what chance is there of ensuring that popular government shall be delivered from all risk of oversight? In proportion to the immense consequences of action must be the precision and completeness of our knowledge.

The longest of the essays is one on Psycho-Analysis, in the course of which a wholly admirable criticism is given of the doctrines of Freud and Jung. It is interesting to note that Mr. Ellis was one of the first to introduce Freud's Psychology into England. But, after all, we can give no higher praise to this most suggestive book than by saying that it is fully worthy of the author's reputation.

HUGH ELLIOT.

The Book of the Lews. By W. C. MACKENZIE, F.S.A. (Scot.). [Pp. 276 + xv.] (London : Simpkin, Marshall, Hamilton, Kent & Co., Ltd.)

THOUGH neither the most picturesque nor the most fertile of the Hebrides, nevertheless Lewis, or "the Lews," as it is sometimes called, is far from being devoid of interest, and under its new ownership the island has great possibilities before it.

Although it was probably inhabited many centuries before the Christian era, the real history of the island commences with its occupation by the Norsemen. Indeed, Norse blood flows in the veins of a very considerable proportion of the inhabitants of Lewis, and up to the time of the battle of Largs the island remained, nominally at any rate, under the sway of the Kings of Norway. The character of the "hardy Norseman" engrafted on that of the native stock has produced a race not inferior in quality to any of the inhabitants of Great Britain.

The history of the old castle of Stornoway, which was destroyed during the Protectorate, and which has no connection with the present castle, is inseparably connected with the name of MacLeod. The MacLeods are, even to-day, the most considerable clan in Lewis. Their chiefs, who were probably of Norse descent, were vassals first of the Lords of the Isles, and then, after the fall of those potentates, of the Crown of Scotland. Towards the end of the sixteenth century, however, the island fell into such a state of anarchy and misrule that King James VI granted it to a certain company of "gentlemen adventurers" from Fife, in order that they might develop the "extraordinary rich resources" of the island "for the publick good and the King's profit," and incidentally that they might "ruit out the barbarous inhabitantis." But the efforts of this company to take possession of Lewis were met with such strong opposition from the "barbarous inhabitantis" that the adventurers were at length compelled to abandon their claims. As a sequel to the struggle between the islanders and the adventurers, the island passed out of the hands of the MacLeods into those of the Mackenzies, Earls of Seaforth.

The attraction which Lewis had for "foreigners" was owing largely to its excellent fisheries. Hence we find continual exploitation of the waters of the Minch by various English companies and, of course, continual quarrels between the Scotch and English fishermen. However, the fisheries of Lewis appear now to be in a more prosperous condition.

If the Earls of Seaforth, proprietors of Lewis for two centuries and a half, had

not dabbled in politics, the island would have enjoyed greater peace during the seventeenth and eighteenth centuries than it actually did. But as the Seaforths were, except during the "Forty-Five," ardent Royalists and Jacobites, Lewis received the attentions of both Cromwell and the Hanoverians; the former caused several forts to be built there. Mr. Mackenzie provides an interesting chapter on the connection between Lewis and the Jacobites.

The three chapters dealing with the prehistoric remains in Lewis will be of great interest to archæologists. The chief of these is a large group of "Druidical" stones at Callernish. These huge megaliths are a monument of the Stone Age, and the author gives an interesting account of their shape and form of arrangement, as well as a list and a criticism of the various conjectures which have been made on the subject of their meaning and use.

Next in antiquarian interest come the "brochs," or forts, of which Dun Carloway is the chief. The author favours the idea that these brochs were built by the pre-Celtic inhabitants of Lewis, and denies that the builders were Celts or Norsemen. He believes that these pre-Celtic builders were the Firbolgs or "Iberians," a race whose type may still be seen in Lewis, as well as in other parts of the British Isles.

The last, but by no means the least interesting, of the prehistoric relics of Lewis is the "Isle of Pigmies," near the Butt of Lewis. On this isle there is a small "kirk." Inside the "kirk" the author found several bones, not, indeed, those of pigmies, but of mammals and birds. Yet he is far from believing that the pigmy tradition is entirely founded on mythology.

BOOKS RECEIVED

(Publishers are requested to notify prices)

An Enquiry Concerning the Principles of Natural Knowledge. By A. N. Whitehead, Sc.D., F.R.S., Fellow of Trinity College, Cambridge, and Professor of Applied Mathematics in the Imperial College of Science and Technology. Cambridge: at the University Press, 1919. (Pp. xii + 200.) Price 12*s.* 6*d.* net.

Vital Statistics: An Introduction to the Science of Demography. By George Chandler Whipple, Professor of Sanitary Engineering in Harvard University, Member of the Public Health Council, Massachusetts, State Department of Health. New York: John Wiley & Sons. London: Chapman & Hall, 1919. (Pp. xii + 517.) Price 18*s.* 6*d.* net.

Infinitesimal Calculus. An Elementary Course. By Horace Lamb, M.A., LL.D. Sc.D., F.R.S., Professor of Mathematics in the Victoria University of Manchester. Third and Revised Edition. Cambridge: at the University Press, 1919. (Pp. xiv + 530.) Price 20*s.* net.

Elements of Vector Algebra. By L. Silberstein, Ph.D., Lecturer in Natural Philosophy at the University of Rome. London: Longmans, Green & Co., 1919. (Pp. ii + 42.) Price 5*s.* net.

Problems of Cosmogony and Stellar Dynamics. By J. H. Jeans, M.A., F.R.S. Being an Essay to which the Adams Prize of the University of Cambridge for the Year 1917 was adjudged. Cambridge: at the University Press, 1919. (Pp. viii + 293.) With 5 Plates. Price 21*s.* net.

The Study of the Weather. By E. H. Chapman, M.A., B.Sc., Fellow of the Royal Meteorological Society. Cambridge: at the University Press, 1919. (Pp. xii + 131.) Price 3*s.* 6*d.* net.

Herschell. By the Rev. Hector Macpherson, M.A., F.R.A.S., F.R.S.E. Men of Science Series, Edited by S. Chapman, M.A., D.Sc., F.R.S. London: Society for Promoting Christian Knowledge. New York: The Macmillan Company, 1919. (Pp. 78.) Price 2*s.* net cloth; 1*s.* paper.

Lectures on Ten British Physicists of the Nineteenth Century. By Alexander Macfarlane. New York: John Wiley & Sons. London: Chapman & Hall, 1919. (Pp. 144.) Price 7*s.* 6*d.* net.

The Simple Carbohydrates and the Glucosides. By E. Frankland Armstrong, D.Sc., Ph.D., F.I.C., Fellow of the City and Guilds of London Institute. Third Edition. London: Longmans, Green & Co., 1919. Price 12*s.* net.

The Chemistry of Colloids. Part I: Kolloidchemie. By Richard Zsigmondy, Professor at the University of Göttingen, Director of the Institute of Inorganic Chemistry. Translated by Ellwood B. Spear, Associate Professor of Inorganic Chemistry, Massachusetts Institute of Technology. Part II.: Industrial Colloidal Chemistry. By Ellwood B. Spear, B.A., Ph.D. A Chapter on Colloidal Chemistry and Sanitation. By John Foote Norton, S.B., Ph.D., Assistant Professor of Chemistry of Sanitation, Massachusetts Institute of Technology. New York: John Wiley & Sons; London: Chapman & Hall, 1917. (Pp. vii + 280.) Price 13*s.* 6*d.* net.

Catalysis in Theory and Practice. By Eric Rideal, M.B.E, M.A., Ph.D., and Hugh S. Taylor, D.Sc., Assistant Professor of Physical Chemistry, Princeton University, U.S.A. London: Macmillan & Co., St. Martin's Street, 1919. (Pp. xv + 496.) Price 17*s*. net.

The Manufacture of Chemicals by Electrolysis. By Arthur J. Hale, B.Sc., F.I.C., Chief Assistant in the Chemical Department, City and Guild Technical College, Finsbury. London: Constable & Co., Orange Court, W.C., 1919. (Pp. xi + 80.) Price 6*s*. net.

Stereochemistry. By Alfred W. Stewart, D.Sc., Professor of Chemistry in the Queen's University of Belfast. London: Longmans, Green & Co., 30, Paternoster Row, 1919. (Pp. xvi + 277.) With 58 Illustrations. Price 12*s*. 6*d*. net.

The Profession of Chemistry. By Richard B. Pilcher, Registrar and Secretary of the Institute of Chemistry of Great Britain and Ireland. London: Constable & Co., 1919. (Pp. xiv + 199.) Price 6*s*. 6*d*. net.

A Course of Practical Chemistry for Agricultural Students. Vol. II. Part I. By H. A. D. Neville, M.A., F.I.C., and L. F. Newman, M.A., F.I.C., School of Agriculture, Cambridge University. Cambridge: at the University Press, 1919. (Pp. 119.) Price 5*s*. net.

Chemistry from the Industrial Standpoint. By P. C. L. Thorne, B.A. London: Hodder & Stoughton, 1919. (Pp. xvi + 244.) Price 4*s*. 6*d*. net.

Geology of India for Students. By D. N. Wadia, M.A., B.Sc., Prince of Wales College, Jammu, Kashmir, India. London: Macmillan & Co., St. Martin's Street, 1919. (Pp. xx + 298.) With 20 Plates and 37 Figures in the Text. Price 18*s*. net.

Petrology for Students. An Introduction to the Study of Rocks under the Microscope. By Alfred Harker, M.A., LL.D., F.R.S., F.G.S., Fellow of St. John's College, and Reader in Petrology in the University of Cambridge. Fifth Edition, Revised. Cambridge: at the University Press, 1919. (Pp. v + 300.) Price 8*s*. 6*d*. net.

Handbook of Mineralogy, Blowpipe Analysis and Geometrical Crystallography. By G. Montague Butler, E.M., Professor of Mineralogy and Petrology; Dean, College of Mines and Engineering; Director, Arizona Bureau of Mines, University of Arizona, Tucson, Arizona. New York: John Wiley & Sons. London: Chapman & Hall, 1918. (Pp. ix + 155.) Price 10*s*. 6*d*. net.

Graines et Plantules des Arbres et Arbustes Indigènes et Communément Cultivés en France. Par R. Hickel. Première Partie, Conifères (pp. 179, avec 93 figures originales, dessins de l'auteur), 1911. Deuxième Partie, Angiospermes (pp. 347, avec 85 figures originales et deux planches hors texte, dessins de l'auteur). 1914. Versailles: chez l'Auteur, 11 bis, Rue Champ-la-Garde.

Joseph Dalton Hooker, O.M., G.C.S.I., C.B., F.R.S., M.D., etc. By Prof. F. O. Bower, Sc.D., F.R.S., Regius Professor of Botany in the University of Glasgow. Men of Science Series, Edited by S. Chapman, M.A., D.Sc., F.R.S. London: Society for Promoting Christian Knowledge. New York: The Macmillan Company. (Pp. 62.) Price 2*s*. net cloth, 1*s*. paper.

Applied Botany. By G. S. M. Ellis, B.A. London: Hodder & Stoughton, 1919. (Pp. viii + 248.) Price 4*s*. 6*d*. net.

The Unity of the Organism or Organismal Conception of Life. By William Emerson Ritter, Director of the Scripps Institution for Biological Research of the University of California, La Jolla, California. Two Volumes. Boston, U.S.A.: Richard G. Badger, The Gorgham Press. (Vol. I., pp. xxix + 398; Vol. II., pp. xv + 408.) Price $5 net each.

Mendelism. By Reginald Crundall Punnett, F.R.S. London: Macmillan & Co., St. Martin's Street, 1919. (Pp. xiv + 215.) Fifth Edition. Price 7*s*. 6*d*. net.

Textbook on Wireless Telegraphy. By Rupert Stanley, B.A., M.I.E.E., Professor of Physics and Electrical Engineering, Municipal Technical Institute, Belfast. In Two Volumes. London: Longmans. Green & Co., 39, Paternoster Row, 1919. (Vol. I., pp. xiii + 471; Vol. II., pp. ix + 357, with illustrations.) Price 10*s*. net per volume.

Engineering Education. Essays for English, Selected and Edited by Ray Palmer Baker, M.A., Ph.D., Professor of English in the Rensselaer Polytechnic Institute. New York: John Wiley & Sons; London: Chapman & Hall: 1919. (Pp. ix + 185.)

Mensuration for Marine and Mechanical Engineers (Second and First Class Board of Trade Examinations). By John W. Angles, M.Sc., Head of Engineering Department, West Hartlepool Technical College. London: Longmans, Green & Co., 39, Paternoster Row. (Pp. xxvii + 162, with diagrams.) Price 5*s*. net.

The Examination of Milk for Public Health Purposes. By Joseph Race, F.I.C., City Bacteriologist and Food Examiner, Ottawa; Chairman of Committee on Standard Methods of Analysis, Canadian Public Health Association, Member of Committee on Municipal Food Administration, American Public Health Association. New York: John Wiley & Sons; London: Chapman & Hall, 1918. (Pp. vi + 224.) Price 8*s*. 6*d*. net.

Catalogue of Lewis' Medical and Scientific Circulating Library, including a Classified Index of Subjects, with the names of those Authors who have treated upon them. Second Edition, Revised to the end of 1917. London: H. K. Lewis & Co., Ltd., 1918. (Pp. vi + 492.) Price 12*s*. 6*d*. net; to subscribers, 6*s*. net.

Messrs. H. K. Lewis & Co. have issued a second edition of their catalogue of their Medical and Scientific Circulating Library. It comprises a list of over 10,000 volumes, of which a very large proportion are of interest to medical men. These are classified under the names of their authors, but, in addition, a classified index of subjects is given in the latter part of the book with cross-references to the authors. The terms of subscription to the library are set out in full, and the catalogue will be of great service, both to subscribers and non-subscribers.

T. B. J.

Mind, and Its Disorders. A Textbook for Students and Practitioners of Medicine. By W. H. B. Stoddart, M.D., F.R.C.P., Lecturer on Mental Diseases to St. Thomas's Hospital Medical School. Third Edition, with Illustrations. London: H. K. Lewis & Co. (Pp. xx + 580.) Price 18*s*. net.

Essentials of Chemical Physiology. For the Use of Students. By W. D. Halliburton, M.D., LL.D., F.R.S., Fellow of the Royal College of Physicians, Professor of Physiology in King's College, London. Tenth Edition. London: Longmans, Green & Co., 39, Paternoster Row, 1919. (Pp. xi + 324, with 72 Illustrations.) Price 7*s*. 6*d*. net.

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Child Welfare and the Teachings of Certain Dentists, School Medical Officers, Medical Officers of Health, and other Medical Men. By J. Sim Wallace, D.Sc., M.D., L.D.S. London: Baillière, Tindall & Cox, 8, Henrietta St., Covent Garden, W.C., 1919, (Pp. x + 102.) Price 5*s*. net.

The Feeding of Nations: the Oliver-Sharpey Lectures. A Study in Applied Physiology. By Ernest H. Starling, C.M.G., M.D., F.R.C.P., F.R.S., Chairman of the Royal Society (War) Food Committee, Honorary Scientific Adviser to the Ministry of Food. Given at the Royal College of Physicians, London, June 3–5, 1919. London: Longmans, Green & Co., 1919. (Pp. x + 146.) Price 5*s*. net.

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The Philosophy of Conflict, and other Essays in War-Time. Second Series. By Havelock Ellis. London: Constable & Co., 1919. (Pp. vi + 299.) Price 6*s*. 6*d*. net.

A Short History of India in the Middle Ages, 1000-1764. By Stanley Lane-Poole, M.A., Litt.D., sometime Professor of Arabic in the University of Dublin. Bombay: K. & J. Cooper, Educational Publishers. (Pp. xvi + 153.)

New Readings from Indian History. British Period. Selected and arranged by W. H. Hutton, B.D., Reader in Indian History in the University of Oxford and Fellow of St. John's College; Archdeacon of Northampton; Honorary D.C.L., Durham. Bombay: K. & J. Cooper, Educational Publishers. (Pp. vii + 218.)

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RECENT ADVANCES IN SCIENCE

PHILOSOPHY. By Hugh Elliot.

We have on various occasions pointed out that Philosophy is a general name covering a great variety of subjects, concerning which our knowledge is in a very primitive condition. As soon as we begin to acquire a certain amount of definite knowledge about any of these subjects, it usually ceases to be called Philosophy, and is incorporated into one of the special sciences, or may itself become the subject of a new special science. During the last few months, a striking illustration of this transition has been furnished by the Principle of Relativity. Theories of space and time have always been regarded as favourite subjects of controversy among metaphysicians. By Science, on the other hand, space and time have been accepted as postulates, and the question of their absolute validity roused little interest, so long as they afforded a framework sufficiently solid to support the edifice of scientific hypotheses. But as that edifice continued to grow, it became here and there too large for its old framework. Adjustments had to be made; and it has been found very much simpler to modify the framework than to modify the building itself. And hence for the first time the attention of men of science has been directed towards scrutinising those fundamental postulates, which had previously been accepted just because they were found to work. But when they were no longer found to work, they had to be altered; and the character of the alteration to be made was determined by the usual scientific methods of observation and experiment—by predictions which would either verify or falsify the new hypothesis. And thus at length, space and time have been incorporated into the science of Physics, and removed from the quagmire of philosophy, where they had been left behind when Physics itself was emancipated by Newton more than two hundred years ago. Such titles as "Natural Philosophy," *The Philosophical Transactions*, still indicate the parentage of the science of Physics. The emancipation, however, was not complete; Newton still left some work for Einstein to perform.

It is true that certain parts of the doctrine of Relativity had been anticipated in the discussions of philosophers. It

has lately been pointed out that Descartes three hundred years ago did sketch out a theory having some sort of crude resemblance to the theory of Einstein. The contention is true, and it is far more than mere chance. Descartes possessed an intellectual power which does not arise more than once in several generations; his penetration into natural phenomena may almost be described as superhuman. But the contention nevertheless does not bear out the conclusions for which it was brought—namely, to justify the methods of metaphysics. For Descartes' mind embraced, not merely all the metaphysics of his time, but all the science of his time as well, in so far as Science was differentiated from Philosophy. He was a distinguished mathematician; he was also a distinguished physiologist, and very clearly set forth the doctrine of physiological automatism which only came into general recognition in the nineteenth century. So far from the prevision of Descartes furnishing an argument for the validity of metaphysical methods, it presents an additional proof of their invalidity. For three hundred years, metaphysicians have had before them Descartes' adumbration of a true theory; for three hundred years they have operated upon it by metaphysical methods; and in all that time have not succeeded in advancing it one whit. Even with a rough approximation to truth before them, they can make nothing of it. But as soon as the man of science comes in with experimental methods, truth begins to appear, even to those who had not the previous advantage of studying the first approximation. Ten years of physics have given results which could not have been reached at the previous rate of progress in a thousand years of metaphysics.

A philosophic reviewer is not called upon for an opinion as to whether Einstein's theory is true, or whether parts of it are true, or adumbrations of the truth. He has only to note its philosophical bearings. Principal among these is that it does remove theories of space and time from the ambiguous and temporary position which they occupied as a branch of metaphysics. They are brought definitely into science, and linked up with the rest of our knowledge about the universe. And the same is true of Gravitation, which has hitherto occupied an anomalous and isolated position. All these are brought by Einstein into the sphere of science.

It has, however, been objected to on the ground that it is itself metaphysical in tendency; and that by certain metaphysicians it has been received with obvious acclamation. But too much importance need not be attached to this latter point. Metaphysicians generally tend to favour any theory in proportion to its obscurity; and the principle of Relativity presents

to them just that kind of *primâ facie* absurdity which would naturally commend it to their belief.

The apparent absurdity, however, is due merely to unfamiliarity. An uncultivated person finds it hard enough to believe that the earth, apparently so still and solid, is really in rapid motion, on its own axis, round the sun, and also with the rest of the solar system in a straight line through space. If there were no outside standard of comparison, the notion of itself would be exceedingly difficult to grasp. The earth would be regarded in all matters as the general standard of rest; movement would always mean movement *relative to the earth*, and in the absence of any wider conception, movement would thus appear to have an *absolute* value. So it now is with regard to space and time. The only standard of space and time hitherto known to us has been that prevalent on the earth; and accordingly they have appeared to be absolute and universal in nature. But at length an outside standard of comparison has been discovered; and it is seen that our ideas of space and time, formed by experience of the space and time upon the earth, are no more absolute than are the ideas of motion which we form by regarding the earth as motionless. We move yet a step further from the anthropocentricity of primitive thought, where everything is judged by the standards of our own immediate outlook. We, after all, are not the centre of the universe; our earth is not the standard of absolute rest; nor are our own space and time absolute properties of the entire universe.

The Principle of Relativity seems, therefore, to be in no way opposed to the spirit of science as hitherto conceived. On the contrary, it brings powerful reinforcement to the doctrine of the Uniformity of Law, and the invariability of the laws of nature. As stated in its most generalised form, the Principle of Relativity reads thus: "Given different groups of observers, some of whom are in uniform motion of translation with reference to others, the laws of nature are precisely identical for these different groups of observers." By pre-relativity standards, the laws of nature must have appeared different from these different aspects; a circumstance which might have afforded some points of vantage to critics of scientific materialism, but which appears to have escaped their attention.

A few words must be said of the death of Prof. Ernst Haeckel, which took place on August 8, 1919. He passed, as many others have done, from medicine to zoology, from zoology to philosophy, and from philosophy to discussion of religious problems; and it is now largely with regard to the latter spheres that he is remembered. The chief influence on his life was probably his reading of the *Origin of Species* in 1860. He was an immediate convert, and set about to popularise the

theory of evolution in Germany, where the rapid progress of true ideas was in no small measure due to his activity. In philosophy he is chiefly identified with the kind of materialism prevailing last century— and with a monism which, if it appears crude and ill-thought-out in these times, was nevertheless a great advance on what had gone before. His present unpopularity in England is not altogether well justified. There were faults in his view of the universe, no doubt; but a true theory cannot be achieved in a day; and it is fairer to compare his philosophy, not with what we know now, but with the philosophy of his contemporaries, whom he attacked. But they are past and forgotten altogether. Their views, if revived, would excite only a smile; the victory was to Haeckel and not to them; and it is strong testimony to the strength of his philosophy that even now it is sufficiently alive to excite serious hostility; and that, too, though the public in general would much sooner have forgotten him than his antagonists, if they could. Science moves step by step; and it is scarcely gracious to attack a man who moved a few steps upwards, merely because he did not climb the whole staircase. He accelerated the rate of progress; both in zoology and philosophy he did work from which others could take their start and continue moving forwards.

Haeckel has also been criticised on account of his very embittered attitude towards England during the war. We are bound to confess that it was unworthy and even contemptible. But let us not forget that he was over eighty when the war broke out, and surrounded on every side by violent prejudice and emotion. It is not to be expected that the mind of a man at that age should retain sufficient strength to stand out against the popular storm, and see things clearly in the wild emotional hurricane of war. It is not only more charitable, it is far more true, to see in this perversion of judgment an inevitable symptom of senility; the senility of an old man who had worked out his life in the arduous pursuit of science, and defiance of popularity. Had he been a younger man, the case would have been different; but so also, if we may judge by the whole tenor of his life and opinions, would have been his views. Haeckel's permanent reputation is assured as a leader of nineteenth-century thought.

PURE MATHEMATICS. By Dorothy M. Wrinch, University College, London.

The *Proceedings of the London Mathematical Society*, series 2, vol. 18, part 3, issued in September 1919, is an exceptionally interesting number. It contains two long papers, dealing

with problems of a very fundamental character, of which a somewhat extended notice appears to be called for. The first is by W. H. Young, on *Series of Bessel Functions*. In modern developments of mathematical physics, no series are used more extensively, and perhaps no usual series of expansions in terms of prescribed functions has been so little treated from the point of view of the pure mathematician. Prof. Young's paper raises such series from the level which they occupied previously—a level at which the investigator, in the absence of definite knowledge of the necessary and sufficient conditions for their validity, has in the main been guided only by the intuition of the physicist—to a new level which is comparable with that of Fourier series. Prof. Young, in fact, has enabled us to transfer bodily a great part of the theory of Fourier series directly to expansions in terms of Bessel functions. Only Dini has hitherto overcome many of the serious analytical difficulties attending the development of this type,—in a sense in which Fourier, Hankel, and others have not contributed. We refer to the difficulties involved in the discussion of the region of convergence of such series. Other contributors who have dealt in a logical manner with special types of such series are Kneser and Hobson.

Prof. Young had already given a new method, based on a theorem in regard to Restricted Fourier series, of discussing the expansion of a function $f(x)$ in a series of prescribed functions. He developed it more particularly in regard to expansions in Legendre polynomials—which were especially suitable in that their asymptotic values, for large values of the order, are of a trigonometric type, while at the same time, being polynomials, their treatment did not require the use of the complex variable. It was clear that the power of the method was not in any real sense due to these simplifications and that it was capable of equally effective use in other domains. The author has selected the Bessel functions for attack in this paper—not, as would have been a compelling reason, because of their importance in other branches of thought, but because these simplifications are no longer present.

Some of his conclusions are of very wide significance, and, taken together, form almost a complete solution of all the problems of convergence of any type relating to such series at *internal* points of the interval $(0, 1)$. For example, under the single condition that the typical term of a Bessel series tends to zero, the series may be shown to behave at any such internal point precisely like a Fourier series.

Very important information is contained in the paper relating to another fundamental problem in the applications of such series. It has usually been assumed, in such work, that if

any individual term satisfies a certain differential equation, the function represented by the sum of an infinite number of such terms will also satisfy it. Prof. Young proves that such a conclusion is valid if the function $f(x)$ has, in the interval in which such an equation is to be satisfied, a differential coefficient of suitable order, such that the Fourier series of a function equal to this differential coefficient in this interval, and having any convenient values elsewhere, converges in the interval in question. This occurs in particular if the differential coefficient has bounded variation.

Considerable limitations still hold, however, as the author points out, in directions, and more especially in other regions where the nature of the convergence must be elucidated—which are to form the subject of further investigation, and on which it is not at first sight clear that the same method is readily applicable. But this paper marks a very notable advance in the pure theory and the logical basis of many of the expansions which are used regularly by the applied mathematician.

The second paper to which we have referred above and which we have already mentioned in Recent Advances (for January 1920) is by G. H. Hardy and J. E. Littlewood, on Abel's theorem and its converse. This paper is intimately related to many others by the same authors. The main object of the authors is to obtain as far-reaching a generalisation as possible of Tauber's theorem—the converse of Abel's well-known theorem relating to power-series, which, with Stolz's generalisation, is described, for instance, in Bromwich's treatise on *Infinite Series*. Let the power series be $\Sigma\, a_n x^n$ or S, and let its sum be $f(x)$. Let $\Sigma\, a_n$ be called A, the point $x=1$ on the circle of convergence being in question. In describing the conclusions of the authors, certain of their abbreviations are necessary. Thus (K) may stand for the proposition that A is convergent, (L) for the proposition that $f(x)$ tends to A, and (O) and (o) for the properties of the coefficients usually written as

$$a_n = O\left(\frac{1}{n}\right), \qquad a_n = o\left(\frac{1}{n}\right)$$

Then Abel's theorem states that (K) implies (L) along a path C which is a radius $(o, 1)$ of the circle of convergence, whereas Stolz's theorem is the same for a so-called Stolz path. The authors have already proved several associated results, for example that the theorem is not true for any regular path. Tauber's theorem states that (L) and (o) imply (K) when the path (C) is the radius $(o,1)$, and this theorem has been extended to any Stolz path, whereas it is also known that (L)

and (O) imply (K) when the path is the radius, and that (O) and (o) in these statements can in fact be replaced by other less restricted conditions of a different form. These are perhaps the more interesting results in this difficult subject which have been so far completely elucidated, but they are not of course exhaustive.

The authors begin the new work by an extension of one of the theorems above—that (L) and (O) imply (K) when C is the radius $(0,1)$. They show in the first instance that this radius can be replaced by a regular Stolz path. The main problem undertaken, after this preliminary, concerns the condition, which may be called $(\varDelta)$, with which it is necessary to replace (L) in order to obtain satisfactory results for *regular* paths—that is to say, paths with a continuously turning tangent at every point except $x=1$, and approaching $x=1$ with a definite direction. This condition $(\varDelta)$ has been elucidated in earlier work, and it was already known that $(\varDelta)$ and (O) imply (K) whenever C is a regular path in this sense. The main problem may be described as the removal of this restriction to regular paths, and the simultaneous replacement of (o) by (O). This problem is solved, but the restrictions on the path cannot be removed entirely when the condition (L) is in question, though some important corollaries emerge which it is not necessary to mention in detail here. The advance made is very fundamental and should be a means of stimulating the interest of other mathematicians, already growing, in problems of this type.

A side-issue of some considerable interest emerges in connection with the simultaneous convergence of a Fourier series and its *conjugate* or *allied* series, a matter which is of importance in the applications of mathematics, and on which insufficient attention is usually bestowed. Such conjugate series are defined by the typical terms $\alpha_n \cos n\theta + \beta_n \sin n\theta$ and $\alpha_n \sin n\theta - \beta_n \cos n\theta$, derived from the complex $(\alpha_n + i\beta_n)$ $\exp. in\theta$.

The same number contains also a note by G. H. Hardy on two problems in the analytic theory of numbers, and the earlier part of a paper by E. L. Ince on some continued fractions of a curious type associated with the hypergeometric equation.

R. L. Moore, *Amer. Journ. of Math.* xli. (1919), pp. 299–319, carries on an investigation of the Lee—Reemann—Helmholtz—Hilbert problem of the foundations of geometry. The paper arises from a well-known remark of Poincaré, that Hilbert's hypotheses are much more general than those of Lee, who defined groups by analytic equations. But Poincaré did

not regard the position even then as satisfactory. The present paper is based on a set of assumptions in terms of the notions of point, region, and motion. The space which undergoes the transformations (or motions) is not subjected in advance to the conviction of being a number plane, and it is not supposed in advance that the regions are in one-one correspondence with portions of such a plane. Hilbert analysed the group of transformations but not the space undergoing them. The author's treatment subjects both to a simultaneous analysis.

M. E. Cartan, *Bulletin de la Société Mathématique de France*, xlvi. (1919), pp. 84–105, continues his researches on certain hypersurfaces in spaces of five and more dimensions.

M. D'Ocagne, *Comptes Rendus*, 170 (1920), pp. 16–171, discusses the distribution of curvature round a point of a surface, in relation to the Meusnier surface associated with the point.

F. Morley, *Amer. Journ. of Math.*, xli. (1919), pp. 279–283, discusses the Lüroth Quartic Curve, starting from Bateman's theorem that the seven points which have the some polar line as to a conic and a cubic give rise to a Lüroth quartic.

A. B. Coble, *Amer. Journ. of Math.*, xli. (1919), pp. 243–266, discusses the ten nodes of the Rational Sextic and of the Cayley Symmetroid. It is proved that the theorem of Conner, in the *Journal* for 1915, regarding the intimate relation between the nodes of the Sextic has its analogue with reference to the Symmetroid under regular Cremona transformation in space.

W. P. Milne, *Proc. Lond. Math. Soc.* (2) xvii. (1919), pp. 274–280, discusses the determinantal systems of Coapolar Triads on a cubic curve. The paper is a sequel to one already noticed a year ago.

J. Hodgkinson, *Proc. Lond. Math. Soc.* (2) xviii. (1919), pp. 268–274, discusses a problem of conformal representation. The curves dealt with form curvilinear triangles whose sides have a real common orthogonal circle, and these are represented on the half plane of a new complex variable. The present paper is designed to elucidate the conditions and limitations of validity of the previous analysis.

P. Boutroux, *Comptes Rendus*, 170 (1920), pp. 164–166, considers further a class of multiform functions associated with a differential equation of the first order. The equation is:

$$zz_1 = 3mz + 2x^3 + bx + c$$

and the functions present an infinite number of branches and singularities. The conditions of the formation of these functions are reduced to a simple form. The functions are shown to be automorphic in a new type.

G. Valvion, *Comptes Rendus*, 170 (1920), pp. 167–169,

discusses the theorem of Picard and the generalisations which Borel has made. The theorem relates to the values possible to a function in the neighbourhood of an essential singularity of an isolated kind, and all previous demonstrations have been indirect. The author gives a direct proof when the singular point is of finite order. This is capable of extension to an infinite order by the method outlined by the author in previous notes, and a short exposition of the process is given.

E. Cotton, *Bulletin de la Société Mathematique de France*, xliv. (1919), pp. 69–84, sets up certain criteria for the convergence of infinite series of the form $\Sigma \mid a_n \mid$ where $a_n = c_{n\ 1} - c_n$, (c_n) being a set of complex numbers which has (1) a finite limit not zero; (2) the limit zero.

L. Pomey, *Comptes Rendus*, 170 (1920), pp. 100–101, obtains some interesting theorems relating to Fermat's numbers. His starting point is the theorem of Lucas, of 1878, on numbers of the form $2^m - 1$. He obtains the necessary and sufficient condition that a number of this form should be a prime. Three distinct necessary and sufficient conditions are proved, of which any one alone will suffice. It is shown further that if $F = 1 + 2^n$ is a prime, every prime number of type $2^m - 1$, where $\frac{m-1}{2}$ is an odd multiple of n, is a primitive root of F. There are further theorems stated of a similar nature.

A. Cunningham, *Messenger of Mathematics*, xlix. (1) (1919), pp. 1–32, discusses the factorisation of $(x^n \mp y^n) \div (x \mp y)$ etc., when x-$y = 1$.

W. H. Young, *Proc. Lond. Math. Soc.* (2) xviii. (1919), develops the theory of non-harmonic Fourier series. Only a portion of the paper has yet appeared, and a comprehensive statement of the results is not yet possible.

Sir George Greenhill, *Phil. Mag.* (3) No. 227, 1919, directs attention to a posthumous memoir of Clifford, indicating the greater elegance of the Clifford function as against the Bessel function ordinarily used. Illustrations are given in relation to problems of diffraction, whirling of shafts, stability of towers, vibrations of an elastic sphere, and so forth. The paper contains some interesting new cases in which the problem of lateral vibrations of shafts can be solved in terms of such functions.

H. J. Priestley, *Proc. Lond. Math. Soc.* (2) xviii. (1919), pp. 266–268, publishes a note on the values of n which cause the function $\frac{d}{dx} P_n^{-m}(x)$ to vanish when $x = n$. The paper contains a proof of a theorem of Carslaw, to the effect that

the roots are all real and separate. This theorem is proved by a simple method based on the theory of integral equations, and is a remarkable instance of the utility of such equations in the solution of specific problems.

E. W. Hobson, *Proc. Lond. Math. Soc.* (2) xviii. (1919), pp. 249–266, discusses Hellinger's integrals, which occurred, in the first instance, in the theory of quadratic forms containing an infinite number of variables. Hellinger regarded the integrals as a new species of limit, but Hahn showed that they were reducible to Lebesgue integrals. The present author gives a much simpler method of carrying out this reduction, and the results are extended to a wider class of cases of which those treated by Hellinger and Hahn are only particular cases.

H. B. Philips, *Amer. Journ. of Math.*, xli. (1919), pp. 266–279, writes on functions of matrices, and studies the functions represented by polynomials or convergent series in a matrix or finite number of matrices. As a preliminary, the main facts concerning the roots of matrices are developed.

W. J. Johnston, *Proc. Royal Soc.*, 678, A Vol. 96, pp. 331–333, lays down a linear associative algebra suitable for the discussion of electromagnetic relations and the theory of relativity. It is based on four fundamental units i, j, k, o, and, with the products, itself has sixteen units. The scalar unit is commutative with the others, which can receive any interpretation consistent with the distributive and associative principles. The vectors i, j, k are ultimately taken as rectangular unit vectors in Euclidean space, and o is perpendicular to the other three. The products i, j, i, k, j, etc., can remain without interpretation. Sir Joseph Larmor, in the same journal, has applied this new algebra to a discussion from a new standpoint of the more extended principle of relativity as given by Einstein.

The following is a further selection from recent papers :—

Matsusaburo Fujiwara, *Science Reports of the Tohoko Imperial University*, viii. (1) (1919), pp. 43–51, generalises the Tauberian theorem to cover the case of double series.

Friedrich Reisz, Potenzreihen mit vorgeschriebenen Augangsliedem, *Acta Mathematica*, xlii. (2) (1919).

O. E. Glenn, on a new treatment of theorems of finiteness, *Transactions of the American Mathematical Society*, xx. (3) (1919).

E. W. Chittenden, on the theory of developments of an abstract class in relation to the calcul functionnel, *Transactions of the American Mathematical Society*, xx. (3) (1919).

Pierre Humbert, Sur deux Polynomes associés aux polynomes de Legendre, *Bulletin de la Société Mathématique de France*, xlvi. (1919).

Motoji Kunujeda, Note on asymptotic formulæ for oscillating Dirichlet's integrals, *Quarterly Journal*, xlviii. (2) (1918), pp. 113–136.

Tsuruichi Hayashi, On the analytic function whose modulus is a rational integral function of the imaginary part of its argument, *Science Reports of the Tohoko Imperial University*, viii. (1) (1919), pp. 17–31.

Matsusaburo Fujiwara, Über Irrationalität unendlicher Kettenbrüche, *Science Reports of the Tohoko Imperial University*, viii. (1) (1919), pp. 1–10.

A. Arwin, Über die Lösung der Kongruenz $(\lambda+1)^p-\lambda^p-1=0$ (*mod* p^2), *Acta Mathematica*, xlii. (2) (1919).

E. Borel, Sur la Répartition Probable et les Fluctuations des Distances Mutuelles d'un Nombre fini de Points, Droites et Plans, *Bulletin de la Société Mathématique de France*, xlvi. (1919).

G. A. Miller, Substitution groups on the terms of symmetric polynomials, *Quarterly Journal*, xlviii. (2) (1918), pp. 147–151.

APPLIED MATHEMATICS. By S. Brodetsky, M.A., Ph.D., A.F.R.Ae.S., University, Leeds.

In his opening address to the Mathematical and Physical Section of the British Association meeting at Bournemouth this summer (*Nature*, **2603**, Sept. 18, 1919, pp. 52–9) Prof. Gray has some useful remarks to offer on the teaching of dynamics. He considers dynamics to be a physical subject, which should be taught in physical departments of Universities, not in mathematical or applied-mathematical departments. His aim is to make the teaching of the subject more experimental. Every experienced teacher of mechanics will agree with this plea, in so far as it involves the introduction of properly devised experiments into every elementary course on dynamics, or indeed statics and hydrostatics too. It is surely possible to make such a practical course more interesting and more varied than the rather dreary set of experiments that the physical student is usually made to work through in "properties of matter." But the time is hardly ripe for the transference of the teaching of dynamics to the physical departments of our Universities. The teacher of dynamics is always called upon to give his pupils much that is almost purely mathematical; he is often forced to supplement their slender stock of algebraical, geometrical, and trigonometrical knowledge. The mathematical department is the appropriate place for such teaching.

Prof. Gray, lays particular stress on a correct appreciation of the dynamics of rotation, and desires the learning of dynamics

to consist largely in the handling of rotating bodies, so that the student might observe and test the laws of precession and nutation, and acquire an instinctive appreciation of the results of rotational theory. There can be no doubt that Prof. Gray's complaint of the appalling lack of knowledge of the principles of rotational dynamics is abundantly justified. Even bona-fide students of mechanics know far too little of this subject. The traditional treatment and sequence in applied mathematics leaves the theory of rotation at a stage that is reached by few students, and when reached the theory is presented in a form difficult of comprehension by any but the most gifted of the remnant.

The remedy suggested is to do away with the teaching which as a beginning contents itself with rectilineal motion. Prof. Gray would make the consideration of the manner of growth of vectors the main subject of dynamical pedagogy. Whilst one is inclined to agree that this would be very desirable and advantageous, it is yet doubtful whether such treatment is possible with beginners. Ordinary, untutored experience, which is what the teacher of elementary dynamics has to deal with in the early stages of the course, cannot be worked up at once into a scientific theory of rates of variation of directed quantities. The simplest and most intelligible scientific problems are undoubtedly those on rectilineal motion, and the teacher must begin with them. As he proceeds, however, he should gradually instil into his pupils a comprehension of vectors and their variations, following the valuable suggestions offered by Prof. Gray in his Bournemouth address and elsewhere.

The fact is that the beginner in mechanics is usually hampered by an insufficient equipment of geometrical experience. This necessitates the mingling with the strictly dynamical part of the subject—the doctrine of force, work, and energy—of a modicum of geometrical doctrine, called kinematics. There is a prospect that matters will improve somewhat as the result of the popular interest in aerial flight. In ordinary travelling the question of relative motion and addition of simultaneous motions is not of very immediate importance. A train follows a prescribed path, a motor-car is restricted to definite roadways, even a ship is not so very dependent on the currents in the sea, at least as far as the ordinary passenger is aware. With aerial flight matters are not so simple. An unknown current in the upper air may be the cause of the ignominious failure of an otherwise formidable air-attack by an enemy power; an uncertainty in the direction of the wind may bring disaster to a transoceanic flight, whilst the success of such an attempt is hailed as a marvellous exhibition of piloting skill. An interesting account of Air Navigation is given by H. E.

Wimperis (*Aer. Journ.* 1919, xxiii. 445–68), and teachers of dynamics should not fail to use the practical problems discussed here in order to give additional force and reality to their lessons on vectors. Two papers of similar interest have been published by L. Dunoyer (*Comptes Rendus*, 1919, **168**, 726–9, 1102–5).

If the student finds it difficult to grasp the theory of rotational dynamics, he finds it equally difficult to understand the theory of units and dimensions. The elementary treatment of mechanics does not admit of the inclusion of an adequate theory of dimensions, and the result is that many of our students cannot think in any but certain stock units. Thus, *e.g.*, accelerations are almost invariably given in ft./sec.^{2} or cm./sec.^{2} An acceleration like ten miles an hour each minute would be understood by the better class of student after some little thought—but not instinctively. The mental effort becomes positively painful when he is asked to apply the idea of units to, say, electricity and magnetism, or to the *a priori* discussion of fluid resistance. Many attempts have been made recently to render the theory of dimensions clearly and simply—with varying success. The subject is discussed by H. Levy in his lecture: "From Model to Full Scale in Aeronautics" (*Aer. Journ.* 1919, xxiii. 326–45, 352–6), in which are treated also the problems of fluid resistance and of vortex motion in a viscous fluid. The theory of dimensions and the value of model experiments in aeronautics are also dealt with by L. Bairstow in his Wilbur Wright Lecture (*Aer. Journ.* xxiii., supplement to July issue, 1919), whilst in the same journal W. S. Farren gives an account of full-scale experiments on aeroplanes (xxiii. 34–72, 1919). The theory of screw propellers based on the theory of dimensions and of dynamical similitude is discussed by Amans (*Comptes Rendus*, 1919, **168**, 822–5).

Several papers dealing with motion in a resisting medium have been published during the past few months. The approximate path of a bomb dropped by an aeroplane in horizontal flight, valid when the length of the trajectory is about 1,000 or 2,000 feet, is deduced by E. B. Wilson: "Note on Bomb Trajectories" (*Phys. Rev.* (2), 1919, xiii. 305–6), who gives a corrective term to be added to the usual parabolic equation of the path. Another approximate construction for a resisted trajectory consists in combining a parabola for the initial part and the highest portion of the path, with a hyperbola for the remainder, which, as is well known, is asymptotic to a vertical straight line. See M. Risser: "Sur les formules représentatives des trajectories" (*Comptes Rendus*, 1919, **168**, 390–2). The rigid dynamics of resisted motion is worked out for the "Two-Dimensional Motion of a Plane Lamina in a Resisting Medium," by S. Brodetsky (*Proc. Roy. Soc.*, 1919, **95**, A, 516–32), where

it is proved that the "terminal" motion is in the nature of a fluctuation about a straight line inclined to the vertical (see also SCIENCE PROGRESS, Oct. 1918, p. 185).

The motion of the balloon forms the subject of an elaborate investigation by F. Jentzsch-Gräfe: "Zur Mechanik des Freiballons" (*Phys. Zeit.* 1919, **20**, 320–8). After quoting the ordinary theory of a body moving under a constant force—the lifting power—and the air resistance, which is taken to vary as the square of the speed, the author discusses the steady motion upwards, the greatest height attainable by a given balloon, and the time taken to reach this height. The temperature effect is also taken into account. The interesting remark is added that the results were worked out in 1915 and used in the German Army.

Corresponding work on aeroplane motion is contained in a series of short papers by A. Rateau (*Comptes Rendus,* 1919, **168,** 1142–7, 1246–51, 1295–1301; **169,** 156–61). The theory of steady horizontal flight at various altitudes is followed by the determination of the greatest height at which such flight is possible with a given machine. Then comes an investigation of rectilineal climbing and the maximum vertical rate of climb, with an application of the theory in a numerical example.

Bairstow's Wilbur Wright Lecture, already quoted above, is well described by its title: "Progress of Aviation in the War Period: Some Items of Scientific and Technical Interest." In addition to the question of model and full-scale experiments, the author discusses the control of aeroplanes and the oscillations of stable and unstable aeroplanes when left to themselves. The reader should also refer to the *Technical Report of the Advisory Committee for Aeronautics for the year* 1913–14, which has been recently released for publication, and which contains valuable information on theoretical and practical aerodynamics, aeroplane control and stability, and other matters of interest in the mechanics of the airship and aeroplane.

The problem of struts in aeroplane construction has already been referred to in a former article (SCIENCE PROGRESS, July 1919, p. 15). A marked advance in the mathematical methods available for the study of the flexure of beams, the torsion of cylinders (as well as the motion of a fluid round a cylinder), is represented by the paper by L. Bairstow and A. Berry: "Two-Dimensional Solutions of Poisson's and Laplace's Equations" (*Proc. Roy. Soc.,* 1919, **95,** A, 457–75). The method of the paper depends on the well-known hydrodynamical theorem that "any irrotational acyclic motion can be reproduced by an appropriate choice of simple sources round the fixed boundaries." This is essentially a theorem in pure mathematics, which can be interpreted in terms of elasticity as well as of

hydrodynamics. The mathematical treatment by Berry is applied by Bairstow to the discussion of some particular problems. Other papers on this and allied subjects are:

WEBB, H. A., and LANG, E. D., Struts of Conical Taper, *Aer. Journ.* 1919, xxiii. 179-86, where it is proved that in the best form of such a strut the cylindrical part is one half of the whole length, and the diameter of each end is one half of the maximum diameter.

COWLEY, W. L., and LEVY, H., Vibration and Strength of Struts and Continuous Beams under End Thrusts, *Proc. Roy. Soc.* 1919, **95**, A, 440–57, the results of which are applicable to the wing spar of an aeroplane in actual flight, with a throbbing engine.

LE COCQ, G. L., Sur une propriété très générale des câbles servant aux transports aériens, *Comptes Rendus*, 1919, **168**, 761–4.

SEEGAR, M., and PEARSON, K., De Saint-Venant Solution for the Flexure of Cantilevers of Cross-Section in the form of Complete and Curtate Circular Sectors, and on the Influence of the Manner of Fixing the Built-in End of the Cantilever on its Deflection, *Proc. Roy. Soc.* 1919, **96**, A, 211–32.

MESNAGER, M., Valeurs maxima de la tension près de la face inférieure d'une plaque carrée supportant une charge unique concentrée en son centre, *Comptes Rendus*, 1919, **168**, 392–5.

LEBLANC, M., Sur les rotations très rapides, *ibid.* 1919, **169**, 627–33.

JEFFCOTT, H. H., The Lateral Vibration of Loaded Shafts in the Neighbourhood of a Whirling Speed—The Effect of Want of Balance, *Phil. Mag.* (6), 1919, **37**, 304–14.

LEES, S., The Whirling of an Eccentrically-loaded Overhung Shaft, *ibid.* (6) 1919, **37**, 515–23.

EASON, A. B., Critical Speeds of Machinery placed on Upper Floors of Buildings, as Related to Vibration, *ibid.* (6), 1919, **38**, 395-402.

An aerial trailing from an aeroplane in flight takes up a position of relative equilibrium depending on the weight and the air resistance. The form is investigated by J. Hollingworth: "On a New Form of Catenary" (*Phil. Mag.* (6), 1919, **38**, 452–63). In its essentials the problem is that of a flexible string suspended from a fixed point and in equilibrium under its weight and the pressure of a steady wind. The author assumes that on each element of the string there acts a normal force due to the air resistance, and proportional to the square of the normal component of the velocity. The results are compared with the observed forms of aerials (in so far as such observation is at all possible), and the agreement is not unsatisfactory. It would be useful to consider other values of the normal wind force. The present writer has during the last two or three years worked out a number of such cases as lecture exercises, and has obtained some really interesting results.

Sound detection has played an important part in war strategy, and is of perennial interest as a means of safety at sea. The subject is investigated by L. V. King: "On the

Propagation of Sound in the Free Atmosphere and the Acoustic Efficiency of Fog-Signal Machinery. An Account of Experiments carried out at Father Point, Quebec, Sept. 1913" (*Phil. Trans.* 1919, **218**, A, 211–93). King discusses the theory of plane and spherical waves of small amplitude, and the propagation of aerial plane waves of finite amplitude. In the experimental work use was made of Webster's phonometer, described in the *Proc. Nat. Acad. Sci. U.S.A.*, 1919.

The scientific aspect of sound theory has enjoyed much attention. The foremost prominence must be given to C. V. Raman's memoir: "On the Mechanical Theory of the Vibrations of Bowed Strings and of Musical Instruments of the Violin Family, with Experimental Verification of the Results" (Pt. I, *Indian Ass. for the Cult. of Science*, Bull. 15, 1918). A judicious combination of theory and experiment is used to elucidate the nature of the vibrations of the violin. The equations of motion are written down and solved for the strings and the bridge respectively. The effect of the mute is investigated by means of placing loads at various points, whilst the effect is discussed of variations in the method of using the bow.

Further papers on sound instruments and theory of vibrations are:

WEBSTER, A. G., Acoustical Impedance, and the Theory of Horns and of the Phonograph, *Proc. Nat. Acad. Sci. U.S.A.* 1919, **5**, 275–82, where the author solves the equations of motion for the air in narrow channels of various shapes. The results were used in designing horns for use in the war.

FOCH, A., Three papers on channels containing air pockets, *Comptes Rendus*, 1919, **169**, 502–5, 569–71, 687–90.

BANERJI, S., On the Vibration of Elastic Shells Partly Filled with Liquid, *Phys. Rev.* (2), 1919, xiii. 171–88. Three shapes of shell are discussed: hemispherical cup, cylinder with flat base, conical cup.

BARTON, E. H., and BROWNING, H. M., Mechanical "Resonators" under Double Forcing, *Phil. Mag.* (6), 1919, **37**, 453–5, in which the authors discuss an analogy of the resonance theory of audition.

DEY, A., A New Method for the Absolute Determination of Frequency, with Preface and Appendix by C. V. Raman, *Proc. Roy. Soc.* 1919, **95**, A, 533–45, based on the maintenance of sub-synchronous frequency in a pendulum by means of a periodic field of force (see *Phil. Mag.*, Jan. 1915, August 1917).

RAYLEIGH, LORD, Two papers on random vibrations, and unit vibrations with random phases, *ibid.* (6), 1919, **37**, 321–47, 498–515.

Meteorology has been receiving increased attention from experimentalists as well as from applied mathematicians. The following papers should be noted:

HILDEBRANDSSON, H. H., Réflexions préliminaires sur les mouvements généraux de l'atmosphère, *Comptes Rendus*, 1919, **168**, 593–8.

BRAZIER, C. E., Influence de la distribution verticale de température sur les vitesses du vent mesurées au voisinage du sol, *ibid.*, 1919, **168**, 1160–1. It is shown that the ratio wind velocity/gradient of pressure is affected by the vertical temperature distribution.

JEFFREYS, H., On the Relation between Wind and Distribution of Pressure, *Proc. Roy. Soc.* 1919, **96**, A, 233–49.

RAYLEIGH, LORD, The Travelling Cyclone, *Phil. Mag.* (6), 1919, **38**, 420-4.

JEFFREYS, H., On Travelling Atmospheric Disturbances, *ibid.* (6), 1919, **37**, 1–8. An explanation is suggested for the conspicuousness of the cyclonic type of disturbance, other types being dissipated before they can be observed.

The following additional papers have been published recently:

Potential Theory.

SEN, N. R., On the Potential of Uniform and Heterogeneous Elliptic Cylinders at an External Point, *Phil. Mag.* (6), 1919, **38**, 465–79.

PRASAD, J., On a Peculiarity of the Normal Component of the Attraction due to Certain Surface Distributions, *ibid.* (6), 1918, **36**, 475–6.

VÉRONNET, A., Figures ellipsoidales d'équilibre d'un liquide en rotation, variation du grand axe, *Comptes Rendus*, 1919, **169**, 328–31.

JEANS, J. H., The Configuration of Rotating Compressible Masses, *Phil. Trans.* **218**, A, 157–210.

MACMAHON, P. A., and DARLING, H. B. C., Contribution to the Theory of Attraction when the Force Varies as any Power of the Distance, *Proc. Roy. Soc.* 1919, **95**, A, 390.

LÉVY, P., Sur la généralisation de l'équation de Laplace dans le domaine fonctionnel, *Comptes Rendus*, 1919, **168**, 752–5.

Dynamics.

GALITZIN, B., An Apparatus for the Direct Determination of Acceleration, *Proc. Roy. Soc.* 1919, **95**, A, 492–507.

AUCLAIR and BOYER-GUILLON, Sur un accélérographe, *Comptes Rendus*, 1919, **169**, 24–6.

ANDRADE, J., La pesée d'un frottement pendant le glissement rélatif des deux solides en contact, *ibid.* 1919, **169**, 638–9.

SOUTHERNS, L., The Double Suspension Mirror, *Phil. Mag.* (6), 1918, **36**, 477–86.

MORTON, W. B., Note on the Motion of a Simple Pendulum after the String has become Slack, *ibid.* (6), 1919, **37**, 280–4.

EMCH, A., On Closed Curves described by a Spherical Pendulum, *Tohôku Math. Journ.* 1919, **15**, 146–65.

LONGDON, A. C., On the Irregularities of Motion of the Foucault Pendulum, *Phys. Rev.* (2), 1919, xiii. 241–58.

EPSTEIN, P. S., Bemerkungen zur Frage der Quantelung des Kreisels, *Phys. Zeit*, 1919, **20**, 289–94.

LORENTZ, H., Kurze Ableitung der Bewegungsgleichungen des Kreisels, *ibid.* 1919, **20**, 294–6.

RICHARDS, H. C., The Motion of a Rotating Projectile, *Phys. Rev.* (2), 1919, xiv, 198–9.

ANDOYER, H., Sur le développement d'une fonction très générale du rayon vecteur et de l'anomalie excentrique dans le mouvement elliptique, *Comptes Rendus*, 1919, **169**, 315–7.

BOURGET, H., Développement algébrique de la partie principale de la fonction perturbatrice suivant la méthode de Cauchy, *ibid.* 1919, **168**, 83–5.

CHAZY, J., Remarque sur les problèmes des deux corps et des trois corps, *ibid.* 1919, **168**, 81–3.

CHAZY, J., Sur les solutions du problème des trois corps où les trois corps formant un triangle isoscèle, *ibid.* 1919, **169**, 526–9.

OGURA, K., Trajectories in the Irreversible Field of Force on a Surface, *Tohôku Math. Journ.* 1919, **15**, 261–77.

OGURA, K., A Remark on the Dynamical System with two Degrees of Freedom, *ibid.* 1919, **15**, 181–3.

WIESNER, S., Über gewisse an den Bewegungsgleichungen durchgefuhrte Eliminationen, *Phys. Zeit.* 1919, **20**, 346–8.

Elasticity.

MESNAGER, M., Sur un cas de simplification des formules de M. Boussinesq. *Comptes Rendus*, 1919, **168**, 156–8.

PLATRIER, C., Sur les efforts intérieurs dans un corps homogène isotrope en équilibre élastique, *ibid.* 1919, **169**, 378–80.

PLATRIER, C., Sur l'équilibre élastique d'un corps de révolution homogène isotrope soumis à des forces rayonnantes, soit proportionnelles, soit inversement proportionnelles au rayon, *ibid.* 1919, **169**, 169–71.

GWYTHER, R. F., The Equations for Material Stresses, and their Formal Solution, *Phil. Mag.* (6), 1919, **37**.

WALKER, G. W., Surface Reflexion of Earthquake Waves, *Phil. Trans.*, 1919, **218**, A, 373–93.

SOMIGLIANI, C., Sur la théorie des ondes sismiques, *Comptes Rendus*, 1919, **168**, 108–11.

Hydrodynamics.

COTTON, E., Sur la formule de Bernoulli, *ibid.* 1919, **168**, 547–9.

FAURE, E., Sur la force gyroscopique des fluides, *ibid.* 1919, **168**, 395–8.

LECORNU, L., Sur les tourbillons d'une veine fluide, *ibid.* 1919, **168**, 923–6.

KOLOSSOF, J., Sur le mouvement d'un solide dans un liquide indéfini, *ibid.* 1919, **169**, 685–7.

HAVELOCK, T. H., Wave Resistance: Some Cases of Three-Dimensional Fluid Motion, *Proc. Roy. Soc.* 1919, **95**, A, 354–65.

RICHARDSON, A. R., Stream-line Flow from a Disturbed Area, *Phil. Mag.* (6), 1919, **38**, 433–52.

PETOT, A., Sur la théorie analytique des turbines hydrauliques, *Comptes Rendus*, 1919, **168**, 600–3.

LECORNU, L., Sur l'écoulement des fluides, *ibid.* 1919, **168**, 481–4.

BURGERS, J. M., A Proposed Hydraulic Experiment, *Phil. Mag.* (6), 1919, **37**, 595.

COSTER, D., The Rotational Oscillation of a Cylinder in a Viscous Liquid, *ibid.* (6), 1919, **37**, 587–94.

CLAUDE, G., Sur une application nouvelle de la viscosité, *Comptes Rendus*, 1919, **168**, 274–6.

GERLACH, W., Notitz zur Frage nach der Gültigkeit des Stokesschen Gesetzes, *Phys. Zeit.* 1919, **20**, 298–9.

RATEAU, A., Three papers on the escape of gas from a vessel, *Comptes Rendus*, 1919, **168**, 330–5, 435–9, 581–7.

REY, J., Propriétés physiques de la vapeur de pétrole, *ibid.* 1919, **168**, 509–13.

REY, J., Sur l'écoulement de la vapeur de pétrole, *ibid.* 1919, **168**, 1092–5.

JOUQUET and CRUSEAUX, Sur la célérité des déflagrations, *ibid.* 1919, **168**, 820–2.

JOUQUET, E., Sur un problème d'hydraulique généralisée. Écoulement d'un mélange gazeux en combustion, *ibid.* 1919, **169**, 326–8.

ASTRONOMY. By H. Spencer-Jones, M.A., B.Sc., The Royal Observatory, Greenwich

The Generalised Theory of Relativity.—At the time of writing these notes, Einstein's generalised theory continues to be much discussed not only in scientific periodicals, but also in articles in several of the monthly reviews and in the daily Press. Of the popular presentations of views for and against, mention may be made of articles by Prof. Eddington in the *Contemporary Review* for December last, and by Sir Oliver Lodge, in the *Nineteenth Century* for December, and the *Fortnightly Review* for January; also a series of three articles in the *Times Educational Supplement* for 1920, January 22, January 29, and February 5, by H. Wildon Carr, F. A. Lindemann, and A. N. Whitehead respectively. Those who require an account of the theory, freed from mathematics as far as that is possible, and of arguments in favour of or against it, may be referred to the *Monthly Notices of the R.A.S.* for December 1919, in which is given in full the report of a discussion on the theory held at the December meeting of the Royal Astronomical Society. A discussion on the theory was also held at the meeting of the Royal Society on February 5, 1920, for an account of which reference should be made to the *Proceedings of the R.S.* A study of the opening address by J. H. Jeans may be particularly recommended to those interested. The best available summary of the mathematical theory at present available in English is the "Report on the Relativity Theory of Gravitation," by Prof. Eddington, published for the Physical Society of London by the Fleetway Press, 1918 (price 6*s.* net, or in cloth, 8*s.* 6*d.*). This report naturally gives an account of the theory in a very compressed form, and is by no means easy reading. The Cambridge University Press have announced the forthcoming publication of a translation of a work by Prof. Freundlich on the "Foundations of Einstein's Theory of Gravitation." Prof. Freundlich, it may be mentioned, had proceeded to the Crimea in August 1914 to test the existence of the displacement of light rays in the gravitational field of the sun by taking photographs at the total solar eclipse in that month. At that time, the generalised theory of relativity had not been developed, and the only possible results anticipated were either a *nil* result, or the Newtonian deflection of $0''\cdot87$ at the limb. The outbreak of war prevented Freundlich making any observations, as he and the other German observers were interned for a while by the Russian Government.

The weak point in the position of the opponents of the theory, of whom Sir Oliver Lodge is the protagonist, is that

no theory or hypothesis has been put forward by them which is capable of explaining at one and the same time, not only the motion of the perihelion of Mercury, but also the deflection of light rays in a gravitational field. It is easy to put forward possible alternative explanations of the latter phenomenon, but then these "explanations" are merely qualitative, and are not quantitative predictions, as was Einstein's: further, they leave unexplained the movement of Mercury's perihelion, which has long remained an outstanding anomaly in gravitational astronomy, and of which Einstein's theory alone gives a rational explanation. Other theories to account for this anomaly have, indeed, been previously put forward, but in no instance were they convincing. A few hypotheses have been put forward which have appeared qualitatively to explain both phenomena; thus Sir Oliver Lodge has suggested that the dielectric constant of the æther may have a value in the neighbourhood of the sun (or other matter) different from that in free space. Prof. Lindemann has pointed out, however (*Observatory*, **43**, 42, 1920), that the value deduced from the motion of the perihelion of Mercury is $K=6$, whilst that deduced from the deviation of light from the stars is $K=4$. The two results are mutually inconsistent. In a similar way do other explanations fall to the ground.

It may be objected that, in spite of the ability of Einstein's theory to account for the motion of Mercury and the deflection of light rays in a gravitational field, the spectroscopic evidence is against it. The position as regards the existence or not of a displacement of solar spectral lines towards the red is at present somewhat peculiar. As far as observation is concerned, it may be stated that the existence of the shift has been neither definitely proved nor disproved, although the balance of evidence appears against it. The spectroscopists at the Mount Wilson Observatory and elsewhere are doubtless busy at the present time collecting further evidence on this matter. Theoretically there are several interesting possibilities; Sir Joseph Larmor (*Proc. R. S.*, ser. A, **96**, 360, 1919) considers that Einstein's theory does not require a spectral displacement, and Cunningham (*Nature*, **104**, 395, 1919) is inclined to support this conclusion. The question is very carefully discussed by Prof. Eddington in his Report referred to above, and he decides that there is "little chance of evading the conclusion that a displacement of the Fraunhofer lines is a necessary and fundamental condition for the acceptance of Einstein's theory; and that if it is really non-existent, under conditions which strictly accord with those here postulated, we should have to reject the whole theory constructed on the principle of equivalence." This agrees

with Einstein's own view as stated in his article in the *Times*. Jeans, in the opening address referred to above, considers, however, that the difficulty can be avoided by abandoning Einstein's interpretation of *ds*, the fundamental invariant of the theory, as a line-element in a distorted space-time continuum. To the present writer the theory becomes unintelligible when this view is adopted. On the other hand, on Einstein's conception of force, a displacement of the solar lines of exactly the same amount as required by the generalised relativity theory should be expected on Newton's law of gravitation. Those interested in the matter will find that the argument in § 34 of Prof. Eddington's report holds equally well for either law of gravitation, since g_{44} is the same for both. The displacement was actually predicted by Einstein in 1911, before his generalised theory had been developed. The possible existence of the shift, therefore, ceases to some extent to be a crucial phenomenon of the theory. If it is definitely shown that there is no shift, we should not be free from difficulties even accepting Newton's law. The situation is therefore full of interesting possibilities which must be left for the future to unravel.

The Sources of Stellar Energy.—It is well known that no explanation has yet been given of the source of energy in the sun (and therefore in any star) which will account for the age of the earth, as deduced from geological evidence. The hypothesis that the energy is derived from gravitational contraction gives an age which is altogether too short ; nor are matters improved much when radio-activity is taken into account. Various other suggestions have from time to time been advanced, but none has yet been found capable of providing a satisfactory explanation. The various possibilities are discussed in a suggestive manner by Prof. H. N. Russell in *Pub. Ast. Soc. Pac.*, **31**, 205, 1919.

The suggestion has been put forward that the rate of radiation from a hot body to space is considerably less than it would be to a material enclosure at the absolute zero. Russell, however, using the Smithsonian Institution data relative to the amount of energy received by the earth from the sun, shows that the earth radiates heat to space at a rate which is certainly not much less than the rate at which it would radiate to a material enclosure at absolute zero. On the other hand, the ratio required to reconcile the probable age of the sun with a purely gravitational supply of energy is of the order of only 0·001. It is improbable that the difference in behaviour is attributable to any appreciable extent to the difference in wave-length of the emitted radiation, and it is therefore concluded that the sun and stars have actually radiated many

times more energy than would be liberated by their gravitational contraction.

It follows that there exists within the stars some unknown source of energy of enormous magnitude which continually supplies the heat lost by radiation. Russell points out that the process of transformation of this energy must satisfy the following conditions :

(1) It must generate heat in the interiors of the stars, but little or none in the earth or under laboratory conditions.

(2) Since the stars are in general very stable, it must not be liable to accelerate its rate so as to end in an explosive catastrophe.

(3) It must be regulated in such a way as to supply heat to each star at almost exactly the rate at which the star radiates heat to space, so ensuring that the rate of transformation is slow, as we know to be the case from the slowness of stellar evolution.

(4) It must ultimately die down so that a stage will finally result in which the radiation is very slow.

(5) Since the " dwarf " stage of a star's life is very long, a sufficient amount of energy must be available even in the later stages to permit them to be of long duration.

Russell points out that (1) indicates a source of energy liberated only under the extreme conditions of temperature or pressure prevailing in a star ; (4) suggests that the store is limited, and that the rate of transformation falls off as the remaining supply diminishes : complete exhaustion would be approached gradually, thus satisfying (5).

Conditions (2) and (3) can be satisfied as follows : the star at first is a diffuse mass of gas at comparatively low temperature. It contracts rapidly with loss of heat, but, according to Lane's law, with rise of temperature, until at length the unknown supply of energy begins to be liberated. The rate of contraction then slows down ; but the rate of radiation of energy, which Eddington has shown to be determined mainly by the mass and opacity, scarcely changes. The temperature continues to increase, and with it the rate of supply of energy from the unknown source. A stage at length arrives at which this rate balances the loss due to radiation, and the contraction is then checked. What then happens ? Will the temperature go on increasing with the further supply until at length there is an explosion ? Russell points out that this cannot be so because the specific heat of a gaseous star is *negative*. When heat is added to it, it expands and the temperature falls. Thus the further supply of heat reduces the temperature, and therefore the rate of supply from the unknown source. On the other hand, if the supply begins to be

insufficient, the star will contract, causing the temperature to rise and the supply to be increased. Conditions are therefore stable, and a practically steady state results with a very slow rate of evolution, the star shrinking only as the unknown source exhausts itself. This corresponds to the slow evolution of the star through the various spectral types in the giant stage. In the dwarf stages, the temperature and rate of radiation are both decreasing, and less and less energy is required to maintain the steady state.

This hypothesis offers an easy explanation of two astrophysical facts which are otherwise difficult to account for. One is the abrupt commencement of the spectral series with the highly luminous giant stars of type M, and the remarkable infrequency of the very red giants. This is explained if it is supposed that the temperature is not sufficiently high for the unknown source of energy to come into play until type M is reached. The earlier stages would therefore be passed through very quickly, and so few stars would at any time be found in them. Subsequent stages might endure for millions of years. The second fact is the maintenance of variability of the Cepheid type. It is fairly generally accepted that the cause of the variation is some form of pulsation in the star. Eddington has shown that such pulsations would be rapidly damped out by the leakage of heat from the hotter to the colder regions of the star's interior. Russell's hypothesis would, however, supply heat to the interior at the greatest rate where it is hottest, and so would tend to make good the leakage. The liberation of heat would then take place when the star was smallest and hottest, and would be periodic.

Prof. Russell does not speculate as to the nature of the supposed source of energy, but it will be apparent that, if a source could be found which would satisfy the conditions postulated, we should be provided with a rational explanation of the source of stellar energy which is not incompatible with the accepted age of the sun. Prof. Eddington (*Observatory*, **42**, 375, 1919) has suggested, as a possible source, the destruction of electrons by mutual annihilation at high temperatures.

The following are some of the more important papers recently published:

Repsold, J. A., Friedrich Wilhelm Bessel, *Ast. Nach.*, 1919, Bd. 210, Nos. 5027–8.

Wanach, B., Die Chandlersche und die Newcombsche Periode der Polbewegung, *Zentralbureau der Intl. Erdmessung* (*Neue Folge der Veröff.*), 1919, No. 34.

Wilkens, A., Eine Methode der Bahnbestimmung für alle Exzentrizitäten, *Ast. Nach.*, 1919, Bd. 210, Nos. 5022–3.

STRÖMGREN, E., A New Class of Periodic Solutions in the General Problem of Three Bodies, viz in the Problem of Three Bodies with all Masses and all Distances of the Same Order of Magnitude, *M.N., R.A.S.*, 1919, **80**, 12.

PLUMMER, H. C., On the Ellipticities of the Maclaurin Ellipsoids, *M.N., R.A.S.*, 1919, **80**, 26.

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CONRADY, A. E., The Five Aberrations of Lens-systems, *M.N., R.A.S.*, 1919, **80**, 78.

PETTIT, E., The Great Eruptive Prominences of May 29 and July 15, 1919, *Astroph. Journ.*, 1919, **50**, 206.

EVERSHED, J., The Solar Prominence of May 29, 1919, *M.N., R.A.S.*, 1919, **80**, 8.

HARPER, W. E., The Orbit of the Spectroscopic Binary Boss 3511, *Pub. Dom. Obs. Canada*, 1919, **4**, No. 14; The Orbit of the Spectroscopic Binary 19 Lyncis, *ibid.*, No. 15; The Orbit of the Spectroscopic Binary, *h* Draconis, *ibid.*, No. 16.

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MAGGINI, M., The Eclipsing Binary RR Vulpeculæ, and the Evidence of Darkening towards the Limb, *Astroph. Journ.*, 1919, **50**, 141.

HEGER, MARY L., The Occurrence of Stationary D-lines of Sodium in the Spectroscopic Binaries β Scorpii and δ Orionis, *Lick Obs. Bulletin*, 1919, **10**, No. 326.

PHYSICS. By JAMES RICE, M.A.; University, Liverpool.

Relativity.—In view of the widespread interest taken in this subject by scientists and general public alike, it may not be out of place to give a connected summary of this theory, thus linking up and extending the references to it made by Mr. Spencer Jones in the last number of SCIENCE PROGRESS, and by myself in earlier numbers.

Relativity in its broadest aspect is based on the assumption that all measurements and observations reveal only *relations* between the object observed and the observer. In the special physical sense, it is based on the complete absence of experimental evidence that any dynamical, optical or electromagnetic effects arise from the motion of matter relative to

ether or "space" (in the absolute sense of the word). This has long been recognised in the dynamical sphere, and this recognition is implicit in Newtonian dynamics, embracing the three laws of motion and law of gravitation. It was the repeated failure of optical and electrical experiments to detect relative motion to the ether which prompted Einstein, in 1905, to depart from the method pursued by earlier physicists, highly perfected as it had been by Larmor and Lorentz, and work out the logical consequences of the following postulate:

The laws according to which the states of physical systems are changing are the same whether the phenomena are referred to the system of co-ordinates (S) or to any other system of co-ordinates (S') moving *uniformly* with respect to the former.

This postulate was in fact a very broad generalisation of the fact that the only motion which gave any positive effects in optical experiments was relative motion of *matter to matter*, of object observed to observer. Recently Einstein has extended its scope by deleting the word "uniformly"; but more of that presently.

The first application which Einstein made of this postulate was to work out the connection between the spatial and temporal measurements of the observers in the system of reference S, and those in S'. He assumed that the velocity of a moving source added nothing to the velocity of light, a well-known assumption in earlier work (its opposite being consistent only with some form of emission theory of light). As, according to Relativity, motion of observer to or from a source is experimentally indistinguishable from motion of source to or from the observer, it followed that light had the same speed to all observers whatever their motion might be relative to the source. In support of this deduction, we can appeal to a number of experiments made in the nineteenth century, culminating in the famous one carried out in 1886 by Michelson and Morley, and repeated with further refinements by Morley and Miller in 1905. Such a view cannot, of course, be reconciled with the theory that light consists in the transmission of mechanical or electromagnetic oscillations though a *fixed, stagnant* ether; but the belief entertained in some quarters that Relativity is inconsistent with ethereal transmission in some form, appears to have little or no foundation.

The relations which Einstein worked out are embraced in a set of equations arrived at by Lorentz in the first place (but interpreted by him in quite a different sense). The S-observers measure the time of an event (idealised as instantaneous) to be t units of time after a chosen original instant, and the place of its occurrence (idealised as a point) to have co-ordinates x, y, z, relative to their frame of axes (relatively fixed to them). The S'-observers record their observations of this event as

given by time and space co-ordinates t', x', y', z', their axes being fixed relative to them. The equations connecting these eight quantities are a set of four linear equations. They take a simple and sufficiently general form if the axes OX and $O'X'$ have been chosen by the observers to be parallel to their line of relative motion and the origins O and O' to have coincided at the time $t = o$ for the S-observers, and $t' = o$ for the S'-observers.

They become, in that case,

$$\begin{array}{ll} x' = a\,(x - ut) & x = a\,(x' - u't') \\ y' = y & y = y' \\ z' = z & z = z' \\ t' = a\,(t - un) & t = \quad (t' - u'x') \end{array} \Bigg\} \qquad (1)$$

In these, we have assumed the units of length and time to be chosen so that the unit of velocity is the velocity of light; the relative velocity of S' to S, as measured by S, is u parallel to OX, and of S to S', as measured by S', is u' parallel to OX', so that $u' = -u$ (the symbol u is, in reality, the ratio of the velocity in question to the velocity of light). The quantity $a = 1/\sqrt{1 - u^2} = 1/\sqrt{1 - u'^2}$.

Three consequences follow from these equations:

(1) The dimensions of a body are not the same to observers for whom it is fixed as to observers for whom it is in motion. If the first group measure a length in it to be l, the second group measure it to be l/a or $l\sqrt{1 - u^2}$, if it is parallel to the relative motion, and to be l (*i.e.* the same) if perpendicular. That is, bodies are shorter to observers past whom they are moving, in a direction parallel to the motion. (The contraction is, of course, excessively small for usual velocities.)

(2) Two events occur at a locality. The interval between them is measured as t units of time by observers fixed in this locality. This interval will be measured as $a\,t$ or $t/\sqrt{1 - u^2}$ units by observers in relative motion to the locality. That is, all periodic mechanisms appear to go slower to observers in relative motion to them than to observers at rest relative to them. (It is implied, of course, that the mechanisms used by S and S' to measure time would synchronise perfectly if all were at rest to one another, and under direct observation by any one observer.)

(3) An event which occurs at one place is regarded as simultaneous with another event which occurs at another place by a group of observers. The events will not be regarded as simultaneous by any other group of observers who are in relative motion to the first group.

It is this last conclusion which appears to the lay mind as

so contrary to "common sense." But it should be noted that the statement is about two events in different places for which the only test of simultaneity or non-simultaneity is the agreement or disagreement of time-marks recorded on different clocks at the instants of occurrence. There is nothing in this to contradict any impressions made in the mind of a *single* observer as to the time relations *to him* of two events that come under his own direct observation.

It is easily derived from the above equations that if a body (idealised as a particle) moves from a point P (xyz in the S-axes, $x'y'z'$ in the S' axes) to a point Q ($x + dx$, $y + dy$, $z + dz$ in the S axes, $x' + dx'$, etc., in the S') between one instant (t for S, t' for S') and another ($t + dt$ for S, $t' + dt'$ for S'), then dx, dy, dz and dt are connected with dx', dy', and dt', by the very same equations as before, and hence

$$dt^2 - dx^2 - dy^2 - dz^2 = dt'^2 - dx'^2 - dy'^2 - dz'^2. \qquad (2)$$

Therefore $dt.\sqrt{1 - v^2} = dt'.\sqrt{1 - v'^2}$, where v and v' are the velocities of the body relative to S and S' respectively. If there were observers on the body itself so that the body would be fixed in their frame of reference (and for them $v = o$), their estimate of the interval would be $d\tau$, where $d\tau = dt\sqrt{1 - v^2}\ (dt/\beta) = dt'\sqrt{1 - v'^2}\ (dt/\beta')$. This is an element of what is called "proper" time for this body. To observers on the body supplied with the spatial measurements of S but adopting the "proper" time of themselves, the velocity would be $dx/d\tau$, $dy/d\tau$, $dz/d\tau$ and acceleration would be $d^2x/d\tau^2$, $d^2y/d\tau^2$, $d^2z/d\tau^2$. Let us write down as a tentative law of motion

$$\left.\begin{aligned} \mu d^2x/d\tau^2 &= P_x \\ \mu d^2y/d\tau^2 &= P_y \\ \mu d^2z^2/d\tau^2 &= P_z \end{aligned}\right\} \qquad (3)$$

P_x, P_y, P_z being components of a vector and u being a fixed number for the body. If we now turn to the S' observers, they would write the above law as

$$\left.\begin{aligned} &\mu\ d^2x'/d\tau^2 = P_{x'},\ \text{and non-similar} \\ \text{where}\qquad &P_{x'} = a\left(P_x - u\ \mu\frac{d\beta}{d\tau}\right) \\ &P_{y'} = P_y \\ &P_{z'} = P_z \end{aligned}\right\} \qquad (4)$$

Translated from "proper" time to times in the system S, we would have

$$\frac{d}{dt}\left(\mu\beta\frac{dx}{dt}\right) = F_x \text{ and two similar} \qquad (5)$$

where $F_x (= P_x/\beta)$, F_y, F_z are components of force. This law would have the same form in S', viz.:

$$\frac{d}{dt'}\left(\mu\beta'\frac{dx'}{dt'}\right) = F_{x'} \text{ and two similar}$$

if

$$\left.\begin{aligned} \beta' F_{x'} &= a\left(\beta F_x - u\mu\beta\frac{d\beta}{dt}\right) \\ \beta' F_{y'} &= \beta F_y \\ \beta' F_{z'} &= \beta F_z \end{aligned}\right\} \qquad (6)$$

Now in the case of electromagnetic forces, which are believed to-day to cover a very wide range of phenomena, equations (6) are known to be satisfied by the measures of such forces made by observers in different systems of reference, if Maxwell's equations are taken as true. In this region, therefore, we have a law of motion which satisfies the principle of Relativity; *i.e.* maintains an invariant form for different systems of reference. It satisfies Nature also; for in systems for which v is small, it degenerates to Newton's law

$$\frac{d}{dt}\left(\mu\frac{dx}{dt}\right) = F_x, \text{ etc.}$$

and in its wider application it is known to be true in the case of high-speed electrons and β-particles. Its most obvious deduction is the variability of mass; for writing (5) in Newtonian *form*

$$\frac{d}{dt}\left(m\frac{dx}{dt}\right) = F_x, \text{ etc.,}$$

we see that $m = \mu\beta = \mu/\sqrt{1 - v^2}$, which increases as v increases, and attains an infinite value as v approaches unity, *i.e.* the velocity of light.

At this point it is natural to ask if to this variation of *inertial* mass there is a corresponding and proportional change in *gravitational* mass. If there were not, then a body passing through a given point near a gravitating mass would experience the same gravitational force whether moving fast or slow, but would not experience the same acceleration, since in the first case its inertial mass would be greater. Now, as far as the most careful experiments can tell us, the acceleration is independent of the velocity, and so the gravitational force must also change with the mass, *i.e.* with the relative motion of the body. Hence gravitational forces should conform to Relativity. Were we assured that these forces are of electro-

magnetic origin, there would be no difficulty, as mentioned above; but it is a commonplace that gravitation has hitherto obstinately withstood any attempt to bring it within the electromagnetic scheme.

It was in 1911 that Einstein published his first work attempting to bring gravitation into conformity with Relativity, by extending the principle so as to cover systems of reference in variable motion to one another for which the postulate enunciated above is still supposed to hold. The equations connecting the measurements made in one system of reference with those made in another are naturally more complicated than the linear ones written above. In fact, from the purely mathematical point of view, for any four equations connecting $x' \, y' \, z' \, t'$ with $x \, y \, z \, t$ however complex, we could conceive two systems of reference whose metrical relations would be formulated in the equations. As physicists, however, we are concerned solely with such equations of transformation as would transform the mathematical equations for some complex physical phenomenon when expressed in terms of measurements made in one frame of reference to a simpler mathematical form when expressed in terms of measurements made in another frame. What Einstein ultimately succeeded in doing (*Annalen der Physik*, 1916) was to give a criterion for such transformations as are of service to us in the investigation of *natural* phenomena constituting only one portion (but to physicists the vital portion) of all conceivable transformations. The criterion is embodied in a group of differential equations which are usually referred to as Einstein's " law " of gravitation. One simple illustration will give some idea of the application of his method to the problem of phenomena in the neighbourhood of a single gravitating centre. A group of observers moving in any path under the action of the centre would observe that the paths of all bodies in their immediate neighbourhood would for the time being be straight, because observers and bodies would be experiencing the same acceleration towards the centre with reference to axes whose origin is at the centre. Of course, distant bodies would not be moving in straight paths relative to the observers (because in different parts of the field the accelerations are different), and the bodies at the moment adjacent would in time separate from the observers and lose the property of rectilinear motion relative to them. Still, for proximate bodies the frame of reference natural to these observers is one in which motion is the simplest possible—uniform; and so this frame is equivalent to one in which force is absent; *i.e.* we " transform away " the gravitation of this centre in our immediate neighbourhood by choosing such axes, and obtain a very simple equation for an element of an orbit.

If we now replace our measures of length and time bv those made by an observer fixed at the centre to whom we are in relative motion (using the relativity equations which hold at the moment), we get the equation of the orbital element for this observer. The integration does not yield the well-known equation of the ellipse in polar co-ordinates, viz.:

$$l/r = 1 + e \cos \theta$$

but

$$l/r = 1 + e \cos (1 - n)\theta$$

where

$$n = 12\pi^2 a^2/c^2 T^2(1 - e^2)$$

[c is velocity of light; a, T, e semi-axis, period and eccentricity of the approximate ellipse.]

The smallest value of r, viz. $l/(1 + e,)$, occurs therefore not at $\theta = 0, 2\pi, 4\pi$, etc., but at $\theta = 0, 2\pi/(1 - n), 4\pi/(1 - n)$, etc.; *i.e.* we have a progressive displacement of the perihelion by an amount practically equal to $n.2\pi$ each revolution. The calculated amount for Mercury by this formula is 42″ per century, which just accounts for the well-known Leverrier discrepancy.

Einstein, using the "principle of equivalence," also postulates that to the moving observers a ray of light *in vacuo* also has the simplest path in their neighbourhood, viz. straight, and shows by the same transformation that it is curved in the fixed frame of reference, a result confirmed qualitatively and quantitatively by the recent Eclipse expedition.

There still remains the third crucial test concerning the displacement of the solar spectral lines toward the red by an amount ·008 Ångstrom units. Hitherto it has remained unconfirmed; but it is only fair to add that, at the moment of writing this article, a short note has appeared in *Nature* to the effect that Einstein, in a private communication to the correspondent of *Nature*, claims that two physicists working at Bonn have detected it with certainty. No doubt the work will shortly be published, and, if possible, reference to it will be made in these notes in the next issue of SCIENCE PROGRESS.

PHYSICAL CHEMISTRY.

By PROF. W. C. McC. LEWIS, M.A., D.Sc., University, Liverpool.

Transition of Dry Ammonium Chloride.—The substance, ammonium chloride, has had a unique interest for chemists for the past ten years or more, owing to the anomalous behaviour of its vapour in respect of vapour pressure. Baker showed many years ago that thoroughly dried chemical substances

were in general very inert, chemically speaking. Thus, ordinary ammonium chloride—to take a particular case—which is not dried in the extreme sense of the term, on being vaporised is very largely dissociated into ammonia and hydrochloric acid. If, on the other hand, the solid is dried with extreme care, the vapour does not dissociate sensibly, as Baker showed experimentally. (The presence or absence of dissociation is shown by the value of the vapour density.) Johnson, in 1908, measured the vapour *pressure* of the undried and dried ammonium chloride and found that the vapour pressures are the same, from 212° to 333° C., but that the vapour density of the undried material at 333° C. corresponded to 98 per cent. dissociation, whilst that of the dried material at the same temperature and exerting the same pressure corresponds to only 8 per cent. dissociation. It follows that the partial pressure of *un*dissociated ammonium chloride vapour in equilibrium with the undried solid must be small, whilst that in equilibrium with the dried solid accounts for practically the whole observed pressure.

Abegg was the first to call attention to the existence of this anomaly, namely, the discrepancy between the two values of the partial pressure of the undissociated molecules. If the solid material, dried or undried, were the same substance it follows necessarily that the vapour pressure of the undissociated molecules should be identical likewise. Since this is not the case there must be some fundamental difference between the two solids. Wegscheider suggested that we are really dealing with two allotropic forms of solid ammonium chloride, one of them, the unstable one, having a considerably greater pressure than the stable one at the same temperature. In the absence of water, *i.e.* in the dried material, according to Wegscheider, the transition from one form to the other cannot take place. On Wegscheider's view it is a pure accident that the pressure of the undissociated vapour over the dried material is numerically the same as the total pressure due to NH_4Cl, NH_3, and HCl, which exist over the stable form.

Scheffer, in 1915, actually discovered two allotropic forms of solid ammonium chloride, with a transition point at 184·5° C. At first sight this looks greatly in favour of Wegscheider's view. Scheffer, however, determined the heat of transition, and the specific heats of the two forms, and found that the heat effect was too small to account for the facts on Wegscheider's hypothesis. (There is a well-known relation connecting the magnitude of the heat effect with the vapour pressure.) Wegscheider's hypothesis loses, therefore, a great deal of its probability. Recently Smith, Eastlack, and Scatchard (*J. Amer. Chem. Soc.*, **41**, 1961, 1919) have cast still further

doubt on the hypothesis by experiments on ammonium chloride which had been dried with exceptional care. The new fact which this investigation has established is that the really dry substance does undergo the Scheffer transition. Wegscheider's hypothesis, therefore, falls to the ground if we identify his allotropic forms with those discovered by Scheffer. What is obviously required, as indeed the authors point out, is to make simultaneous determinations of vapour density and transition points, to see whether a given sample undergoes transition and at the same time gives rise to *un*dissociated vapour.

The Chemical Constant.—It is well known that the thermodynamic theory of chemical reactions has for its object the expression of the equilibrium constant as a function of the temperature. The classical theory permits us to calculate all the necessary terms with one exception, the integration constant. In order to determine this quantity it was necessary to make at least one experimental determination of the equilibrium constant itself. The great merit of the heat theorem of Nernst lies in the fact that it enables us to obtain a value for the integration constant without actually measuring the equilibrium, provided we have made measurements of the vapour pressure of each separate substance at different temperatures. Each single vapour pressure expression contains a term called the "chemical constant" of the substance, and the integration constant referred to above in connection with the chemical equilibrium is simply the algebraic sum of such chemical or individual constants, obtainable from the vapour pressure data.

Attempts have naturally been made to analyse still further the chemical or individual constant of any single substance with the object of expressing it in a form which shall contain a term independent of the nature of the substance and another term or terms characteristic of the substance. There is, however, difference of opinion regarding the dimensions of the chemical constant, one view being that it has the dimensions of the logarithm of a pressure, the other that it has the dimensions of the logarithm of a pressure divided by a temperature to the power 5/2. The attempt to distinguish between these two possibilities has been made recently by Lindemann (*Phil. Mag.*, Jan. 1920). It is not possible to enter closely into the argument.

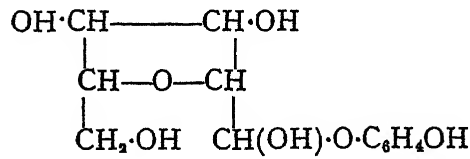
Suffice it to say that the problem involves a choice between two possible values to be ascribed to the atomic heat of a monatomic gas in the neighbourhood of absolute zero. A so-called "degradation" theory has been advanced in recent years, according to which the atomic heat of such a gas will diminish to zero. The classical theory leads us to expect,

on the other hand, that the atomic heat will give a constant value (5/2)R. Lindemann points out that the problem may be settled provided sufficiently accurate data are obtainable for the chemical constant of a number of individual substances. Such data are furnished by the work of Egerton (*Phil. Mag.*, Jan. 1920), and the results show decisively that the atomic heat of a monatomic gas maintains its value (5/2)R down to the lowest temperatures. From this it is to be concluded that the chemical constant has the dimensions of the logarithm of a pressure divided by a temperature to the power 5/2. This conclusion is important in that it involves the rejection of the "degradation" theory of gases at low temperatures, a theory upon which considerable stress has been laid in recent years.

ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., University College, London.

IN October of last year (SCIENCE PROGRESS, 1919, **54**, p. 215) reference was made to a paper by Pictet and Cramer on the distillation under reduced pressure of egg albumen. Since then two other papers have been published by Pictet and his pupils on the distillation of sodium stearate and of certain glucosides. In the first paper, by Pictet and Potok (*Helv. Chem. Acta*, 1919, **2**, 501), the authors describe the distillation under 12–15 mm. of 1 kg. of sodium stearate, from which they obtained a distillate consisting of a mixture of hydrocarbons such as decane, tetradecane, pentadecane, and tetratriacontane, the latter in largest quantity. Sodium oleate under similar conditions gave a number of unsaturated hydrocarbons including nonylene, decylene, undecylene, and tridecylene. The physical properties of the saturated and unsaturated hydrocarbons agree with those occurring in Pennsylvanian petroleum, which fact is regarded as supporting Engler's hypothesis of the origin of natural petroleum from the fats of marine plants and animals. The entire absence of cyclic hydrocarbons and optically active naphthenes among the products of distillation, in the author's opinion, points to a different origin for these compounds when they occur in natural petroleum, where it is assumed that they may be derived from the decomposition of such substances as the resins or terpenes. In the second paper, Pictet and Goudet (*Helv. Chem. Acta*, 1919, **2**, 698) draw attention to the fact that, although the majority of naturally occurring glucosides are lævorotatory, they yield glucose on hydrolysis; by the distillation under reduced pressure of salicin, arbutin, and phloridzin, the authors have in each case obtained a certain quantity of lævoglucosan; since they have also been able to

isolate lævoglucosan from these three substances by hydrolysis by means of barium hydroxide, they consider that these compounds are in reality derivatives of lævoglucosan, and not of glucose. On this hypothesis the constitution of arbutin, for example, would be represented as follows :



Reference has already been made in these columns to the theory of alcoholic fermentation and to the products formed during this complex change (SCIENCE PROGRESS, 1919, **52** and **54**). In a paper recently published by Neuberg and Reinfurth (*Berichte*, 1919, **52**, [B], 1677) attention is once more drawn to the fact that in the presence of sulphites, especially calcium sulphite, the acetic aldehyde formed as an intermediate product is fixed in form of its bisulphite compound ; the available hydrogen, no longer being able to reduce the aldehyde, is therefore employed in attacking other substances, and glycerol therefore results in practically equimolecular proportions to the amount of acetaldehyde produced. Under these conditions the fermentation proceeds according to the equation

$$C_6H_{12}O_6 = CH_3CHO + C_3H_5(OH)_3 + CO_2,$$

and about 73 and 70 per cent. of the quantities of acetaldehyde and glycerol respectively required by the above equation are produced.

The separation of potato tyrosinase into two constituents is described by Haehn (*Berichte*, 1919, **52**, [B], 2029). By filtration this enzyme yields a thermolabile residue α-tyrosinase and a thermostable filtrate or activator, neither of which alone is active, while on reuniting these two substances, the resulting mixture regains its original activity. The inactive residue can also be rendered active by adding to it an aqueous solution of the ash obtained by igniting the juice of potatoes expressed with dilute acetic acid.

A number of papers have recently appeared dealing with the colouring-matters of plants. Everest and Hall (*J. Soc. Dyers*, 1919, **35**, 275) have been examining the tinctorial properties of some anthocyans (glucosides) and their related compounds the anthocyanidins, obtained from them on hydrolysis. Both classes of compounds dyed full shades on tannin-mordanted cotton from a faintly acid bath, but on wool,

mordanted with a metallic oxide, the anthocyans have scarcely any tinctorial power, whereas the anthocyanidins dye readily. It would thus appear that the tannin mordant acts by virtue of the pyran ring containing basic oxygen; on the other hand, the affinity for the metallic mordant is presumably due to the phenolic hydroxyls of the catechol nucleus which, in the glucosidic anthocyans, are masked by the sugar residues. The shades produced in all cases are very beautiful, but are not fast to washing, and are, moreover, very sensitive to acids or alkalies. With the object of determining whether chlorophyll and carbon dioxide form an additive compound during the assimilation of the latter in green leaves, Kremann and Schniderschitsch (*Monatsh.*, 1916, **37**, 659) have determined the solubility of carbon dioxide in alcohol and in alcoholic solutions of chlorophyll both in the light and in the dark, and have found it to be practically the same in all cases; it is concluded that there is therefore no measurable formation of an additive compound, and if formed in the living plant, it must be in almost unrecognisable quantity. Osterhout (*Amer. J. Bot.*, 1918, **5**, 511) describes a simple method of demonstrating the production of aldehyde from chlorophyll. A bell-jar was lined with filter-paper which had been sprayed with a carbon tetrachloride solution of chlorophyll, the solvent being removed by evaporation. The filter-paper was moistened with water, and the jar was inverted over a small dish of water, sealed from the air, and exposed to sunlight. When the paper was bleached to a pale green, the water in the dish gave a positive test for aldehydes. The same result was obtained whether carbon dioxide was entirely excluded from the air in the jar, or whether its concentration was increased to 10 per cent., showing that the aldehyde was produced from the chlorophyll rather than from the carbon dioxide. Similar results were obtained when the chlorophyll was replaced by a number of aniline dyes, notably methyl green and iodine green.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

Regional and Stratigraphical Geology.—A considerable amount of important work upon British stratigraphy has recently been published. Portions of the volume relating to the British Isles in the *Handbuch der Regionalen Geologie* continue to appear. Prof. J. W. Gregory deals with the geomorphology of Scotland, and the Pre-Cambrian rocks of that country (3 Bd., 1 Abth., 1916, pp. 13–17, 34–42); and Mr. J. Parkinson contributes an account of the geology of the Channel Islands (*ibid.*, pp. 334–41).

Prof. Garwood and Miss Goodyear deal with the Longmyndian and Silurian of the Old Radnor district, and note an algal development in the Woolhope Limestone (*Q.J.G.S.*, 1919, **74**, pt. 1, 1–30); while Mr. L. D. Stamp describes the uppermost Siluran rocks of the Clun Forest district, and demonstrates an uninterrupted transition from Upper Silurian through Downtoniani to the Old Red Sandstone (*Q.J.G.S.*, 1919, **74**, pt. 3, 221–46).

The Coal Measures and Magnesian Limestone of Durham are described by C. T. Trechmann and D. Woolacott in two papers, that on the Magnesian Limestone being by Dr. Woolacott alone (*Geol. Mag.*, 1919, 203–11; 452–65; 485–98). Mr. Linsdall Richardson continues his detailed researches on the Jurassic of the West of England in a paper upon the "Inferior Oolite and Contiguous Deposits of the Crewkerne District, Somersetshire" (*Q.J.G.S.*, 1919, **74**, pt. 2, 145–73). Mr. H. J. Lowe gives a useful résumé of the Tertiary geology of Devonshire and Cornwall (*Trans. Devon. Assoc. for the Adv. of Science, Literature, and Art*, 1918, **1**, 391–401).

Knowledge of the regional geology of the North Atlantic-Arctic region has been considerably advanced by the publication of a group of important papers. The Geological Survey of Norway has published a large memoir upon the geology of the Kongsberg district, an Archæan region containing important silver mines associated with fahlbands believed to be cònnected with diabase dykes of later age (*Norges Geol. Undersök.*, 1917, **82**, 272, with English summary).

An attempt has been made by O. Holtedahl to outline the probable distribution of land and sea in the North Atlantic and Arctic regions during various periods of Palæozoic time (*Amer. Journ. Sci.*, 1920, **49**, 1–25). This brings out the remarkable homology in geological structure, and relative ages of folded zones, between eastern North America and North-western Europe, although the arrangement of the different structural elements is reversed in the two regions.

Baron de Geer has written what will become a classical paper on the tectonic history of Spitsbergen under the too modest title of "The Physiographical Evolution of Spitsbergen" (*Geografiska Annaler*, 1919, H. **2**, 161–92). Its scope is best indicated in his own words: "The plan of this paper is to describe the main structure-lines of Spitsbergen, its division into a Northern Oldland and a marginal horst along the west coast. Further, an extended Cretaceous base-level plain, a great Tertiary submergence, followed by a late Tertiary re-elevation of the horst and eastward overfolding of the adjacent table-region; considerable faulting along the west coast and the fjords, in the latter case proved by measured warpings of

intra-fjord blocks, all those dislocations having determined the present attitude of the coal horizons. Finally, the coast- and fjord-topography of Spitsbergen is compared with that of Scandinavia, and conclusions are drawn concerning the probably analogous physiographical evolution of Northern Europe and the recent land configuration in general."

An important paper by K. A. Grönwall (*Medd. om Grønland*, 1917, **43**, 511–617) shows that North-eastern Greenland possesses a series of marine Upper Carboniferous rocks, which, like those of Spitsbergen, transgress over Lower Carboniferous measures of continental character. The rocks were deposited in very shallow water, or even in the littoral zone, as shown by the presence of fine-grained conglomerates. The brachiopod fauna has a decided Russo-Arctic character, and points to an Upper Carboniferous north polar sea stretching from Russia, through Spitsbergen, to North Greenland and Arctic North America.

Basing his notes on the work of Thoroddsen and Suess, Mr. E. B. Bailey discusses the significance of the volcanic arcs and arcuate fractures of Iceland (*Geol. Mag.*, 1919, 466–77); and suggests that that country "may well supply a stepping-stone between arcuate fractures of the type so familiar in Scotland (Glencoe, Skye, Mull), with their accompaniment of ring dykes, and the vastly greater Aleutian arc, with its serial volcanoes."

North American Pre-Cambrian geology is dealt with in the three papers noted below ("The Harricanaw-Turgeon Basin, Northern Quebec," *Geol. Survey of Canada*, Mem. **109**, 1919, p. 84; "Pre-Cambrian Rocks of South-east Newfoundland," *Journ. Geol.*, 1919, **27**, 449–79; "Some Problems of the Adirondack Pre-Cambrian," *Amer. Journ. Sci.*, 1919, **48**, 47–68).

The regional and stratigraphical geology of three widely-separated Canadian areas is described in new *Geological Survey Memoirs* ("The Silurian Geology and Faunas of Ontario Peninsula, and Manitoulin and Adjacent Islands," Mem. **111**, 1919, p. 195; "Geology of the Disturbed Belt of South-western Alberta," Mem. **112**, 1919, p. 71; "The Mackenzie River Basin," Mem. **108**, 1919, p. 154).

A bulletin of the *Geological Society of America* (1918, **29**, No. 4) is devoted to a symposium on the geology, palæontology, and palæogeography of the Antillean region, with Central America and Mexico. The titles of the two most general papers are quoted (Stanton, T. W., "Mesozoic History of Mexico, Central America, and the West Indies," pp. 601–6; Vaughan, T. W., "Geologic History of Central America and the West Indies during Cenozoic Time," pp. 615–30). With these should be associated T. W. Vaughan's comprehensive work on "Fossil

Corals from Central America, Cuba, and Porto Rico, with an Account of the American Tertiary, Pleistocene, and Recent Coral Reefs" (*Smithson. Inst., U.S. Nat. Mus. Bull.*, 1919, **103**, 189–524), which is further notable as containing a full statement of Vaughan's views on coral-reef formation, to be dealt with more fully in forthcoming notes.

Aequinoctia is the name given by Abendanon (*Journ. Geol.*, 1919, **27**, 562–78) to an old Palæozoic continent which extended over 45 degrees of latitude between South-east Asia and the east of Australia. The remains of its nucleus of Archæan and Pre-Cambrian rocks are to be found in many of the East Indian Islands. The dismemberment of this continent began in the Permo-Carboniferous, and continued during the Mesozoic and Tertiary.

Falklandia is a name applied by Clarke (*Proc. Nat. Acad. Sci.*, 1919, **5**, 102–3) to a continental land which existed in the occidental parts of the Southern Hemisphere during the Devonian period, and preceded Gondwanaland and Antarctis. The Devonian of these latitudes is a unit both in life and in sedimentation. Falklandia traverses part of Schwarz's Flabellites Land, which name is superseded for no very clear reason.

Dr. W. Howchin's important work on the "Geology of South Australia" (Adelaide, *Education Dept.*, 1918, p. 543), and J. C. Branner's "Outlines of the Geology of Brazil" to accompany the Geological Map of Brazil (*Bull. Geol. Soc. Amer.*, 1919, **30**, 189–338), are too comprehensive to comment upon profitably in a short paragraph.

Petrology of Igneous Rocks.—Dr. N. L. Bowen has replied to recent criticism by Prof. Daly relative to his theory of differentiation by crystallisation (*Journ. Geol.* 1919, **27**, 393–435). He maintains the relative unimportance of syntaxis as a factor in producing the known diversity of igneous rocks, and denies even the possibility of liquid immiscibility in silicate magmas. He accentuates the importance of the "straining-out" process of residual magma at a late stage in crystallisation—filter-press action—as a factor in differentiation; and prefers this, combined with ordinary sinking and zoning of crystals, to Grout's convection theory, as an explanation of the differentiation of the Duluth lopolith (see Science Progress, Oct. 1919, p. 219).

From the Strathbogie (Aberdeenshire) and Lower Banffshire district, H. H. Read describes igneous rocks of two petrogenetic cycles, separated by an epoch of great earth movement which caused the foliation of the Dalradian Series (*Geol. Mag.*, 1919, pp. 364–71). The magmas have each given rise to a series of differentiates, ranging from anchimonomineralic types to granites, with various norites and gabbros as dominating

members. There is a well-marked similarity in characters and sequences between the two magmas, although the similarity does not extend to details.

Mr. J. F. N. Green has made a comprehensive study of the vulcanicity of the Lake District (*Proc. Geol. Assoc.*, 1919, **30**, 153–82), in which he comes to the conclusion that the special features of the lavas and tuffs indicate that they were accumulated on the sea floor probably at a considerable depth. He also emphasises the importance of the evanescent gaseous and liquid constituents of magmas in producing mineralogical and structural modifications of the resulting rocks.

Mr. J. Morrison presents a notable contribution to Lake District petrography in his paper on the Shap minor intrusions (*Q.J.G.S.*, 1919, **74**, pt. 2, 116–44). He appeals to a rather complicated series of processes, including gravitative differentiation and syntaxis, as important factors in producing the observed diversity of rock types.

An interesting study of the basal phases of the great Duluth gabbro mass and its contact effects near Gabamichigami Lake, Minnesota, has been made by the late Dr. M. L. Nebel (*Econ. Geol.*, 1919, **14**, 367–402). He believed that the gabbro had contributed material, especially metallic sulphides like those of Sudbury, to the metamorphosed rocks.

According to H. C. Cooke (*Geol. Surv. Canada, Mus. Bull.*, No. 30, 1919, p. 48), a large gabbro complex at East Sooke (Vancouver Island) consists mainly of olivine-gabbro, with subordinate quantities of augite-gabbro, anorthosite, and granite, disposed in such a way as to indicate gravitative differentiation as the geological process concerned in their mutual arrangement. After consolidation the complex was intruded by two series of satellitic dykes, gabbro-aplites and granite-aplites, in the order given. Hornblendisation by hot solutions took place still later along broad shear zones.

The plutonic mass of Cape Willoughby (South Australia) is an adamellite, with minor intrusions of microcline-aplite and albitite (Tilley, *Trans. Roy. Soc. South Australia*, 1919, **43**, 316–41). In form it is chonolithic, and is arranged, along with other granitic intrusions, parallel to the strike direction of the older Palæozoic folding. According to the same author, certain quartz-tourmaline nodules in this granite owe their origin primarily to pneumatolytic processes (*ibid.*, pp. 156–65). The term *pneumatolith* is proposed for nodules resulting from these processes.

The following are further references to recent important work:

Davis, W. M., The Framework of the Earth, *Amer. Journ. Sci*, 1919, **48**, 225–41.

WILLIS, B., The Discoidal Structure of the Lithosphere, *Proc. Nat. Acad. Sci.*, 1919, **5**, 377–83.

BARRELL, J., The Nature and Bearings of Isostasy, *Amer. Journ. Sci.*, 1919, **48**, 281–90; The Status of the Theory of Isostasy, *ibid.*, 291–338.

COX, A. H., A Report on Magnetic Disturbances in Northamptonshire and Leicestershire, and their Relation to the Geological Structure, *Phil. Trans.*, Ser. A., 1919, **219**, 73–135.

BRYAN, K., The Classification of Springs, *Journ. Geol*, 1919, **27**, 522–61.

GREGORY, J. W., A Low-Level Glaciated Surface in the Eastern Himalaya, *Geol. Mag.*, 1919, pp. 397–406.

SHAW, E. W., Present Tendencies in Geology—Sedimentation, *Journ. Washington Acad. Sci.*, 1919, **9**, 513–21.

KINDLE, E. M., Inequalities of Sedimentation, *Journ. Geol.*, 1919, **27**, 339–66.

GALLOWAY, J. J., The Rounding of Grains of Sand by Solution, *Amer. Journ. Sci.*, 1919, **47**, 270–80.

KINDLE, E. M., A Neglected Factor in the Rounding of Sand Grains, *ibid.*, 431–4.

WENTWORTH, C. K., A Laboratory and Field Study of Cobble Abrasion, *Journ. Geol.*, 1919, **27**, 507–21.

GILES, A. W., Brecciation in the Niagara Limestone at Rochester, New York, *Amer. Journ. Sci.*, 1919, **47**, 349–54.

RASTALL, R. H., The Mineral Composition of the Lower Greensand Strata of Eastern England, *Geol. Mag.*, 1919, pp 211–20, 265–72.

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., University College, London.

Anatomy and Cytology.—Several additional species of Monocotyledons exhibiting intrafascicular cambium are recorded by Mrs. Arber. These belong to the genera *Juncus, Anigozanthos, Narcissus, Crocus, Iris, Moræa, Sisyrinchium, Carludovica,* and *Sciaphila* (*Ann. Bot.*). In the same journal Carter describes the detailed structure of the chloroplasts in *Chætomorpha, Cladophora,* and *Rhizoclonium*. In all cases it appears that these form a parietal film, more or less perforated, from which strands pass inwards when the cell contents are abundant.

Worsdell, in an extensive paper dealing with the medullary phloem in the stems of the Compositæ (*Ann. Bot.*), concludes that here also this is a vestige of a system of medullary bundles. These latter are regarded as having been retained in the petioles of certain species such as *Inula helenium* and *Petasites officinalis*. The feature is attributed to a geophytic habit in the ancestors of the group.

Morphology.—Several interesting papers dealing with the marine algæ have, during the past year, appeared in the *Journal of Botany* from the pen of Dr. Church. These not only summarise much of the scattered literature, but are also very

suggestive. In dealing with the zoid of the Phæophyceæ, the lateral position of the flagella is regarded as a derived one. The anterior is supposed to be a survival of the plankton state, which had for its function the vertical traction of the organism towards the light. It has been retained, in still-water forms, as a means of approximating the gametes, but the flagella tend to diminish in size in algæ of disturbed water.

In "Plankton Phase and Plankton Rate," the same writer brings together the data as to concentration of organisms, which are suspended and autotrophic, in the surface of the sea. Lohman, in Kiel Bay, estimated this to be from 500,000 to 2½ millions per litre, though the Plymouth data are lower. Dr. Church construes the inevitable supposition of autotrophic organisms as the first form of life, as indicating the existence of a "Plankton Epoch." Benthic organisms are compared with plankton on the basis of their output of gametes. The amount of plankton is possibly determined by the concentration of P and N ions, all of which are approximately present in the proportion of 1 in 10 million.

Economic.—The application of Botanical Science to economic problems has been vigorously prosecuted of recent years, especially in the Colonies. Prof. Cockayne, in New Zealand (*N.Z. Journ. Agric.*), has been investigating the Montane Tussock-Grassland, which is dominated by *Festuca novæ-zealandiæ*, but comprises some 250 spp. of flowering plants, of which 40 are introduced. Of this large number only a few are eaten by the mountain sheep which are here pastured. The prevalence of the inedible species is such that much of the area only carries one sheep to five acres. The nutritious species are kept in check through grazing by rabbits, and fires likewise favour undesirable forms. *Danthonia pilosa* is the species most to be encouraged, as also some introduced plants such as *Trifolium repens* and *Dactylis glomerata*. Prof. Cockayne gives useful data obtained experimentally on the relative palatability of various species, which showed that sheep preferred *Poa pratensis*, *Dactylis glomerata*, *Hierochloe redolens*, *Danthonia semi-annularis*, *Holcus lanatus*, and *Hypochœris radicata*. This was in a mixed pasture with numerous species, most of which (some 80 spp.) were untouched except by rabbits.

M. Renouard (*La Nature*, Oct. 1919) has dealt with the textile fibres experimented upon by the Germans during the war to replace the imported cotton and jute. Considerable hopes were based on the Nettle, and several large companies, one with a capital of over 15,000,000 marks, were floated for its exploitation. The nettle, however, only yielded 8 to 12 per cent. of very fine fibre from 20 to 60 mm. in length, or about 33 lb. per acre. The fibres of Broom, as in France,

were used to mix with those of Flax, but here again the yield was small. Other sources employed were Peat, Pine-needles, Cotton Grass, and *Typha angustifolia*, of which the last was the most important.

Messrs. Rowes & Chittenden (*Journ. Roy. Hort. Soc.*) summarise the results of experiments at Wisley, on the effect of grass on apple-trees, which have now extended over seven years. They confirm Pickering's conclusions. Five varieties in grass only yielded 23 apples throughout this period, whilst an equivalent number of trees of the same varieties grown in cultivated land yielded 811 apples. Trees with grass to within 1ft. 6ins. of the trunk gave an intermediate yield of 185 apples, and growth measurements showed the same gradation for the three sets of conditions.

The causes which have led to the failure of regeneration of oak woods in Britain have been investigated by A. S. Watt (*Journ. of Ecology*). The results obtained show that the greater part of the acorns which fall are destroyed by animals, probably mainly, as was suggested by Cobbett (*Woodlands*, 1825), by mice. The conclusions of Hickel are also confirmed, that acorns are very susceptible to loss of water, and readily lose their viability from this cause. The mortality amongst germinated acorns is also shown to be largely the effect of herbivorous animals.

Taxonomy.—Recent numbers of the *Kew Bulletin* furnish descriptions of a number of new species belonging to the following genera: *Aconitum, Anisophyllœa, Brachiaria, Bruguiera, Chrysopogon, Desmodium, Eriocaulon, Erythrina, Indigofera, Kalanchoe, Kniphofia, Linociera, Memecylon* (5 spp.), *Panicum* (3 spp.), *Pimpinella, Rosa, Rutenbergia, Sarcococca, Schefflera* (2 spp.), *Scyphosyce, Smithia, Sonerila, Vigna.*

In "Notes from the Royal Botanical Garden, Edinburgh," Prof. Bailey Balfour describes 45 new species of *Rhododendron*, and Hutchinson 13 spp. of the same genus. In the same journal, Craib describes 9 new species of *Enkianthus*, and 11 of *Primula*.

In the *Journal of Botany*, new species belonging to the following genera are described: *Gardenia, Oxyanthus, Atractogyne, Pavetta, Ratidea* (3 spp.), and *Randia* (Wernham); *Chætocarpus* and *Dendro-cousinsia* (Fawcett and Rendle).

A new genus of fresh-water algæ placed in Hydrogastraceæ and termed *Urnella* is recorded by Playfair (*Proc. Linn. Soc., N.S. Wales*), besides nearly 60 new species and varieties of other genera. Numerous species of marine algæ from the Pacific Coast, chiefly belonging to the Myxophyceæ, have been described by Gardner (*Univ. Calif. Bot.*, 1918 and 1919).

New British Fungi belonging to *Hypochnus* (2 spp.) and

Helotium (Wakefield), *Mycena* (Pearson), *Boydia, n.g.* (L. Smith), and *Empusa* (Cotton), are described in *Trans. Mycol. Soc.*

PLANT PHYSIOLOGY. By FRANKLIN KIDD, D.Sc., Botany School, Cambridge. (Plant Physiology Committee)

Respiration.—Professor Osterhout has recently turned his attention to the study of respiration, and a series of papers by him and by other workers associated with him under the general title of *Comparative Studies in Respiration* are now appearing in the *Journal of General Physiology* (vols. i. and ii.). The *raison d'être* for this series of studies is to be found in the experimental method and apparatus used. These are ingenious, and are stated to allow of rapid and continuous measurements of extremely small quantities of CO_2 taken in or given out by the plant tissues studied. The outline of the method is as follows : a small quantity of tissue is placed in a tube containing water and an indicator (and other substances in solution as the case may be), or the tissue is placed in one tube and the indicator in another, a current of air being circulated so as to bubble through both solutions. The carbon dioxide evolved by the plant causes an increase in H-ion concentration and consequently a change in the colour of the indicator. An arbitrary interval in tint is chosen which by the standard buffer mixture method can be expressed absolutely as corresponding to a definite change in H-ion concentration. The relation of the arbitrary change in H-ion concentration chosen to the actual amount of CO_2 given out by the tissue has not been attempted and is undoubtedly complex. It is, nevertheless, clear that, other things being the same, variations in the time taken to produce a definite though arbitrary change in H-ion concentration correspond to variations in rate of CO_2 production. Moreover, if the rate of CO_2 production is rapidly changing, the shorter the time intervals the closer will be the correspondence. The smaller the arbitrary change in H-ion taken as a basis the better. The order of change in these experiments is from P_h 7·60 to P_h 7·25, and the order of time interval three minutes. To sum up, the method aims at measuring the times taken for a tissue under observation to give off arbitrary and unknown but small and equal quantities of CO_2. Changes in rate of respiration recorded are expressed in percentages of the normal rate. The advance made is that changes in rate can be followed practically from minute to minute and in this way phenomena can be observed which would completely escape notice in any method which requires several hours for a single observation. Such methods only

give the average rate for the whole of such a relatively long period, and so may miss very large changes of rate which only last for a short time.

This series of studies has so far dealt mainly with the effect of ether upon the respiration of bacteria, moulds and seeds, and with the effect of salts [NaCl, KCl, and $CaCl_2$] in the first two cases. The effect of ether is found to be the same as has been previously recorded, except that by means of this method of closer observation it appears that the changes are quicker and larger than was to be expected. The effect of ether is to produce an increase in the rate of respiration which is not maintained but gives way later to a decrease. The stronger the dose the quicker and more severe the result. The maximum increases in rate recorded as the result of the strongest dose used, *i.e.* a saturated solution of ether in water (7·3 per cent.), are as follows : Aspergillus niger, 150 per cent. normal in two minutes ; Bacillus subtilis, 5,000 per cent. normal in one minute ; wheat-seed, 150 per cent. normal in fifteen minutes approximately.

The effect of salts upon the respiration of Bacillus subtilis is in the same sense as that of ether, namely, low concentrations cause an increase and higher concentrations a decrease in the rate of CO_2 evolution. The changes are, however, much milder, and though they occur just as quickly, the changed rate is maintained at a constant value. NaCl, $CaCl_2$, and KCl, $CaCl_2$ are stated to antagonise each other in their effect upon respiration.

Perhaps the most notable contribution that has recently been made towards our knowledge of the chemical complexity of respiration is that contributed by H. A. Spoehr in one of the publications of the Carnegie Institute of Washington, 1919, and entitled "The Carbohydrate Economy of Cacti." The most interesting section of this paper is the introduction, which brings under review in an able and illuminating manner for the biologist converging lines of experimentation in chemistry, physics, and physiology bearing upon the extraordinary complex problems presented by the plant's carbohydrate metabolism, in which respiration plays a fundamental part. In the first place, in the light of modern developments, Spoehr attempts to formulate the main features of the complex system which constitutes the seat of metabolism in the living organism. Protoplasm can no longer be regarded as a complex compound of definite constitution entering into and supporting the processes of metabolism. Rather it is to be considered as a physical medium or substrate in the nature of a highly complex mixture having two main characteristics : (1) colloidal nature ; (2) relative chemical stability. Upon this relatively permanent sub-

strate metabolism proceeds, and may be viewed as a complex of interrelated chemical changes, the substances concerned being characterised by their instability.

In considering respiration and the carbohydrate metabolism of plants, the preconception as to the great stability of the carbohydrates as a class, a preconception founded on knowledge of them only as pure substances in simple solution, is erroneous. Recent chemical investigations have shown that monosaccharides, when associated with the salts of various metals in alkaline solution, undergo spontaneously an extraordinary series of complex and balanced changes leading to the appearance of a very large number of products which range up in complexity from CO_2 and formic acid. For example, Nef has found that in a system containing initially dextrose and sodium hydroxide no less than ninety-three different substances are finally in equilibrium. Concentration, oxidation potential, and temperature control the nature and proportion of the final products. In the presence of dilute alkali sugar is not decomposed, but the structural arrangement of the sugar molecules is affected. It is a striking and puzzling fact that but a small number of the thirty-two possible sugars which are now known as synthetic products are ever found in nature in the plant, and therefore it may be regarded as suggestive that the composition and proportion of the various sugars in equilibrium found in these experiments with dilute alkali closely approaches those found in nature. The explanation of the great instability and variability of reaction of the sugars lies in the fact that the sugars, as weak acids, form easily dissociated salts with metals and that the sugar ions are unstable and highly reactive. Dissociation may be a *conditio sine qua non* before oxidation can take place. The colloidal mixture forming the substrate or medium of sugar metabolism and the presence of a large variety of inorganic salts in the plant appear to constitute conditions under which we should expect a dissociation of the sugar molecule and a very large range of possible consequent reactions.

In the work which Spoehr himself has conducted he has elaborated methods for isolating the carbohydrates of cacti. During the cycle of the year cacti go through periods characterised by low water content of the plant and high water content, and by high and low temperatures associated with both the above conditions. From a study of the seasonal variations in the carbohydrates and from a study of the effect of water content and temperature under experimental conditions, it is found that—

(1) Lower water content and high temperatures are

associated with increase of polysaccharides, decrease of monosaccharides, and increase of pentosans.

(2) High water content and low temperature are associated with decrease of polysaccharides, increase of monosaccharides, and decrease of pentosans.

The rate of respiration was found not to be definitely controlled by any one group of sugars. The hexose monosaccharides which have been most commonly supposed to be closely concerned in respiration are at times only present in very small amounts, but the rate of respiration is not markedly reduced. It would seem, therefore, that under conditions of stress the plant utilises polysaccharides in respiration, and Spoehr thinks that the formation of pentosans is in connection with this condition. In the case of the polysaccharides the carbonyl group $-CH{:}O$ is not the first point of attack, but the sugars are first affected at the opposite end of the chain of carbon atoms, *i.e.* at the $-CH_2OH$ group. Such a reaction results in a primary formation of glucuronic acid, $CH{:}O(CHOH)_4{\cdot}COOH$, and this substance Spoehr has isolated definitely from the cactus. This is the first time that glucuronic acid has been reported as a plant constituent, though it has been found as a product of glucose metabolism in mammals. From glucuronic acid we should expect the formation of *l*-xylose, a pentose sugar, under the action of sunlight. In support of this conclusion of Spoehr's as to the origin of pentosan in the plant under conditions in which the plant utilises disaccharides or polysaccharides in respiration, it may be observed that if pentoses were derived from a direct oxidation of hexoses, *d*-glucose would yield *d*-arabinose and *d*-galactose give *d*-xylose, whereas in nature *d*-glucose is found with *l*-xylose and *d*-galactose with *l*-arabinose.

ZOOLOGY. By PROF. CHAS. H. O'DONOGHUE, D.Sc., F.Z.S., University of Manitoba, Winnipeg, Canada.

Protozoa.—Hegner gives an account of the "Heredity, Variation, and the Appearance of Diversities during the Vegetative Reproduction of *Arcella dentata*" (*Genetics*, vol. iv, March 1919). This records the results of a study of the breeding of the protozoon mentioned, and is concerned particularly with one largefamily of 5,557 individuals, direct descendants of one original specimen, and produced in the course of 69 generations of asexual reproduction. The species was chosen because it presented two characters—*i.e.*, the diameter of the shell and the number of spines on it—that are suitable for mathematical treatment,

and the problem investigated was " Can heritably diverse lines be recognised among the descendants of a single specimen of *Arcella dentata* produced by vegetative reproduction ? " There appears to be a certain correlation between the two factors chosen, since on the average those of larger diameter had more spines, and the limits of variation were, in diameter 73–150 microns, in the number of spines 7–17. The two main conclusions arrived at are : " A large family of *Arcella dentata*, therefore, derived from a single specimen by vegetative reproduction, consists of a number of branches that are hereditarily diverse with respect to diameter and spine number. These diverse branches resemble the hereditarily diverse families that were obtained by vegetative reproduction from different " wild " specimens. The formation of such hereditarily diverse branches appears to be a true case of evolution that has been observed in the laboratory, and that occurs in a similar way in nature.

Other papers include :

Flather, " The Effects of some Glandular Extracts upon the Contractile Vacuoles of *Paramæcium caudatum* " (*Biol. Bull.*, vol. xxxvii, July 1919) ; and Kasai and Kobayashi, " The Stomach Spirochæte occurring in Mammals " (*Journ. Parasit.*, vol. vi, Sept. 1919).

Invertebrata.—A continuation of the investigations on " The Axial Gradients in Hydrozoa " has been recorded by Child in " II, Susceptibility in Relation to Physiological Axes, Regions of the Colony and Stages of Development in Certain Hydroids " (*Biol. Bull.*, vol. xxxvii, Aug. 1919). The author finds a decreasing susceptibility to lethal concentrations of KNC and other salts on passing from the tentacles downwards, or from the apex of the colony towards the base. It is possible, however, to reverse this susceptibility under laboratory conditions, and it is found that these regions are more capable of acclimatisation to certain ranges of lower concentrations.

Other papers include :

Krecker, " Circulation of the Cœlomic Fluid in a Nematode " (*Biol. Bull.*, vol. xxxvii, Sept. 1919) ; Moore, " The Effect of Adrenin upon the Rate of Locomotion of Planaria and Toad Larvæ " (*ibid.*, Sept. 1919) ; Stunkard, " On the Specific Identity of *Heronimus chelydræ* MacCallum and *Aorchis extensus* Barker and Parsons " (*Journ. Parasit.*, vol. vi, Sept. 1919) ; Yoshida, " On the Migrating Course of Ascarid Larvæ in the Body of the Host " (*ibid.*) ; Ackert, " On the Life History of *Davania tetragona* (Molin), a Fowl Tapeworm " (*ibid.*) ; Guberlet, " On the Life History of the Chicken Cestode, *Hymenolepis carioca* (Magalhaes) " (*ibid.*) ; Nakagawa, " Further Notes on the Study of the Human Lung Distome, *Paragonimus westermani* " (*ibid.*) ; Goto, " *Dissotrema* Synonymous with *Gyliauchen* " (*ibid.*) ; Tannreuther, " Studies on the Rotifer *Asplanchna ebbesbornii*, with Special Reference to the

Male" (*Biol. Bull.*, vol. xxxvii, Sept. 1919); and Whitney, "The Ineffectiveness of Oxygen as a Factor in causing Male Production in *Hydatina senta*" (*Journ. Exp. Zool.*, vol. xxviii, July 1919).

Interesting results have been recorded by Gould in "Studies on Sex in the Hermaphrodite Mollusc *Crepidula plana*: III, Transference of the Male-producing Stimulus through Sea-water" (*Journ. Exp. Zool.*, vol. xxix, Aug. 1919). As is known, this animal exhibits a male phase, a transitional phase, and a female phase, during its life-history. The observations tend to show that well-grown specimens produce an unstable substance, diffusible in sea-water, that acts as a stimulus to young or smaller individuals, causing them to develop into full males. In the absence of this stimulus the male phase is suppressed. The action is apparently specific, for large *C. fornicata* do not seem to be able to call forth this response in a small *C. plana*.

Other papers include:

Copeland, "Locomotion in Two Species of the Gasteropod Genus *Alectrion*, with Observations on the Behaviour of Pedal Cilia" (*Biol. Bull.*, vol. xxxvii, Aug. 1919); and Guthrie, "Cleavage and Mesenchyme Formation in *Toxopneustes variegatus*" (*Biol. Bull.*, vol. xxxvii, Sept. 1919).

Bridges has added another to the already numerous papers on *Drosophila* in "Specific Modifiers of Eosin Eye-colour in *Drosophila melanogaster*" (*Journ. Exp. Zool.*, vol. xxviii, July 1919). In this it is claimed that eight mutant genes have been demonstrated which, while by themselves producing no effect, are yet capable of modifying the eye-colour in the sex-linked mutant "eosin." These modifiers can alter the eosin colour from shades darker than eosin on the one hand, to a pure white on the other. It is suggested that "The main significance of the facts presented is in their bearing on questions of the method by which selection attains its results." The chromosomes of insects, ever favourite objects of investigation, have been treated by Goldsmith, in "A Comparative Study of the Chromosomes of the Tiger Beetles (*Cicindelidæ*) (*Journ. Morph.*, vol. xxxii, Sept. 1919), and by Nakahara in "A Study of the Chromosomes in the Spermatogenesis of the Stonefly, *Perla immarginata*, Say, with Special Reference to the Question of Synapsis" (*ibid.*, Sept. 1919). The former shows that, according to previous workers, three types of chromosome behaviour may be recognised in beetles, one in which hetero-chromosomes are present, one in which a bipartite deeply staining body is found, and lastly one in which a single odd chromosome makes its appearance. It is noted that the germ cells of the female appear to contain about twice as much chromatin as those of the male. The various division stages are fully described and

figured. In the second paper it is shown that the chromosomes, including an X and Y, appear in the spermatogonial complex. The occurrence of Parasynapsis at the early stage of division is not substantiated.

Other papers include :

Brues, " The Classification of Insects on the Character of the Larva and Pupa " (*Biol. Bull.*, vol. xxxvii, July 1919) ; Burnes, " Experiments in Breeding as a Means of determining Some Relationships among Cyclops " (*ibid.*, July 1919) ; Davey, " Prolongation of life of *Tribolium confusum* apparently due to Small Doses of X-rays " (*Journ. Exp. Zool.*, vol. xxviii, July 1919) ; Green, " Studies in the Life-cycle of *Simnocephalus vetulus* " (*Biol. Bull.*, vol. xxxvii, July 1919) ; Kornhauser, " The Sexual Characteristics of the Membracid, *Thelia bimaculata* (Fabr.) : I, External Changes induced by *Aphelopus theliæ* (Gahan) " (*Journ. Morph.*, vol. xxxii, Sept. 1919) ; and Whiting, " Genetic Studies on the Mediterranean Flour Moth, *Ephestia kühniella* Zeller " (*Journ. Exp. Zool.*, vol. xxviii, July 1919).

Vertebrata.—" The Comparative Myology of the Shoulder Girdle and Pectoral Fin of Fishes " is discussed at some length by Shann (*Trans. Roy. Soc. Edin.*, vol. lii, pt. iii, 1919). A series of twenty species of the class ranging from Selachii to Teleostei were dissected, and a description of the musculature is given in each case. In addition to the actual account, there is also provided a " Key to Comparative Study of Pectoral Musculature " in the six different groups, and also in the Urodela, and again a tabular statement of the nomenclature employed by the various authorities in the Elasmobranchii, both of which are very useful.

Other papers include :

" The Homologies of the Maxillary and Vomer Bones of *Polypterus* " (*Amer. Journ. Anat.*, vol. xxv, July 1919), and " The Innervation of the Intermandibularis and Genioglossus Muscles of the Bony Fishes " (*Anat. Rec.*, vol. xvi, July 1919), both by Phelps ; Turner, " The Seasonal Cycle in the Spermary of the Perch " (*Journ. Morph.*, vol. xxxii, Sept. 1919) ; Allen, " Development of the Thyroid Glands of *Bufo* and their Normal Relation to Metamorphosis " (*ibid.*) ; Hoskins, " Growth and Development of Amphibia as affected by Thyroidectomy " (*Journ. Exp. Zool.*, vol. xxix, Aug. 1919) ; Jordan, " The Histology of the Blood and the Red Bone-Marrow of the Leopard Frog, *Rana pipiens* " (*Amer. Journ. Anat.*, vol. xxv, July 1919) ; Kampmeier, " A Summary of a Monograph on the Morphology of the Lymphatic System in the Anuran Amphibia, with Especial Reference to its Origin and Development " (*Anat. Rec.*, vol. xvi, Aug. 1919) ; Latta, " The Morphology of the so-called Balancers in Certain Species of *Amblystoma* " (*ibid.*, Sept. 1919) ; Larsell, " Studies on the Nervus Terminalis : Turtle " (*Journ. Comp. Neur.*, vol. xxx, Aug. 1919) ; and Tannreuther, " Partial and Complete Duplicity in Chick Embryos " (*Anat. Rec.*, vol. xvi, Aug. 1919).

Corner has written "On the Origin of the Corpus Luteum of the Sow from Both Granulosa and Theca Interna" (*Amer. Journ. Anat.*, vol. xxvi, Sept. 1919). He has investigated a full series of stages by means of various staining methods, and paid particular attention to the fate of the theca interna. There is no doubt in view of the evidence provided that the membrana granulosa is retained intact in the sow, in spite of the previous statements to the contrary, and is the main contributor to the glandular cells of the corpus luteum. The whole of the theca interna appears to pass into the corpus with the invading blood-vessels, but its cells are apparently not converted into fibroblasts. On the other hand, they increase by mitotic division, and are heavily laden with lipoid substance, so that it is probable that some of the theca cells persist throughout pregnancy as distinct elements of the corpus luteum. The exact fate of all of them cannot be learned by the present methods, because of a confusing resemblance between some of them and some of the modified granulosa cells. King has continued a series of "Studies on Inbreeding," in part "IV, A Further Study of the Effects of Inbreeding on the Growth and Variability in Body-weight of the Albino Rat" (*Journ. Exp. Zool.*, vol. xxix, Aug. 1919). Over 600 rats were examined in the sixteenth to the twenty-fifth generation, of a strain inbred brother and sister from the same litter only. It is noted that continued inbreeding has produced no noticeable deterioration in rate of growth or extent of growth. The normal body-weight was maintained throughout, and a high degree of variability kept up.

Other papers include :

Arey, "On the Presence of Haversian Systems in Membrane Bone" (*Anat. Rec.*, vol. xvii, Sept. 1919); Baldwin, "Variations in the Carotid Arteries of the Rabbit" (*ibid.*, July 1919); Buddington, "The Influence of Certain Ductless Gland Substances on the Growth of Plant Tissues" (*Biol. Bull.*, vol. xxxvii, Sept. 1919); Cyriax, "A Brief Note on 'Floating' Clavicle" (*Anat. Rec.*, vol. xvi, Aug. 1919); "The Development of the Sternum in Sus Scrofa" (*Anat. Rec.*, vol. xvi, Sept. 1919), and "The Ontogeny and Phylogeny of the Sternum" (*Amer. Journ. Anat.*, vol. xxvi, Sept. 1919), both by Hanson; Henderson, "The Adult Lymphatic System of the Striped Ground-squirrel (*Spermophilus tridecemlineatus* Mitchell)" (*Anat. Rec.*, vol. xvi, July 1919); Hunt, "Birth of Two Unequally Developed Cat Fœtuses (*Felis domestica*)" (*ibid.*, Aug. 1919); Jordan, "The Histology of the Umbilical Cord of the Pig, with Special Reference to the Vasculogenic and Hemopoietic Activity of its Extensively Vascularised Connective Tissue" (*Amer. Journ. Anat.*, vol. xxvi, Sept. 1919); Komine, "Metabolic Activity of the Nervous System: III, On the Amount of Non-protein Nitrogen in the Brain of Albino Rats during Twenty-four Hours after Feeding" (*Journ. Comp. Neur.*, vol. xxx, Aug. 1919); Kudo, "The Facial Musculature of the Japanese" (*Journ. Morph.*, vol. xxxii, Sept. 1919); MacArthur and Doisy, "Quantitative Changes in the

Human Brain during Growth" (*Journ. Comp. Neur.*, vol. xxx, Aug. 1919); Moore, "On the Physiological Properties of the Gonads as Controllers of Somatic and Psychical Characteristics: II, Growth of Gonadectomised Male and Female Rats" (*Journ. Exp. Zool.*, vol. xxviii, July 1919); Murray, "The Development of the Cardiac Loop in the Rabbit, with Especial Reference to the Duodo-ventricular Groove and Origin of the Interventricular Septum" (*Amer. Journ. Anat.*, vol. xxvi, Sept. 1919); Myers, "Studies on the Mammary Gland: IV, The Histology of the Mammary Gland in Male and Female Albino Rats from Birth to Ten Weeks of Age" (*ibid.*, July 1919); Plant, "Factors influencing the Behaviour of the Brain of the Albino Rat in Müller's Fluid" (*Journ. Comp. Neur.*, vol. xxx, Aug. 1919); and Takenouchi, "On the Resistance of the Red Corpuscles of Albino Rats at Different Ages to Hypotonic Solutions of Sodium Chloride" (*Anat. Rec.*, vol. xvi, Sept. 1919).

General.—Danforth has put forward "Evidence that the Germ Cells are Subject to Selection on the Basis of their Genetic Potentialities" (*Journ. Exp. Zool.*, vol. xxviii, July 1919). The method employed was to get fowls of known genetic constitution to inhale alcohol vapour, which in the lungs probably passes directly into the blood, and so may be carried to the germ cells. It was found that the relative proportion of certain traits, brachydactyly, polydactyly, and white colour in the offspring produced during periods of control and during periods of treatment differed. This selection was found to be more rigorous the more severe the treatment.

Other papers include:

Harvey, "Mitotic Division of Binucleate Cells" (*Biol. Bull.*, vol. xxxvii, Aug. 1919); Massopust, "A Simple Method of preparing Daylight Glass for Microscopic Work" (*Anat. Rec.*, vol. xvi, Aug. 1919); Sumner, "Adaptation and the Problem of Organic Purposefulness" (*Amer. Nat.*, vol. liii, July 1919); and Castle, "Piebald Rats and Selection" (*ibid.*).

EDUCATION. By A. E. Heath, M.A., University, Manchester.

Since the publication of Bain's *Education as a Science* there has been a great change in educational opinion. There used to be a not altogether unreasonable fear, which that work did little to dispel, that any stressing of the science—as opposed to the art—of education would tend to fetter tentative and pliant methods in the bonds of a rigid mechanical schematisation. But that fear was based on a false view of scientific method. Systematisation in any concrete field only proceeds side by side with the empirical garnering of experience; and thus knowledge of any field of endeavour (and of cognate domains) helps practice, not by presenting it with a set of *a priori* principles or rules from outside, but by turning the

attention of the practitioner to the rational, orderly study of successful methods in actual use, and of the underlying reasons for their success. In Prof. Laurie's pointed phrase, sound theory is only sound practice "conscious of itself." In our day none can justly accuse educationists of not being conscious of the problems raised by their practice, and of the need for an adequate basis of scientific knowledge about cognate domains. But the growth of such conscious attention to the science of education has led them to take a greater interest in the work of their fellows. Hence have arisen a succession of educational conferences, a growing professional literature, and a mass of technical periodicals with the sum total of which it is quite beyond the busy teacher to cope. It will be the object of this series, therefore, to help the expert more easily to make use of the literature of his subject, and to give to the general reader some impression of contemporary educational movements.

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Underlying the varied programme of the eighth annual Conference of Educational Associations held in January at University College, London, there seemed to run two main currents of thought.

A. Thus many of the papers group themselves naturally as being motivated by the awakened interest in the help which recent advances in pyschological knowledge can give to education. There were, for instance, admirable papers on "Mental Tests" by Prof. J. Adams and Dr. P. B. Ballard. In the discussion which followed the clear and useful history of the subject given by the latter, Prof. Spearman pointed out how easily the teacher who knows nothing of this type of work can mistake the stupidity due to some simple physical defect, or remediable failure in correlation, for a sign of low intelligence. Earlier in the conference Mr. G. F. Daniell had suggested that a certain amount of help might be afforded by psychological tests in the selection of elementary children for higher forms of education.

Other lecture-subjects grouped about a similar motive were those based on the growing trend of psychological opinion that the cognitive side of human personality has been unduly stressed, with the consequent relative neglect of its conative and affective aspects. Thus, in the first place we have the claim that scope should be given to the child's hunger for doing and for making things; and hence that a due place should be assigned to handicrafts and other creative activities not solely nor directly concerned with knowing. Under this heading we may class the set of lectures by Mr. Ben Greet and others on the educational uses of the drama; the paper by

Prof. Shelley on the play spirit; the claims for folk-dancing by Mr. Cecil Sharp; and the demonstration by M. Jaques Dalcroze by which he sought to illustrate his thesis that rhythmic bodily activity is an essential factor in the training of an intelligent appreciation of music, and a powerful weapon in forging for the child a "vigorous and clastic body." At the same time we have, closely connected with this, the correlative realisation that the affective or emotional side of the child needs its due meed of opportunity for development: a realisation which embodied itself in the lectures by Dr. Somervell, Mr. Barrett Carpenter, and others on the place of the various arts in education.

There is, moreover, a certain connection between the trend of thought sketched above and the claims which were voiced at the conference for a study of the child's unconscious mind. Mere unthoughtful repression, or neglect of any one side of the child's activity, can only—it is contended—lead to danger. The better way was well illustrated (perhaps unconsciously) in Mr. E. A. Craddock's exceedingly interesting paper on "The New Discipline." In this he showed how he had been able to convert the combative instincts, as exemplified in competitive and sporting impulses, from an enemy to discipline into an ally to self-governed class-work—even in so unpromising a subject as French irregular verbs. Bearing on these issues were also Dr. O. A. Wheeler's able address on "New Views of Human Personality," and Dr. Constance Long's sane and temperate plea for a proper consideration of the guidance and warnings afforded by the study of the unconscious for all those, whether parents or teachers, who are called on to give sex instruction.

B. The second main current along which the conference lectures seemed to align themselves was that concerned with the social significance of education. In his inaugural address the President of the Board of Education had spoken of the war against "our ignorance": and many of the lectures—those *e.g.* of the Eugenic Education Society, and the admirable course organised by the Infant Welfare Association and the Civic Education League—were delivered in full realisation of the dire consequences of that ignorance. Some other papers—amongst them being Prof. S. D. Adshead's on "Environment in relation to Civic Education," and Prof. J. J. Findlay's on "New Projects of Education and Social Reform"—dealt directly with the social questions involved in education. And, of the meetings on Continuation Schools, the discussion on "Education and Industry" opened by Mr. A. P. Fleming and Mr. J. M. Mactavish was as much concerned with economic principles as with education; whilst the joint conference on

the subject of such schools was largely occupied with the social factors involved in their formation.

The outstanding feature of this side of the conference was, however, the great stress laid on the need for adult education. The Master of Balliol's racy paper on "The Education of the Citizen," and the substance of what was said by the speakers at the joint conference on Adult Education under Canon J. H. B. Masterman, all served vigorously to press home upon the educational workers present the too often forgotten fact that one's education is only ended (if then) when one is dead.

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The January meetings of the Geographical, Science Masters', and Mathematical Associations dealt, in the main, with technical matters. Mr. W. W. Vaughan's presidential address to the science masters, however, took the form of a most inspiring call upon them to recognise their great responsibilities in the immediate future. Furthermore, the general question of the value to teachers of the history of their subject was touched upon both in Sir Richard Gregory's plea for general science courses separate from the laboratory work, and in Mr. R. C. Fawdry's paper to the Mathematical Association on the teaching of mechanics. Finally an important matter of principle was raised in the discussion on the paper read to the latter Association by Mr. C. Godfrey, in which it was contended that the informal stage of teaching should be extended over the whole of elementary plane geometry—use being made of the notions of symmetry and similarity—logical methods being reserved for a subsequent formal recapitulation. The danger here is not the informality itself, but that such courses may degenerate, in less expert hands than Mr. Godfrey's, into the acquirement of sense knowledge only, to the exclusion of all reasoning. But there is not this sharp alternation in the child's own development. "All the elementary mechanisms essential to formal reasoning are present before the child leaves the infant department, *i.e.* by the mental age of seven" is the conclusion of Mr. Burt in the papers referred to below. And that accounts for the practical failure of *purely* measurement courses as substitutes for Euclid. At any rate, Prof. T. P. Nunn's advice to strike the mean: to deal with the concrete (even solids), to take the obvious for granted; *but* to introduce rigid reasoning of a simple kind from the very first—would seem a desirable correlative to Mr. Godfrey's claim.

The following is a selection of references to recent work bearing on the subjects mentioned above:

E. Rignano, *Monist* (1918), 28, 3, pp. 373-93, in "The School of To-morrow," urges on all educationists the pressing need of the time for deliberate and conscious attention to the problems raised by schooling.

C. Burt, *Journ. of Exper. Pedagogy* (1919), **5**, 2, pp. 68–77; and **5**, 3, pp. 121–7. Two valuable papers on tests of reasoning and the light they throw on the development of reasoning in children. In the same place R. C. Moore gives a comparison of various methods of measuring intelligence. There is also an article by Mr. Burt in *Journ. of Ed.*, Jan. 1920, pp. 27–9.

In referring to educational applications of abnormal psychology it is perhaps well to state without equivocation that the applying of the knowledge gained in that study to teaching method is a very different matter from allowing oneself to be tempted into amateur psycho-analysis of children. That, on the part of anyone without a thorough medical and psychological training, is as unpardonable as to attempt an amateur surgical operation—and should carry the same legal penalties. There are two recent books. *The Child's Unconscious Mind* (Kegan Paul, 1919) by W. Lay is addressed to teachers; but some of the suggested applications run somewhat to crankiness. *Echo Personalities: A short Study of the Contributions of Abnormal Psychology towards the Problems of Normal Education* (George Allen and Unwin, 1918) by Frank Watts, presents, however, a balanced estimate of what can profitably be made use of in normal practice. See also Constance Long, *Journ. of Exp. Ped.* (1919), **4**, 2, pp. 57–70.

On the fundamental importance of æsthetic education, and on the real dangers in over-stressing it, by far the ablest modern discussion is contained in the chapter on "Education and Æsthetic" in Dr. M. W. Keatinge's *Studies in Education* (A. and C. Black, 1916), pp. 71–103.

The *Final Report of the Committee on Adult Education* (H.M. Stationery Office, 1919) contains a mass of material and suggestions of great importance to all educationists.

ARTICLES

THE RELATION OF MAGNETISM TO THE CRYSTALLINE STATE

By MAJOR A. E. OXLEY, R.A.F., M.A., D.Sc., University College, London.

Introduction.—In recent years important developments of the theory of atomic structure have been made, but it must be admitted that most of these provide a very imperfect means of correlating certain definite facts bearing on the magnetic properties of matter. To take one instance, the Rutherford-Bohr theory of the simpler types of atom ignores magnetic forces completely, and the Bohr hydrogen-molecule is definitely paramagnetic in contradiction to the well-known fact that molecular hydrogen is diamagnetic, possessing a molecular susceptibility -61×10^{-7}. While emphasising this point, it is to be understood that no attempt is here being made to show that an alternative theory based purely on an element of magnetism will give a more satisfactory solution; rather, that the known magnetic data imply that the more recent theories are inadequate, and require to be supplemented or modified, so that while retaining their present validity, as regards the interpretation of the phenomena which they were devised to explain, due recognition is taken of the available experimental data furnished by recent magnetic researches. It is therefore proposed to examine to what extent the magnetic forces due to orbital electron motions are capable of explaining magnetic phenomena, to regard these effects as superimposed upon the electrostatic effects, and, as far as possible, to ascertain whether in any particular case the electrostatic or electromagnetic forces predominate.

Experimental.—In a series of memoirs the author[1] has attacked this problem. The substances examined consisted of about 40 organic compounds whose magnetic properties were

[1] *Phil. Trans. Roy. Soc.*, Vol. 214A, p. 109, 1914 (Parts 1 and 2); *ibid.*, Vol. 215A, p. 79, 1915 (Part 3); *ibid.*, in course of publication (Part 4). For brevity these memoirs are referred to as Parts 1, 2, 3 and 4 in the text.

investigated over a temperature interval extending from —180° C. to 200° C. For details concerning the method of experiment and the elimination of errors, due to disturbing sources, reference is made to Part 1. In 1895, Curie investigated the magnetic properties of a large number of substances. He found that the specific susceptibility (i.e. the susceptibility per gram) of paramagnetic substances varied inversely as the absolute temperature, while that of diamagnetic substances (with the exception of bismuth) remained constant as the temperature varied. In 1905, Langevin developed an electron theory of magnetism for gases, which accounted theoretically for the results observed by Curie. On this theory, the sum of the magnetic moments of the electron orbits in a molecule is zero, when the substance is diamagnetic, but is different from zero in a paramagnetic substance. Weiss extended Langevin's theory to account for ferro-magnetism, where the molecules exert intense mutual action on one another. In the present experiments it was found that the Curie law of constancy of diamagnetism held very approximately for most of the substances, to within the limit of experimental error, providing no appreciable change of molecular aggregation with variation of temperature took place. A sudden change of molecular constitution, however, such as occurs on crystallisation, might be expected to affect the magnetic property appreciably, the molecules becoming distorted by mutual forces. In confirmation of this view, it was found that a definite change of specific susceptibility actually did take place on crystallisation, the change amounting to a few per cent. in the case of aromatic compounds. Aliphatic substances showed a smaller effect, and in some cases the change was inappreciable. The smallness of the effect presented by such substances is considered below (p. 592). If the substance did not crystallise, but passed into a supercooled liquid or jelly state, the change of specific susceptibility was too small to be detected. In the case of benzophenone, by cooling the liquid carefully, the substance passed into a vitreous mass with no change of susceptibility. After cooling to — 20° C. (i.e. 70 degrees below the m.p.), crystallisation set in and the crystals were found to be less diamagnetic than the liquid or vitreous mass. On heating the crystals, their susceptibility remained practically constant, until the fusion-point was reached, where the susceptibility increased to the normal value possessed by the liquid. Thus a definite hysteresis loop with respect to temperature was obtained with this diamagnetic substance. The loop was similar to those presented by nickel steels,[1] the critical tem-

[1] J. H. Hopkinson, *Proc. Roy. Soc.*, A, vol. xlviii, p. 1, 1890. Other curves than the one reproduced are given.

perature of the latter corresponding to the fusion-point of the organic compound. (See figs. 1A and 1B.)

As the result of these determinations, it appears that the diamagnetic property is complex and very similar to the magnetic property possessed by ferro-magnetic substances above and below their critical temperatures.

The essential difference between the two types of phenomena

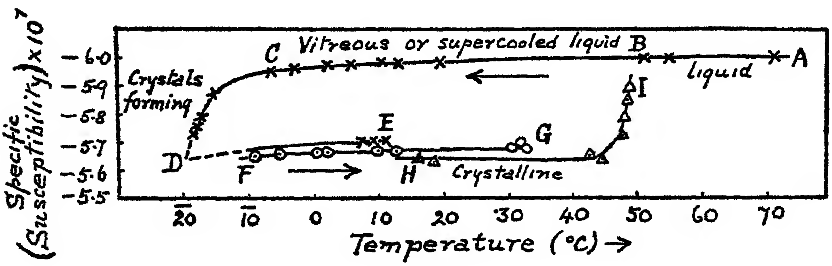

FIG. 1A.

Substance, benzophenone. ABC shows susceptibility on cooling from liquid to vitreous mass. Crystallisation started at C and was complete at D. DE, susceptibility curve for crystals on warming to room temperature. FG and HI, two independent curves, the latter showing the rise of susceptibility at the melting-point B.

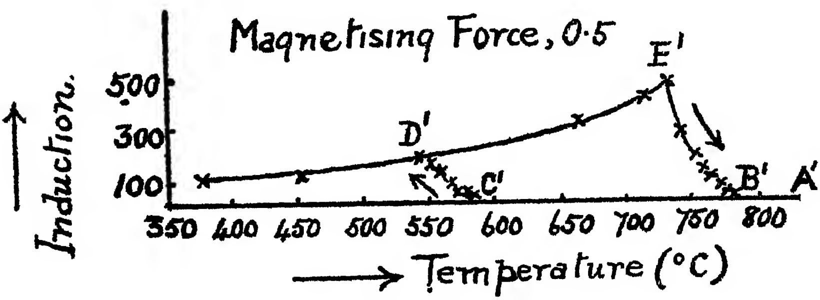

FIG. 1B.

Substance, nickel steel (4·7 per cent., Ni). A'B'C' shows very small induction on cooling from above 800° C., through magnetic critical temperature B', to about 600° C. Molecular change started at C', and was complete at D'. D'E', induction curve on heating to critical temperature, E'B' showing the fall of induction at the magnetic change-point.

lies in the nature of magnetic moment associated with an individual molecule, i.e. with the arrangement of the electron orbits on Langevin's theory. If the substance is diamagnetic, the molecule, taken as a whole, has a zero resultant moment; if the substance is paramagnetic or ferro-magnetic, the molecule has a definite resultant magnetic moment, and consequently the magnetic property varies in the latter case with temperature.

Now during crystallisation the rigidity which arises is due to the forces which the molecules, in virtue of the contained rotating electrons, exert on one another. These forces must be large in order to produce such a large cohesive effect. In spite of the zero magnetic moment of the diamagnetic molecule, taken as a whole, the local force, at a point in between a pair of molecules, may be very large, comparable in fact (see Part 2, p. 143) with the local force within a ferro-magnetic medium, and may be capable of distorting the molecules to such an extent as to give rise to the observed change of specific susceptibility on crystallisation.

The Local Molecular Field of a Diamagnetic Substance.—From the extent of this change of susceptibility per gram on crystallisation, it is possible to derive a value of the intensity of the local force, which is operative in the formation of a crystalline space lattice, and to which the rigidity is due. The usual method of defining the force at an internal point of a material medium is to take a cavity, whose dimensions are small in comparison with ordinary lengths (e.g. 1 cm.), and yet large compared with molecular dimensions. A convenient designation of the size of such a cavity is contained in the phrase "physically small." In molecular theory, the subdivision of the medium into elements is not valid beyond the limits of physical smallness, and only in media which are absolutely continuous may the elements be pushed to the limits of mathematical smallness. The case of a fluid composed of discrete molecules has been worked out by Sir Joseph Larmor, who found that the part contributed to the force at an internal point, by the molecules immediately surrounding the point, was separable into two parts, a purely local part and a part due to the rest of the medium. The latter part is determinate, as it is derivable from the potential due to the combined volume and surface density distributions of Poisson, while the former part, on account of rapid motions and irregular distributions of the molecular axes, is shown to be negligibly small. When, however, we come to the case of crystalline media, the molecules, which lie in the immediate neighbourhood of the internal point, contribute an unknown amount to the local force, for in this case there is no averaging out on account of random motions of the molecules immediately surrounding the point. As we do not know the law of force which holds at such close range, or the relative positions of the atoms in the crystalline medium, we can only deduce the value of the local forces indirectly from experimental data.

Up to the present we have made no supposition as to the nature of the local molecular force. It may be electrostatic, electromagnetic, or indeed of any other nature, for a force of

any of these types, associated with each molecule, would produce a distortion in all the molecules and would give rise to a new set of properties which would distinguish the crystalline from the fluid state. If we examined a single crystal we should no doubt find that the distortion, and therefore the properties, are different along different directions. In a mass of small crystals these directional differences would not be detectable, but there would still be an outstanding mean distortion which would produce a definite change of property on crystallisation. Now we know that the effect of applying a magnetic field to a substance is to distort the configuration of each molecule, and we can ascertain what must be the magnitude of such a field which will produce the change of susceptibility which was observed experimentally. Let us therefore *assume* that the molecular field is a magnetic one and determine its magnitude.

If ΔM is the change of moment produced in an electron orbit of moment M, by applying a magnetic field H, τ being the period of rotation of the electron ($\doteqdot 10^{-15}$ sec.), ϵ/m the ratio of charge to mass ($1{\cdot}7 \times 10^{7}$ in E.M.U.), we may write (Part 3, p. 84):

$$\frac{\Delta M}{M} = -\frac{H\tau\epsilon}{4\pi m} = -10^{-9} H.$$

The largest field which we can produce in the laboratory is of the order 10^4 gauss, and hence the largest value of ΔM is $10^{-5}.M$. Suppose, however, on crystallisation that the molecular forces were of the order 10^7 gauss, then the value of $\frac{\Delta M}{M}$ would be of the order 10^{-2}, and would correspond to a change of 1 per cent. in the specific susceptibility. This is the extent of the change actually observed in aromatic compounds, and therefore we *could* interpret the molecular force in such crystalline media as being of the order 10^7 gauss. Other evidence confirming a molecular field of this intensity is furnished by double refraction, magneto-striction, and magneto-rotation data, and this suggests an extension of the idea of an intense molecular field to media which show an inappreciable change of susceptibility on crystallisation. A liquid submitted to a magnetic field becomes feebly doubly refracting and undergoes a small volume change. If we could apply a magnetic field of 10^7 gauss (such a field is, on the above view, brought into play on crystallisation), the double refraction would be comparable with the natural double refraction of quartz (Part 3, p. 87), while the magneto-striction effect becomes the change of volume on crystallisation (of the order 0·1 c.c. per c.c.) (Part 4). The

large local molecular field can also be applied with success to account for the anomalous cases of the Faraday effect, the reversed action of the local molecular field producing in certain cases a change in the sign of the rotation (Part 4).

Our conception of a diamagnetic atom (or molecule) is represented diagrammatically in fig. 2. Taken as a unit, each atom (or molecule) A, B, etc., is a balanced system possessing zero magnetic moment. Of course A and B may each possess a large number of electrons rotating in small orbits scattered over the "surface" and interior (see fig. 3, p. 597) which would determine forces in other directions than those indicated by the straight arrows in fig. 2, but as all the electrons in a diamagnetic molecule must have a total zero moment, the above

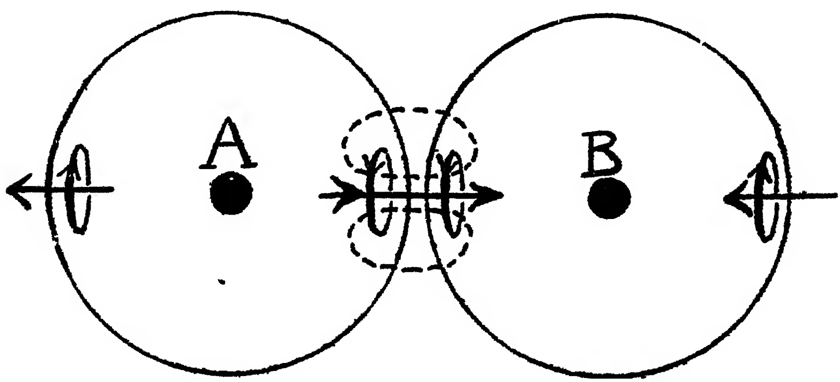

FIG. 2.

Diagrammatic representation of the electromagnetic attractive force between two units, A and B, of the crystalline structure. The black dots indicate the diffuse nuclei, consisting of positive charges and a distribution of electrons rotating in small circles at varying distances. The circular currents shown are positive; the actual direction of rotation of the electron is opposite to that indicated by the arrow on each small circle.

diagrammatic scheme fulfils our purpose. The general case of an asymmetric diamagnetic atom, whose localised surface electrons may have a close bearing on directed valencies, is referred to below (p. 597). In the crystalline structure, A and B are attracted to one another and to the surrounding atoms or molecules, *locally*, as indicated by the arrows between the attracting orbits, and it will be observed that the local molecular field is of an alternating character, the distance over which it is unidirectional being comparable with the distance between the atoms or molecules. We should naturally expect, on this view, that the molecular field in between the atoms of a diamagnetic medium would be comparable with that in ferro-magnetic media, since the periods of the electron orbits and the number of electrons are comparable in the two cases.

The value, 10^7 gauss, of the local molecular field found above, is of the same order as that in ferro-magnetic substances (Weiss & Beck, *Journ. de Phys.* IV, vol. vii, p. 249, 1908). Locally, the diamagnetic molecule is ferro-magnetic, and the repulsion observed in a magnetic field, on diamagnetic matter in bulk, is purely an induction effect produced in a system of molecules each of which has a zero magnetic moment.

Energy of the Molecular Field.—If H_e be the intensity of the local molecular field in a diamagnetic crystalline medium, I the aggregate *local* intensity of magnetisation for all the molecules in unit volume, the energy associated with one gram of the medium, over and above that associated with one gram of the liquid, will be $\frac{1}{2\rho} \cdot H_e \cdot I$, ρ being the density of the crystalline medium.[1] If we assume that H_e is proportional to I, and write $H_e = A_e \cdot I$, the energy term becomes $\frac{1}{2\rho} \cdot A_e \cdot I^2$, which is analogous to the energy term $\frac{1}{2\rho} \cdot N \cdot I^2$. obtained by Weiss for ferro-magnetic media. N, which corresponds to A_e, is the coefficient of the ferro-magnetic molecular field. The local molecular fields in the two cases are comparable, and so are the values of the local intensity of magnetisation; hence the energies, associated with one gram of the crystalline media in virtue of the crystalline grouping of the molecules, are of the same order. This energy is approximately 10^9 ergs. During fusion this molecular potential energy is dissipated, and therefor the latent heat of fusion should be of the order $10^9/4{\cdot}2 \times 10^7$ or 25 gram calories. The following are values[2] of the latent heat for some of the substances experimented on in Part 1:

Benzene, 30	Chlorobenzene, 30	Aniline, 21
Xylene, 39	Bromobenzene, 30	Acetophenone, 23
Pyridine, 22	Nitrobenzene, 22	Naphthalene, 35

It should be noted that these are comparable with the values for the elements:

Bismuth, 13	Cadmium, 14	Silver, 22
Zinc, 28	Gallium, 19	Iron (magnetic change-point), 59

Of course, until we know the disposition of the molecules in the space lattice, their distances apart, and the law of force which

[1] For details of the argument leading up to the derivation of this term, see Part 3, p. 90.

[2] *Recueil des Constantes Physiques*, Paris, 1913, p. 323.

is operative within molecular range, it will be impossible to obtain individual values characteristic for each substance. The molecules are held in certain mean positions by the operation of the local molecular field, and at any temperature below the fusion-point, the thermal energy consists partly of translational and partly of rotational oscillations. At the fusion-point, the molecular field ceases to have any directing effect on the molecules, and the amplitude of the rotational oscillations has grown so great that the crystalline arrangement has been destroyed. This theory has obviously a close bearing on the theory of specific heats of the solid state, and suggests a connection between the latter and the elastic properties of the medium (*cf.* Debye's theory). Superimposed upon the thermal energy of linear vibrations of the molecules we shall have a rotational term:

$$\frac{1}{\rho} \cdot A_e \cdot I \cdot \frac{I}{\delta\theta} \cdot$$

at temperature θ, and the dissipation of the energy of crystalline grouping will give rise just below the fusion-point (or, as Weiss has pointed out, in the case of ferro-magnetics, just below the magnetic critical point) to a small but measurable increment of specific heat.

Experimentally, such effects have been observed by Nernst and Lindemann.

Again, the relation between the optical and thermal frequencies and the elastic constants of the medium follows at once, since the molecular field, which holds the molecules together and determines the rigidity of the medium, is sufficiently strong to cause the electrons to vibrate with optical frequencies ν, in accordance with the relation $\nu = \frac{H\epsilon}{2\pi m}$. Within an atom the local field may be greater than 10^7 gauss; if it attains a value of 10^9 gauss, oscillations of X-ray frequencies could be accounted for. The intermolecular field of 10^7 gauss may account for the shift of an absorption band on crystallisation in accordance with the theory of the Zeeman effect.

Molecular Cohesion in Crystals.—We have seen that the energy per unit volume of a crystalline medium is of the order 2×10^9 ergs (Part 4). This gives a measure of the ultimate tensile strength of the medium. It should be noted that in determining the magnetic induction, which gives rise to this mechanical stress, we must imagine *a crevasse, whose gap is small in the mathematical sense, situated between a pair of molecules.* The true local molecular field would then be determined, whether the medium as a whole is diamagnetic or ferro-

magnetic, its order of magnitude being 10^7 gauss. If we took a crevasse in a ferro-magnetic crystal whose width of gap was large compared with molecular dimensions, the force in the gap, when the external field is zero, would be $4\pi I$, where I is the spontaneous intensity of magnetisation. This is small compared with the local value of the molecular field and is totally inadequate to account for the ultimate tensile strength. The difference is attributed to the localisation of the molecular field, and, while the principle of continuity of magnetic induction is obeyed, the intense local field is still capable of modifying the structure of an immediately neighbouring molecule and of setting up an internal stress equal to the ultimate tensile strength of the material as observed mechanically. If we take an isolated crystal, the stresses between the layers of atoms will be different in different directions, and hence the effort required to separate the layers of molecules or atoms will be different according to the directions chosen. In this way, the positions of the planes of cleavage are found. On this view, the crystalline symmetry is characteristic of the residual valency forces due to the nucleus and nuclear electrons, the symmetry of which is reflected in the crystalline symmetry. It is necessary to point out here that this statement is not inconsistent with the atomic space lattice disclosed by X-ray analysis. The diffracting centres are very small, diffuse regions surrounding the nucleus, and containing numbers of nuclear electrons. The true valency electrons are roaming over the atomic "surface," and are in general small in number compared with the nuclear electrons. As the diffracting power is probably proportional to the number of electrons, we should expect that the photographic effect, observed in X-ray analysis, would be characteristic of the nuclear group and that the two valency electrons which determine the special relation of two different atoms would not be discoverable by such means. Therefore it is regarded as probable that the molecule, at least in compounds, still exists as a definite unit within the crystalline structure (fig. 3). Furthermore, in the liquid state, the atoms of a compound molecule bear a special relation to one another, determined by definite valencies, and it is not likely that, when crystallisation sets in, the *residual* forces of two such stable systems will be capable of controlling the specially strong valency forces in such a way that a given atom becomes associated *equally with all* its neighbours. Unless we adopt this view, and regard certain atoms as specially bound together to form a molecule, just as they do in the liquid state, we must admit that a single valency can be subdivided, for, on the alternative view, each atom is related to its neighbour in precisely the same way.

In a remarkable series of experiments, Tyndall (*On Diamagnetism and Magneto-crystallic Action*, 1870) has shown that the deportment of crystals suspended in a magnetic field is closely linked up with the orientations of the principal planes

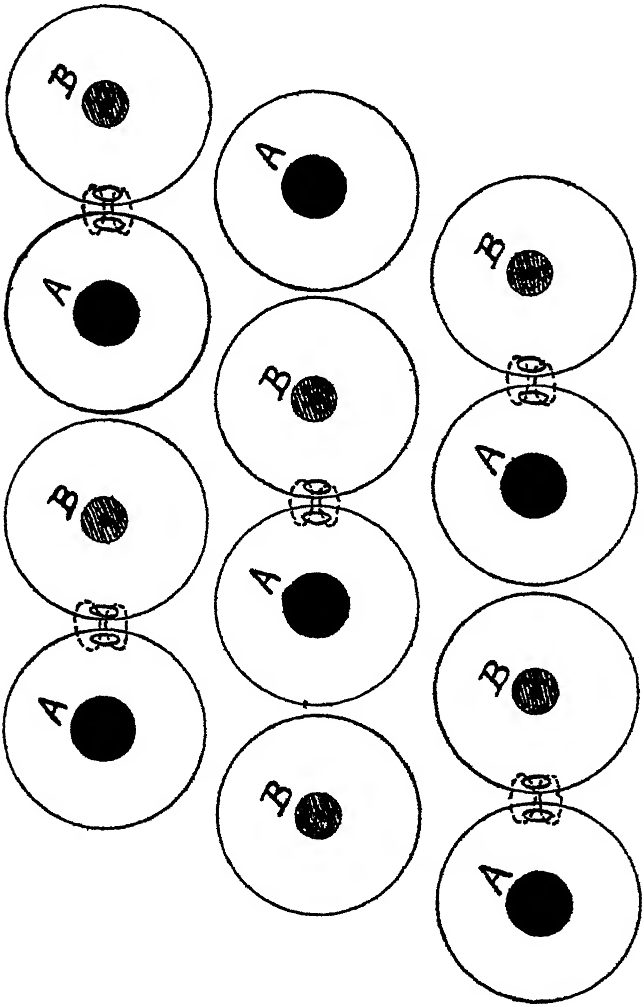

FIG. 3.

Diagrammatic representation of space lattice containing two types of atoms A and B. Space lattice determined by residual forces of nuclear electrons and nuclei. Dotted lines indicate localised regions where true valency electrons operate. Valencies of A and B are small integral numbers, and determine how the A's and B's form molecules within the lattice as they would in the liquid. The valency electrons may be regularly (as shown) or chaotically distributed through the space lattice, the symmetry of the latter being due to relatively large number of electrons in nuclei and to the nuclei themselves.

of cleavage. Tyndall sought to explain these effects as due to reciprocal molecular induction, an explanation plausible enough in the case of ferro-magnetic media, but hardly likely to be applicable in the case of diamagnetic media as they were then understood. The simple diamagnetic polarity is of so

small an order that the differential induction effect along different directions within the crystal could certainly not give rise to the mechanical movements observed. On our view of the structure of a diamagnetic molecule, which we have seen is strongly magnetic locally, but as a whole diamagnetic, this difficulty disappears. The local force is large enough to explain the deportment of the crystal in a magnetic field and also to produce the molecular distortion which accompanies the transition from the liquid to the crystalline state.

Tyndall examined more than one hundred different crystals. The importance of his results can best be judged by quoting one or two examples. He remarks (p. 24): " If a prism of sulphate of magnesia be suspended between the poles with its axis horizontal, on exciting the magnet, the axis will take up the equatorial position. This is not entirely due to the form of the crystal; for even when its axial dimension is shortest, the axis will assert the equatorial position; thus behaving like a magnetic body, setting its longest dimension from pole to pole.

" Suspended from its end with its axis vertical, the prism will take up a determinate oblique position. When the crystal has come to rest, let that line through the mass which stands exactly equatorial be carefully marked. Lay a knife-edge along this line, and press it in the direction of the axis. The crystal will split before the pressure, disclosing shining surfaces of cleavage. This is the only cleavage the crystal possesses, and it stands equatorial.

" Sulphate of zinc is of the same form as sulphate of magnesia, and its cleavage is discoverable by a process exactly similar to that just described. Both crystals set their planes of cleavage equatorial. Both are diamagnetic.

" Let us now examine a magnetic crystal of similar form. Sulphate of nickel is, perhaps, as good an example as we can choose. Suspended in the magnetic field with its axis horizontal, on exciting the magnet the axis will set itself from pole to pole; and this position will be persisted in, even when the axial dimension is shortest. Suspended from its end, the crystalline prism will take up an oblique position with considerable energy. When the crystal thus suspended has come to rest, mark the line along its end which stands *axial*. Let a knife-edge be laid along this line, and pressed in a direction parallel to the axis of the prism. The crystal will yield before the edge, and discover a perfectly clean plane of cleavage.

" These facts are suggestive. The crystals here experimented with are of the same outward form; each has but one cleavage; and the position of this cleavage, with regard to the form of the crystal, is the same in all. The magnetic

force, however, at once discovers a difference of action. *The cleavages of the diamagnetic specimens stand equatorial; of the magnetic, axial.*

Again, " a cube cut from a prism of scapolite, the axis of the prism being perpendicular to two of the parallel faces of the cube, suspended in the magnetic field, sets itself with the axis of the prism from pole to pole.

" A cube of beryl, of the same dimensions, with the axis of the prism from which it was taken also perpendicular to two of the faces, suspended as in the former case, sets itself with the axis equatorial. Both these crystals are magnetic.

" The former experiments showed a dissimilarity of action between magnetic and diamagnetic crystals. In the present instances both are magnetic, but still there is a difference; the axis of the one prism stands axial, the axis of the other equatorial. With regard to the explanation of this, the following fact is significant. Scapolite cleaves *parallel* to its axis, while beryl cleaves *perpendicular* to its axis; the cleavages in both cases, therefore, stand axial, thus agreeing with sulphate of nickel."

The above quotation clearly shows how closely allied are the forces of cohesion in different directions, with the magnetic behaviour of the crystalline medium, and lead us to suspect that the forces of crystallisation are probably of a magnetic nature. The fine points are so completely explained by the magnetic deportment that it is difficult to dissociate the cohesive forces from a magnetic origin. If the cohesive forces had an electrostatic origin, an electrostatic field would only disclose the above information regarding the orientation of the planes of cleavage providing the magnetic and electrostatic axes of the molecules coincided. This we know is not the case. Again, A. W. Porter and D. K. Morris (*Proc. Roy. Soc.*, A, vol. lvii, p. 469, 1895) have shown that in paraffin, dielectric hysteresis (as distinct from viscosity), under the influence of a powerful electrostatic field, does not exist to within one part in fifty thousand. If the molecules of the dielectric were held together by electrostatic forces, we should expect that here, if in any substance, the molecular groups would be reorganised by the applied field, the process being accompanied by hysteresis. In the corresponding cases of ferro-magnetic substances, under the influence of a magnetic field, considerable hysteresis effects are observed. In diamagnetic media, we could hardly hope to observe any appreciable effect, since the molecule has initially a zero magnetic moment and only orientates an account of its dissymmetry, after the small diamagnetic moment has been induced, thus giving rise to a minute magnetic double refraction. (Langevin,

Le Radium, vol. vii, p. 251, 1910. Cotton and Mouton, *Ann. de Chim. et de Phys.*, 8, vol. xix, p. 155, 1910.)

Atomic Magnetism.—The additive nature of the diamagnetic property will now be considered. Pascal (*Ann. de Chim. et de Phys.*, vol. viii, p. 19, 1910) has shown that in the case of a large variety of organic compounds, the sum of the atomic susceptibilities of the component atoms is equal to the molecular susceptibility of the compound, providing the compound does not contain any one of certain types of peculiarity such as the benzene and naphthalene nuclei, an ethylene linkage, etc. In any case, if the atomic susceptibility [1] of a component element be χ_a, the molecular susceptibility of the compound χ_m, then

$$\chi_m = \Sigma\chi_a + \lambda$$

where λ is a constant, for a given series of compounds, having a definite peculiarity of structure.

The atomic susceptibility of hydrogen is $-30{\cdot}5 \times 10^{-7}$, that of carbon $-59{\cdot}5 \times 10^{-7}$. It is not a little remarkable that the differences of molecular susceptibilities of a series of hydrocarbons, whose constitutions differ by the group CH_2, is always very nearly -120×10^{-7}, *i.e.* the sum of the susceptibilities of one carbon and two hydrogen atoms in the elementary state. It appears that the addition of one or more atoms, or of a group of atoms, adds a definite amount of diamagnetism to the compound, at least in so far as the hydrocarbons are concerned. Complications are found, however, in compounds containing oxygen, nitrogen, etc., which behave differently according to the positions of such atoms in the molecule, but even in these cases the effect of carbon, hydrogen or CH_2, is as described above. The author has shown (*Proc. Roy. Soc.*, A, vol. xcv, p. 58, 1918) that if the electron in the hydrogen atom rotate with a frequency corresponding to the line H_a, then the magnetic moment of the orbit of radius 10^{-8} cms., which gives an atomic susceptibility of $-30{\cdot}5 \times 10^{-7}$, is equal to $16{\cdot}3 \times 10^{-22}$, which is nearly equal to the moment of the magneton,[2] as derived from ferro-magnetic considerations. In the light of these results it seems that in considering the atomic constitution of the molecule due regard must be paid to magnetic effects. (See also a paper by Sir J. J. Thomson, *Phil. Mag.*, April 1919.)

[1] Atomic susceptibility = susceptibility per gram of element × its atomic weight.
Molecular susceptibility = susceptibility per gram of compound × its molecular weight.

[2] The magneton is an experimentally derived unit of magnetism, obtained by Weiss, from measurements on the saturation intensities of magnetisation of a number of ferro-magnetic elements and alloys.

In many cases, there is no semblance of an additive law, as is well shown by the metallic oxides. Again, iron and nickel are very magnetic, yet iron carbonyl ($Fe(CO)_5$) and nickel carbonyl ($Ni(CO)_4$) are diamagnetic.[1] No explanation of such effects is at present in sight, unless, perhaps, the effect of chemical combination causes in special cases a redistribution of electron orbits, or possibly even an electron transfer, so that the molecule as a whole acquires a zero magnetic moment. Such does not appear to be the case, however, in the non-conducting hydrocarbons, and here, probably, the local magnetic force plays a predominant part (Part 4).

Certain extensions of this work have been made, dealing with the origin of spectral series and optical activity, the latter being dependent upon right- and left-handed electron revolutions. Lack of space prevents more than a mere reference to these effects in the present article.

Conclusion.—The satisfactory explanation of many difficult points met with in stereo-chemistry and in connection with the periodic law appears to be possible only by giving due consideration to both the magnetic and electrostatic forces; the former predominating in some cases, the latter in others. (See also A. L. Parson, *Smithsonian Miscell. Collections*, vol. lxv, p. 1, 1915, and I. Langmuir, *Journ. American Chemical Society*, vol. xli, p. 868, 1919.) In any case it is clear that the magnetic forces cannot be ignored, even when the electrostatic forces do predominate, and the complete theory has yet to appear which shall give to these two types of force the relative prominence which the experimental data demand. The arguments which have been advanced show that the complete theory must (1) account for the molecular structure of crystals and the preservation of indivisable valencies as they are known to the chemist; (2) explain the close connection which exists between orientations of the planes of cleavage and the deportment of crystals in a magnetic field; (3) be consistent with a spacial distribution of directed valencies, such as are required by stereochemistry; (4) account for the additive nature of the diamagnetic property in non-conducting compounds; (5) be consistent with the absence of dielectric hysteresis.

[1] A. E. Oxley, *Proc. Cam. Phil. Soc.*, vol. xvi, p. 102.

THE DISTRIBUTION OF CHEMICAL ELEMENTS

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Science has reached another milestone on its road toward an understanding of nature. This mark is the completion or near completion of the system of matter as expressed in the periodic law of the chemical elements. Perhaps no other natural law, save some astronomical laws, has been so fully established. From its early conception about fifty years ago by Newlands,[1] Mendeleeff,[2] and Lothar Meyer,[3] it has had various fortunes, enthusiastically supported by some, and despised by others[4] on account of its fragmentary nature. There have been three great discoveries of recent decades which have helped to perfect the system and eliminate the many gaps appearing in the original table.

The discovery of the rare gases of the atmosphere[5] revealed a group of elements which are entirely inert and enter into no chemical combination. After several years of discussion they were finally placed into a zero-group of the periodic system.[6] In this position they revealed themselves as a missing link between a strongly negative halogen and a strongly positive alkalimetal, for their electro potential is assumed to be $\pm \infty$. Thus a continuous line or series of the elements was formed when they were arranged according to increasing atomic weight.

The discovery of the radio-active elements[7] established

[1] *Chem. News*, 1864, vol. x, pp. 59, 94; 1866, vol. xiii, pp. 113, 130.

[2] *Journ. Russ. Chem. Ges.*, 1869, vol. i, pp. 60, 229; Ber. ii, p. 553, 1869.

[3] *Ann. Chem. Pharm.*, 1870, vol. vii, p. 354.

[4] *E.g.* Ostwald, *Lehrbuch*, 2 Aufl., Bd. 1, pp. 137 and 1117; Wyrouboff, *Chem. News*, 1896, vol. lxxiv, p. 31; Wilde, *Compt. Rend.*, 1898, vol. cxxvii, p. 613.

[5] Ramsay, Rayleigh, Travers, Cleve, and others.

[6] Thomson, *Zeit. Anorg. Chem.*, 1895, vol. ix, pp. 190, 283; Deeley, *Chem. News*, 1895, vol. lxxii, p. 297; Wilde, *Chem. News*, 1895, vol. lxxii, p. 317; 1896, vol. lxxiii, p. 35; Sperber, *Zeit. Anorg. Chem.*, 1897, vol. xiv, p. 164; *Chem. News*, vol. lxxvii, p. 87; Ramsay, Ber., 1898, vol. xxxi, p. 3111; Crookes, *Zeit. Anorg. Chem.*, 1898, vol. xviii, p. 72; Howe, *Chem. News*, 1899, vol. lxxx, p. 74.

[7] Curie, Schmidt, Debierne, Rutherford, Soddy, Marckwald, Fajans.

a number of elements of high atomic weight which disintegrate or break apart, and thus led to the conclusion that, under present terrestrial conditions, elements of higher atomic weight cannot exist. Therefore the radio-active elements indicate the upper limit of the periodic system.

The continuity and upper limit of the system of matter thus revealed, it remained, before the system became complete, to determine how many individual members there are, and how many of the gaps may be due to undiscovered elements. This problem was solved by the discovery of the high-frequency or X-ray spectra, which permitted the assignment of atomic numbers to the chemical elements,[1] and thus indicated that between hydrogen (atomic number 1) and uranium (atomic number 92) only five elements—namely, those of atomic numbers 43, 61, 75, 85, and 87—have not yet been found.[2]

With this gradual completion of the periodic system there came the realisation, based upon a study of the properties of the elements, that the periods are not of equal length, but become *longer*.[3] This fact furnished further evidence for the completeness of the system, for it has been shown that no more than six noble gases, from helium to niton inclusive, are possible, and that therefore all the elements of electro potential $\pm \propto$ are known, and can serve as the terminals of the periods.[4]

The scientific epoch which comes to a close by the recognition of the system of matter, showing that there are ninety-two different types of atoms, interlaps the new epoch which searches for means of determining the constitution and the structure of these ninety-two types of atoms. No matter what the results of this inquiry, the term "element" for the building-stones of the universe will remain, modified, however, by the conception of subatomic structure, and perhaps the assumption of a single "protyle" or "urstoff." The writer may be pardoned if he attempts at this time to take an inventory of the known distribution of these "bricks"—the chemical elements—followed by some thoughts concerning the practical use of such knowledge.

The painstaking and excellent work of F. W. Clarke[5] has

[1] Moseley, *Phil. Mag.*, 1913, vol. xxvi, pp. 1924 ff.; de Broglie, *Compt. Rend.*, 1914, vol. clix, pp. 304 ff.; Higgs, *Phil. Mag.*, 1914, vol. xxviii, p. 139; Rydberg, *Phil. Mag.*, 1914, vol. xxviii, p. 144; Siegbahn and Friemann, *Phil. Mag.*, 1916, vol. xxxi, pp. 403 ff.

[2] *Astrophys. Journ.*, 1918, vol. xlviii, p. 241; *Amer. Journ. Pharm.*, 1918, vol. xc, p. 478.

[3] *E.g.* Batschinsky, *Zeit. Phys. Chem.*, 1893, vol. xliii, p. 372; Werner, *Ber.*, 1905, vol. xxxviii, p. 914; Adams, *Journ. Amer. Chem. Soc.*, 1911, vol. xxxiii, p. 648; Hackh, *Weltwissen*, 1915, vol. iii, p. 63; Harkins, *Journ. Amer. Chem. Soc.*, 1916, vol. xxxviii, p. 169.

[4] *Amer. Journ. Science*, 1918, vol. xlvi, p. 481.

[5] Clarke, "Data of Geochemistry," *U.S. Geolog. Survey*, Bull. 616.

enriched chemical literature with a valuable table of the estimated distribution of the chemical elements upon the known crust of the earth—that is, the lithosphere, or solid crust extending to a depth of ten miles; the hydrosphere, which includes the oceans and lakes; and the atmosphere, which embraces the gaseous envelope. Clarke's table is given in percentage of weight, the figures therefore simply state, *e.g.*, that oxygen forms about 50 per cent., and hydrogen 0·9 per cent., of the weight of the known terrestrial matter, and they therefore contain just as much information as the statement that water contains 88·89 per cent. of oxygen and 11·11 per cent. of hydrogen. To arrive at the chemical meaning of these numbers—that is, the proportions of the atoms concerned—these values must be divided by the atomic weight. Thus if m is the percentage or mass, and A the atomic weight, then $m/A = x$, where x is the relative proportion of the atoms. Taking the most abundant twenty-five elements, it was found that the value x for Bromine was the lowest, namely, ·000125; and using this arbitrarily as unity, the values for the relative abundance a of the atoms were calculated by the simple equation $x/{\cdot}000125 = a$. These new values give a further insight into the distribution of the chemical elements. They are, arranged as to their magnitude, as follows:

O	249,850	Mg	6,835	Ti	714	N	171	V	23
H	75,312	Ca	6,422	Cl	451	Mn	116	Li	22
Si	72,860	Fe	5,982	Fl	421	Ba	46	Sr	16
Al	21,528	K	4,660	P	283	Cr	46	Zr	13
Na	8,200	C	1,199	S	274	Ni	26	Br	1

These values of the relative abundance of the atoms indicate, *e.g.*, that for each bromine atom there are about 250,000 oxygen atoms and 75,000 hydrogen atoms, while, *e.g.*, for each carbon atom there are about 250 oxygen atoms and 75 hydrogen atoms. A comparison with Clarke's table of percentages reveals the most striking fact that hydrogen advances from the ninth to the second place, making the proportion of O:H as 10:3. This interpretation of the distribution seems to give a better view of the importance of the elements.

It is significant that *all* the important elements have a low atomic weight, for they all fall between the atomic numbers 1 to 28, or atomic weight 1 to 58,6. Furthermore, the same elements all enter into life, with the exception of titanium (which so far has not been reported as being present in the living organism). For the elements of the biosphere quantitative data are meagre; it is possible, however, to select two types for which the ultimate composition has been calculated—namely, man as representative of the mammals,[1] and the long-leaf pine[2]

[1] Hackh, *Journ. Gen. Physiology*, 1919, vol. i, p. 429.
[2] Little, *Journ. Ind. Eng. Chem.*, 1916, vol. viii, p. 102,

as representative of the gymnospermous plants. The presence of the elements in the respective spheres, together with the qualitative composition of meteorites,[1] is illustrated by the figure, which is based upon the modified periodic table.[2] It will be noticed that all the important elements occupy neighbouring positions in the periodic system, and that the few important elements of higher atomic weight are near the group of the inert noble gases. Most characteristic is the fact that there are only *four* elements (H, O, N, C) which are in *all* spheres. These are the well-known bio-elements, which form from 97–99 per cent. of all living matter. The possibility of millions of different compounds of these elements, together with the ever-growing complexity of organic compounds revealed by modern investigations, has led to the development of a system of structure symbols [3] wholly based upon these four elements. A similar quartet of elements (Na, Mg, Al, Si) is found on the other side of the noble gases; these are third to sixth in order of abundance, for their oxides form the basis of all the rock-forming compounds of the solid earth crust.[4]

These eight elements form a prominent centre around which the elements next in importance are closely grouped. Six more elements—namely, Fe, Ca, K, Cl, P, and Ni—are necessary to bring the percentage of distribution in all four spheres, including the meteorites, close to or above 99 per cent. By substituting S for Ni, the list will contain the fourteen fundamental elements which not only make the bulk of the known matter, but also are the essential elements of every material science, whether biology or geology, pharmacy or metallurgy.

What is the significance of these data? Does it point to a material evolution in the universe? An evolution more fascinating by its apparent universality, for our earth, being not isolated, but an integral although insignificant part of the cosmos, must partake in cosmical evolution. Is the abundance of elements the earmark of a state of evolution? As there are in celestial objects indications of a stellar evolution, whose stages are recognised by the spectra revealing the abundance

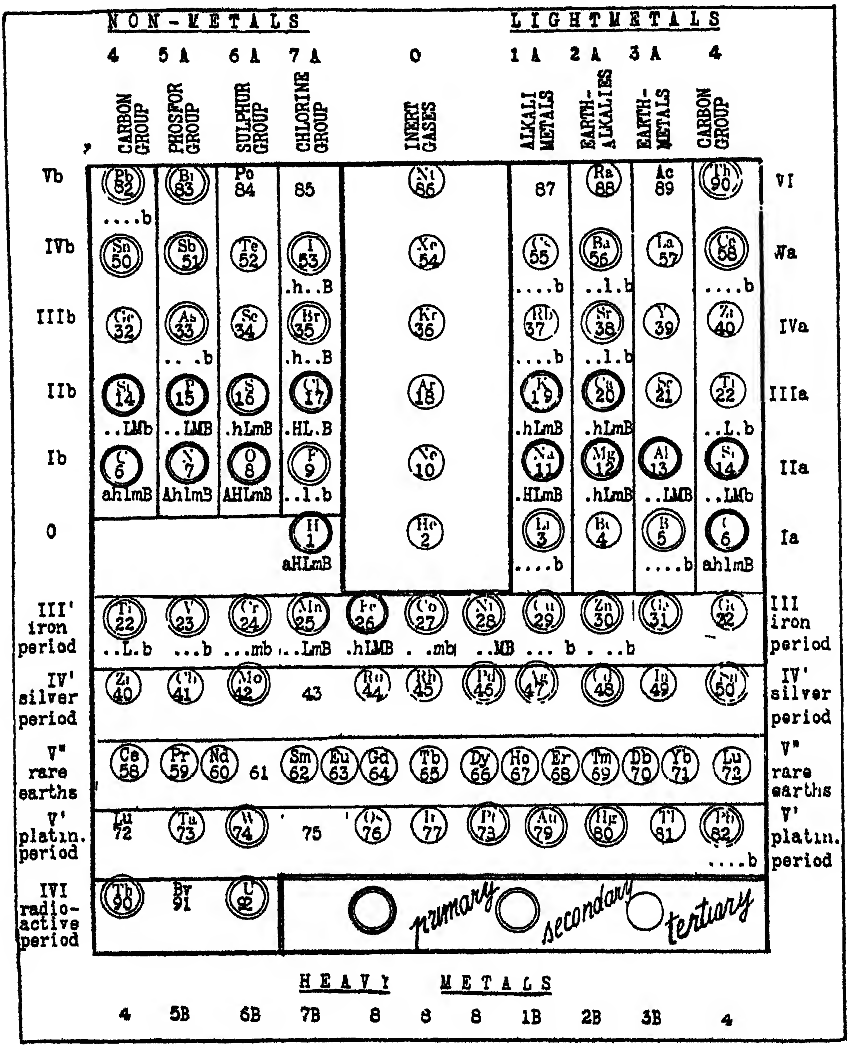
[1] Farrington, *Field Columbian Museum, Chicago*, Publ. 120 and 151; Harkins, *Proc. Nat. Acad. Sciences*, 1916, vol. ii, p. 216.

[2] Hackh, *Journ. Amer. Chem. Soc.*, 1918, vol. xl, p. 1023.

[3] *Can. Chem. Journ.*, 1918; vol. ii, p. 135; *Science*, N.S., 1918, vol. xlviii, p. 333; *Chem. News*, 1919, vol. cxviii, p. 289.

[4] These four elements (Na, Mg, Al, Si), having a valency of 1, 2, 3 and 4 respectively, could also be made the basis for a system of structure symbols. However, as we know at present little or nothing of the structure of silicates, this proposal is somewhat premature. Nevertheless the writer could not refrain from pointing out this possibility, with the suggestion that both systems of structure symbols can jointly be used by differentiating the atoms of Na, Mg, Al, Si with circles.

of certain elements, their sequence following in general the periodic system, so the abundance of terrestrial elements may indicate the earth's stage of evolution! Where is the evidence that the chemical properties and physical characteristics of



what we call to-day, *e.g.*, sodium was the same eons ago, and will be the same eons hence?[1] Where is the evidence that our life

[1] "Old Age of Chemical Elements," *Science*, N.S., vol. xlix, 1919, p. 328; "A Possible Source of Cosmical Energy," *Science*, N.S., vol. l, 1919, p. 252.

based upon carbon is the only possible one in the universe, and that a life based upon silicon cannot exist? In short, what does this grouping together of the most important elements mean?

EXPLANATION OF FIGURE

THE IMPORTANCE AND DISTRIBUTION OF ELEMENTS

The importance of the elements is shown in a heavy, double, or single circle around the symbol and the atomic number.

The distribution of the elements is shown by letters:

A and *a* = occurrence in atmosphere, abundant and less abundant.
H ,, *h* = ,, ,, hydrosphere, abundant and less abundant.
L ,, *l* = ,, ,, lithosphere, abundant and less abundant.
M ,, *m* = ,, ,, meteorites, abundant and less abundant.
B ,, *b* = ,, ,, biosphere, abundant and less abundant.

(For an explanation of the new periodic table, see also *Journ. Amer .Chem. Soc.*, 1918, vol. xi, p. 1023; *Amer. Journ. Science*, 1918, vol. xlvi, p. 481; *Astrophys. Journ.*, 1918, vol. xlviii, p. 241.)

HEREDITY AND EVOLUTION IN PROTOZOA

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Ever since the general acceptance of the theory of evolution, biologists have been attempting to discover the method by which evolution has taken place. Certain experimental studies of the Protozoa that have been carried on in the laboratories of the Johns Hopkins University in Baltimore, Maryland, U.S.A., throw some light on this subject, and some of the results and conclusions of these investigations will be discussed in the following paragraphs.

Many investigators have shown that selection is ineffective as a means of changing the genotypic constitution of plants and animals when practised on self-fertilised plants, on plants reproducing by tubers or cuttings, and on animals reproducing by parthenogenesis, budding, and fission. Among the animals in which this has been found to be true is the ciliate protozoon Paramœcium (Jennings, 1908; Ackert, 1916).

The data presented below, however, all indicate that by selection it is possible to isolate heritably diverse strains within a family of protozoons derived by fission from a single specimen without the intervention of conjugation. The investigations described are—(1) heritable diversities in fission rate in *Stylonychia pustulata*, by Middleton (1915); (2) heritable diversities in shell size, spine number and spine length, and number of teeth in *Difflugia corona*, by Jennings (1916); (3) heritable diversities in spine number, shell size and form, and mouth size and form in *Centropyxis aculeata*, by Root (1918); and (4) heritable diversities in shell size, spine number, shape of mouth, and size and number of nuclei in four species of Arcella, by Hegner (1918, 1919*a*, 1919*b*, 1920).

Stylonychia pustulata, which was used by Middleton in his experiments, is a ciliate infusorian that divides, at a temperature of 70° F., about twice per day. It was found that

specimens produced by fission by a single parent differed in their rate of division as indicated in Fig. 1.[1]

Specimens were isolated on hollow-ground slides and carefully maintained under uniform conditions. An attempt was made to obtain a " high line," in which the fission rate was rapid, by selecting generation after generation the offspring of specimens that divided most quickly; and a " low line," in which the fission rate was slow, by selecting for further reproduction the offspring of specimens that divided slowly. Selections were thus carried on for 130 days, during which the differences in the fission rate between the two lines gradually increased until it averaged 21·2 per cent. During this period

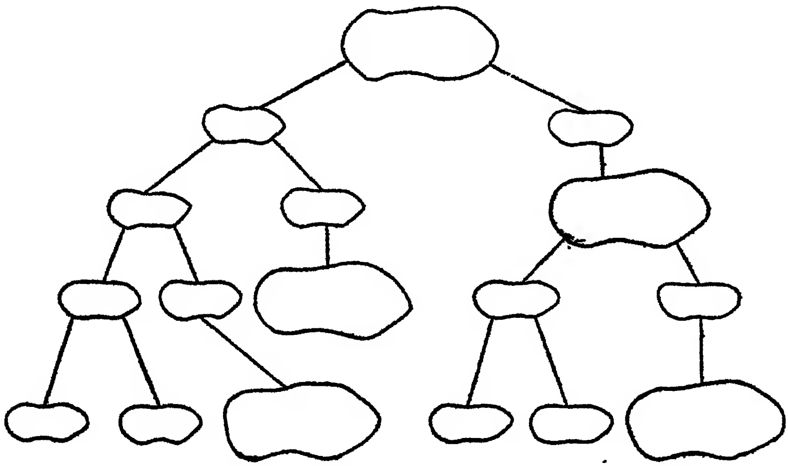

Fig. 1.

the number of generations produced in the high line averaged 183, and in the low line 122 (Fig. 2). The next problem was whether these high and low lines were heritably different, or would undergo complete reversion, as had been shown to occur in other organisms by many investigators. Accordingly, some of the members of the high line and some of the low line were, from time to time, cultivated without selection. The difference in the two lines persisted as long as they were maintained without selection (102 days), and the diversity in fission rate thus seemed to be inherited and to have been built up by means of small heritable variations. At the conclusion of the series of experiments the high and low lines were again subjected to selection, but in the reverse direction; slowly-

[1] For an explanation of these figures see the end of the article.

dividing specimens in the high line and rapidly-dividing specimens in the low line were selected generation after generation. This type of selection was also found to be effective, and the original high line became the slower one, and the original low line became the faster. The difference in fission rate was here likewise found to be heritable, since it persisted when the specimens were cultivated without selection. In order to be certain that heritable diversities in fission rate were not restricted to the descendants of one parent, but were characteristic of the species in general, another "wild" specimen was studied, and similar selections were practised on its descen-

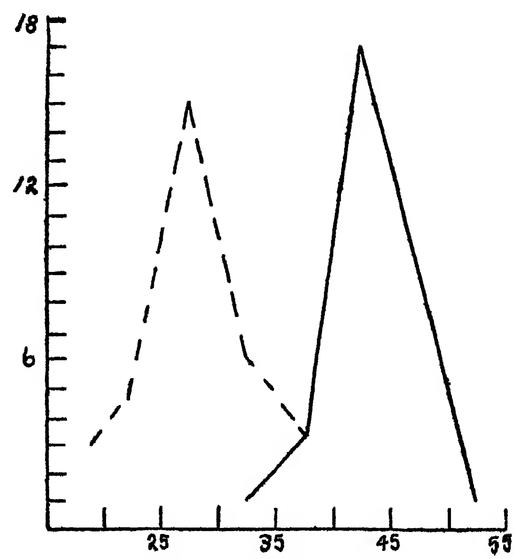

FIG. 2.

dants. The results in every way resembled those already described. By selection, high and low lines, with respect to fission rate, were obtained in which the difference gradually increased as selection progressed, and this difference was shown to be heritable, since it persisted through fifty days of non-selection, followed by conjugation, and then through fifteen days more of non-selection. The final conclusions are that in *Stylonychia pustulata* heritably diverse lines may be derived by selection from a single specimen dividing by fission; that these new lines (genotypes) have resulted from the accumulation of small heritable variations; and that the selection of such variations is effective for evolution in this species.

Jennings (1916) used the fresh-water Rhizopod *Difflugia corona* for his investigations of heredity, variation, and the results of selection in uniparental reproduction. The same problems as stated above were involved and similar methods were used in the experiments. *Difflugia corona* possesses a shell consisting of particles of sand fastened together by a secretion produced by the organism. As shown in Fig. 3, it has six characteristics suitable for selection work: (1) diameter of the shell; (2) depth of shell; (3) diameter of mouth; (4) number of teeth; (5) number of spines; and (6) length of spines. Specimens were brought into the laboratory and reared on hollow-ground slides under uniform conditions. They exhibited the following variations: (1) diameter

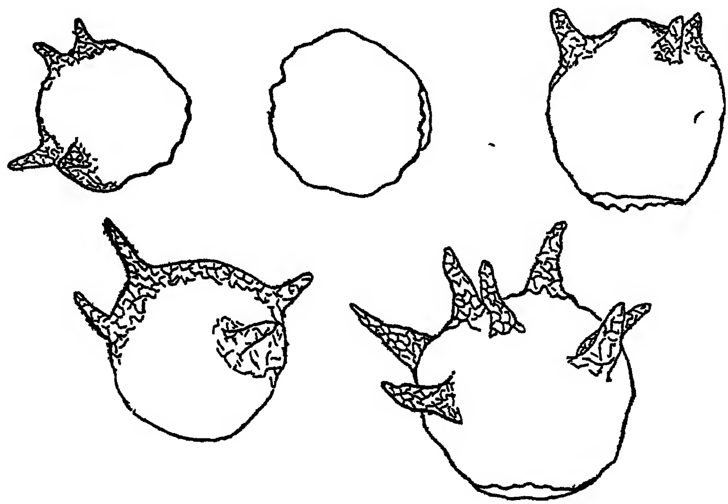

FIG. 3.

of shell, 107–280 microns; (2) depth of shell, 107–201 microns; (3) diameter of mouth, 56–98 microns; (4) number of teeth, 0–21; (5) number of spines, 0–14; (6) length of spines, 0–149 microns. The presence of diverse strains in nature was determined by the rearing of large numbers of specimens from single parents. For example, in one line containing 117 specimens the *spine number* ranged from 0 to 6, whereas in another line of 261 specimens the spine number ranged from 4 to 12. Ranges in variation of other characters were as follows: *number of teeth*, one line of 55 specimens, from 12 to 17; another line of 52 specimens, from 9 to 11; *diameter of shell*, one line of 40 specimens, from 121 to 145 microns; another line of

66 specimens, from 168 to 240 microns; *spine length*, one line of 23 specimens, from 9 to 46 microns; another line of 116 specimens, from 46 to 149 microns. When measurements of these characters were compared, it was found that correlations often existed within the various lines; for example, larger specimens usually have a greater number of spines and teeth, and larger spines than smaller members of the same line. Different lines, however, differed in these correlations, indicating that certain of the diverse hereditary characters vary independently. The number of heritably diverse lines that occur in nature is therefore very large.

Selection was carried on with several lines; the most valuable results were obtained from one line which was continued over a period of 252 days, and contained 4,645 specimens. The basis of selection used was chiefly past performance. For example, when an attempt was made to obtain high and low lines with respect to spine number, all members in one line (the high line) were kept that had many spines, and that produced offspring with many spines. If a specimen of the high line that possessed many spines persisted in giving rise to offspring with only a few spines, it was discarded. Similarly, members of the low line were discarded if they produced progeny with many spines, although they themselves possessed none or only a few spines. Extensive experiments with this large line resulted in the isolation by selection of heritably diverse strains. The diversities occurred with respect to number of spines, length of spines, number of teeth, and diameter of shell, and since the last three named varied independently, a large number of heritably diverse strains were obtained (Fig. 4). While these heritable diversities apparently were brought about gradually by small heritable variations, larger changes were observed which might be brought under the definition usually given to the word "mutation." For example, in one line with an average diameter of about 190 microns, one specimen appeared that was about 40 microns larger than the others; the descendants of this specimen were likewise about 40 microns larger than the other members of the line.

Root's (1918) investigations on inheritance in asexual reproduction were carried on with the fresh-water Rhizopod, *Centropyxis aculeata* (Fig. 5), a species resembling *Difflugia corona* in many respects. His work is not so extensive as that of Jennings, but confirms certain results obtained by the latter. It was first shown that diverse strains exist in nature. The descendants of one "wild" specimen had on the average about 6 spines, and divided on the average once every 6 days; whereas those of another "wild" specimen averaged 1·4

spines, and divided once every 11 days. Diversities in the size of the shell and of the mouth and in number of spines were found to be inherited. The 1,277 descendants of one

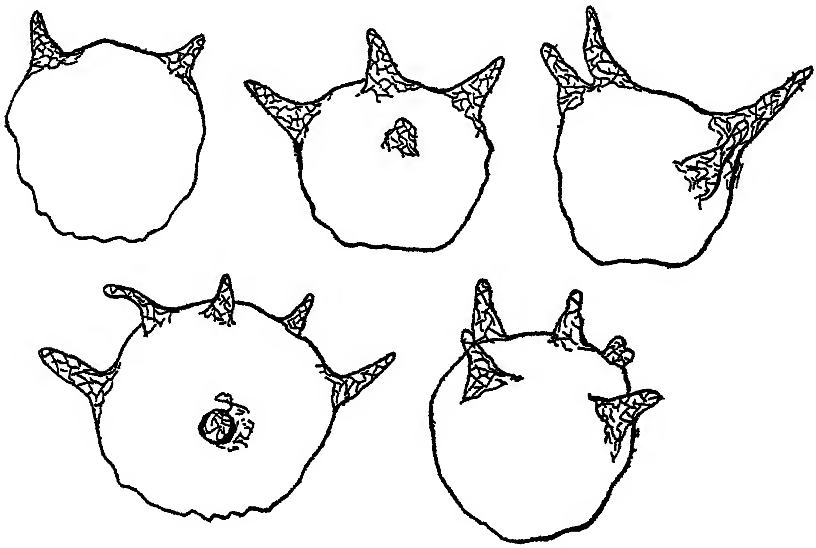

FIG. 4.

"wild" specimen were used to determine whether diverse lines could be isolated by selecting, on the one hand, individuals with a low spine number, and on the other hand those with a high spine number. As a result two diverse lines were obtained

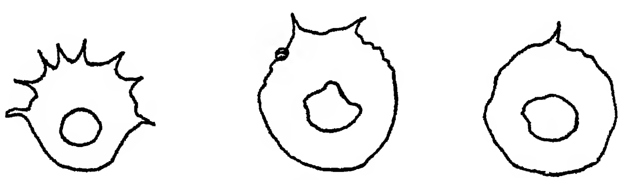

FIG. 5.

(Fig. 6). The average spine number before selection was begun was 2·67; that of the high line reached, and maintained after selection was discontinued, an average spine number of 2·75; whereas that of the low line decreased to 1·76. Root does not consider the isolation of these heritable diversities as invalidating the "pure-line" hypothesis, since we may

assume that each specimen " inherits, in general outlines, the genetic constitution of its species; in smaller details, that of its pure line; in still more minute details, that of its individual parent; and yet may differ from this parent in some hereditary points."

The researches of the writer (Hegner, 1918, 1919*a*, 1919*b*, 1920) on four species of the genus Arcella are in accord with those described above, but carry the investigation further, involving the relations between nucleus, chromatin, and cytoplasm, and shell characteristics.

Two very interesting biological theories involved in these studies are those of Minot (1908) and of Hertwig (1903), regarding the problems of growth, cell-division, old age, senescence, physiological degeneration, and death. These theories

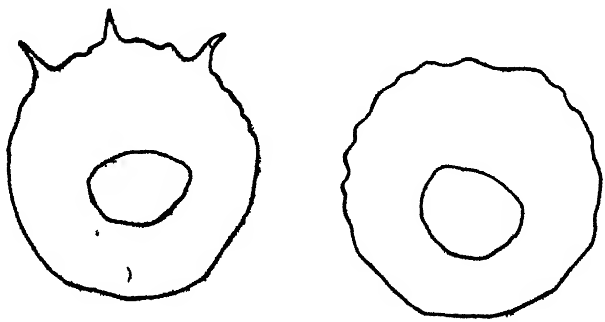

FIG. 6.

are based on the idea that the nucleus is the dynamic centre of the cell. The nuclear membrane is considered as semi-permeable, allowing a constant but selected flow of materials from the nucleus into the cytoplasm and *vice versa*. Involved in the theory is also the idea expressed by Sachs in 1892, and the following year by Strasburger in a slightly different form. According to Sachs, the nucleus and that part of the cytoplasm that falls within its " sphere of influence " make a morphological and physiological unit. He proposed the term " energid " for such a unit. Strasburger (1893) also recognised a limit to the distance through which the metabolic interchange between nucleus and cytoplasm could take place, and was able to show that in the cells of certain plants the ratio between nuclear size and cell size was fairly constant. He accounted for cell-division on the hypothesis that, when the cytoplasm had increased so as to extend beyond the working sphere of the nucleus, the normal ratio was regained by the division of both nucleus and cytoplasm into two. The nucleo-cytoplasmic

relation theory as we now know it is largely due to the investigations carried on by Richard Hertwig and his students. Hertwig (1903) came to the conclusion that in every cell under normal conditions there is a definite relation between the quantity of cytoplasmic and nuclear materials, which he called the " Kernplasmarelation." According to Hertwig, the magnitude of the K/P (nucleus/cytoplasm) ratio determines vital processes such as growth, division, senescence, and physiological degeneration. The latter, for example, result from an abnormal increase in the ratio (*i.e.* in the growth of the nucleus); and certain rhizopod and ciliate protozoa in which this condition was induced by overfeeding either died, or else regained the normal ratio by direct elimination of nuclear material or by conjugation. The intervals between successive divisions were divided by Hertwig into two periods: (1) a period of " Funktionelle-wachstum " during which the cytoplasm grows more rapidly than the nucleus, leading to an abnormal K/P ratio; and (2) a period of " Theilungs-wachstum " during which the normal K/P ratio (the " Kernplasma-Norm ") is regained by the rapid growth of the nucleus, and at the end of which cell-division occurs. The end of the period of functional growth, when the K/P ratio is abnormal, is the moment of " Kernplasma-Spannung." Many external and internal conditions are supposed to affect the normal K/P ratio. Thus Hertwig found that the ratio was increased if the organisms were subjected to low temperatures, and was diminished at higher temperatures.

Many investigators have studied the nucleo-cytoplasmic relations in Protozoa and in Metazoa, confirming in part the conclusions reached by Hertwig and his followers. The work of Minot (1908), however, leads to a theory directly opposed to that of Hertwig. Minot finds that in segmenting eggs the amount of nuclear material increases as compared with the quantity of cytoplasm, and concludes that this increase is indicative of the process of rejuvenation. Rejuvenescence is thus revealed as an increase of the nuclear material, and senescence as an increase and differentiation of the cytoplasm. When applied to Protozoa, Minot's theory requires, " in those cells which are old, an increase in the proportion and in the differentiation of the protoplasm (cytoplasm), and consequently a diminution in the relative amount of nucleus " (1908, p. 231).

The theory offered by Hertwig to account for the various complex processes that occur during the life-cycles of the Protozoa appears at first quite plausible, but will not withstand close analysis. The data are entirely inadequate to sustain the claims of the theory, but investigations of the actual relations of nucleus, chromatin, and cytoplasm to one

another, and their relations to external heritable characteristics, are very desirable.

The two species of Protozoa that were chiefly used in my investigations are shown in Fig. 7. They belong to the class Rhizopoda and to the genus Arcella. Both species are provided with shells secreted by their protoplasm. The pseudopodia (which are locomotor and food-capturing protrusions) are thrust out through a circular opening in the centre of the under-side. The species *Arcella dentata* is characterised by the presence of two nuclei, which can be seen through the shell in the living animal, and by a circlet of spine-like projections around the periphery which vary in number

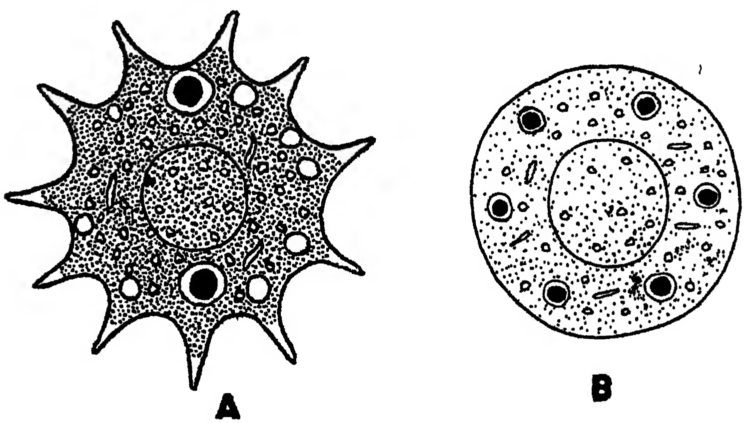

FIG. 7.

from 7 to 20. The diameter of the shell ranges from about 100 to 150 microns. *Arcella polypora*, the other species studied, lacks these spines, but possesses a variable number of nuclei ranging, in nature, from 3 to 13. The shell in this species varies in diameter from 73 to 327 microns. Some of the advantages of these organisms for nucleo-cytoplasmic studies are as follows: they multiply rapidly by simple division; their morphological characteristics are definite and easily measured; they are large enough to be conspicuous when placed under a binocular microscope; they withstand severe operations; they can easily be cultivated; and their nuclei and other cell contents can be observed and measured through the shell of the living animal. There is space available here only for the statement of certain results of these investigations; but it is believed that the conclusions reached are based on adequate data.

Arcella dentata.—It was found that many strains exist

within this species in nature. For example, from one specimen with 10 spines, 148 descendants were obtained by fission; these had a spine number ranging from 8 to 14, and a mean spine number of 11·04. By way of contrast, another specimen was the progenitor of 39 descendants which varied in spine number from 12 to 16, and had a mean spine number of 14·07. Extensive measurements revealed the fact that a definite size, as indicated by the diameter of the shell, is a very constant character of the specimens that have descended from one progenitor. Furthermore, when spine number and diameter of shell were compared, it was discovered that these two characters are very closely correlated, and that the greater the diameter the larger the number of spines. Thus specimens with 10 spines had a mean diameter of 112 microns; those with 11 spines had a mean diameter of 114 microns; those with 12 spines had a mean diameter of 118 microns, etc. A large family containing 5,557 specimens and representing 69 generations was reared in the laboratory from a single specimen by vegetative reproduction (fission). Selection as regards spine number and diameter was practised within this family by preserving on the one hand those specimens that had few spines and were small in diameter, and on the other hand those specimens that had many spines and were large in diameter. By this means two heritably distinct branches were isolated within this family; the members of the larger branch had a mean spine number of 11·48, whereas those of the smaller branch had a mean spine number of 10·32; and these differences persisted for many generations after selection was discontinued (Fig. 8). It was concluded from these data that the diversity obtained within this family represented a change in the germ-plasm, and that evolution had thus been observed in these small organisms in the laboratory.

These measurements prepared the way for experimental studies of nucleus and cytoplasm. As shown in Fig. 7, *Arcella dentata* possesses two nuclei. These nuclei are almost invariably located on opposite sides of the mouth opening as indicated. They are of the vesicular type, the chromatin being concentrated in the centre of the nucleus as a spherical mass. Both the nuclei and the chromatin mass within them were observed and measured while the specimens were alive. The ability of these organisms to withstand severe operations is quite remarkable, and many cutting experiments were performed with them. Specimens, when cut into two or even four pieces, continued to live and reproduce, and the part of the shell that was cut away was in no case regenerated. When a binucleate parent is cut in two so that each half contains a single nucleus, the halves produce uninucleate descendants

for a varying number of generations (Fig. 9). These uninucleate specimens are smaller than the original parent, but resemble each other in size and spine number. For example, a binucleate specimen with 13 spines and a diameter of 150 microns was cut in two, and its uninucleate halves produced 209 descendants which had an average spine number of 10·48, and an average diameter of 26·45 microns. These uninucleates, after a few generations, regained the binucleate condition, and as soon as this occurred the diameter and spine number of the

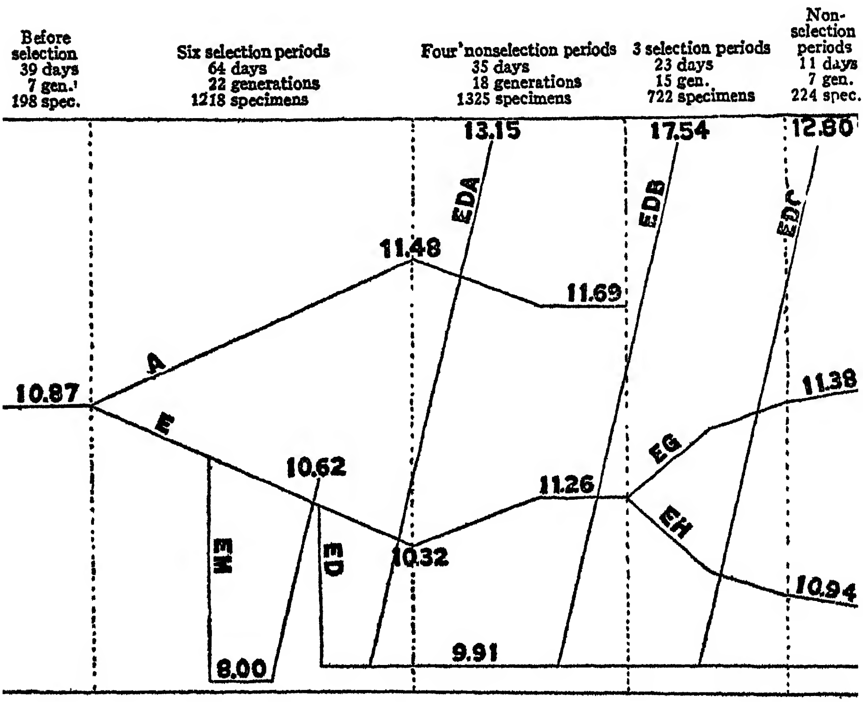

FIG. 8.

specimens increased very quickly to that of the original binucleate parent. Operations of this kind were performed many times, and in all 757 binucleate and uninucleate specimens were measured and recorded. In every case the results were similar to those just described. The conclusion reached is that each nucleus in a binucleate Arcella is associated with a definite quantity of cytoplasm; that the quantity that comes within the sphere of influence of a single nucleus may be obtained by determining the mass of cytoplasm within a uninucleate specimen. This mass is doubled when the binucleate condition is regained.

With these data in mind, a plausible hypothesis immediately suggests itself to account for the differences in size and spine number between different strains of these organisms. This hypothesis is that the quantity of chromatin within the nucleus determines the limits of the reactions of the nucleus with the cytoplasm, and that the greater the chromatin mass, the greater the quantity of cytoplasm associated with it, and the larger the specimen will be that contains it. Since the chromatin mass can be measured, and its size determined, this hypothesis could be tested. It was so tested and found to be correct. For example, the chromatin masses of specimens

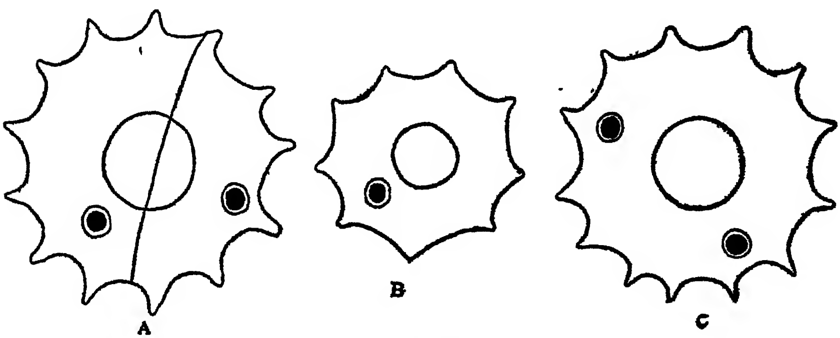

FIG. 9.

belonging to a small family were measured, and a corresponding number from a large family, with the following results :

	No. of Specimens measured.	Average Diameter in Microns.	Average Spine Number.	Comparative Diameter of Chromatin Mass in Units.
Small Family .	40	112	9·91	35·66
Large Family .	40	150	14·25	47·82

As the hypothesis demands, the quantity of chromatin within the nuclei of the small family is less than that within the nuclei of the large family. The differences in size among the families reared from single " wild " specimens appear, therefore, to be due to differences in the quantity of chromatin present in them. Similarly, the difference in size and spine number between the two branches of the large family described above are probably also due to a diversity in chromatin mass, although the origin of this diversity is problematical.

Arcella polypora.—The characters of this species of Arcella that were studied are diameter, nuclear number, and chromatin mass. As in *Arcella dentata,* families of this species reared from single " wild " specimens by vegetative reproduction

differed in the characters mentioned. First a study was made of the number of nuclei and the diameter of the shell. Two representative families will be described here. In family 5 the nuclear number ranged from 3 to 7. Specimens with one and with two nuclei were obtained by cutting trinucleate specimens in two. Changes in nuclear number within the members of the family were continually taking place. Thus a specimen with 3 nuclei sometimes produced offspring with 4, 5, or 6 nuclei, and a specimen with 5 nuclei sometimes gave rise to descendants with 3, 4, 6, or 7 nuclei. The most striking phenomenon revealed by these organisms was the close correlation between nuclear number and size. In every case, when an increase in nuclear number occurred, it was accompanied by an increase in the size of the specimen, and likewise a decrease in nuclear number always was accompanied by a decrease in size. The members of family 34 were provided with from 5 to 10 nuclei. Changes in nuclear number were accompanied by changes in size as in family 5, but the members of family 34 with a certain number of nuclei were always smaller than those in family 5 with the same number of nuclei. The differences are indicated in the following table :

No. of Nuclei.	Average Diameter of Specimens in Microns.	
	Family 5.	*Family* 34.
1	82	
2	86	
3	109	
4	113	
5	120	105
6	127	106
7	130	112
8		116
9		120
10		125

These results led to a study of the chromatin masses within the nuclei of members of the two families, and to the conclusion that in this species, as in *Arcella dentata*, size depends upon the quantity of chromatin present. Measurements of the chromatin masses of specimens in families 5 and 34 showed that in both families a certain size is correlated with a certain amount of chromatin, and that increases and decreases in size are accompanied by corresponding increases and decreases in chromatin mass. The following table shows that this is true :

Average Diameter of Shell in Microns.	Chromatin Mass in c.mm.	
	Family 5.	*Family* 34.
112·19	781·76	831·88
116·67	977·20	950·72
125·56	1172·64	1188·40

The measurements (see both preceding tables) show, for example, that, as a rule, specimens that are about 112 microns in diameter possess 4 nuclei in Family 5, and 7 nuclei in Family 34, but that the *quantity* of chromatin in each nucleus of the specimens in Family 5 is greater than that in each nucleus of the specimens in Family 34, and that the *total quantity* of chromatin in such specimens in Family 5 is approximately the same as in those in Family 34. The conclusion is inevitable that in these organisms each nucleus is associated with a rather definite quantity of cytoplasm, and that the quantity of cytoplasm varies directly as the quantity of chromatin within the nucleus. Why the chromatin masses should be smaller and more numerous in specimens of one family (34) than in those of the other family (5) is a problem still unsolved.

Senescence and Death in Arcella dentata.—During the course of the experiments with *Arcella dentata*, certain data were obtained that seem to favour Minot's theory, that senescence and death are due to an excess in the amount of cytoplasm as compared with that of the nucleus. From a line containing relatively small specimens (Fig. 8, ED)—specimens averaging 101 microns in diameter and 9·91 in spine number—a branch line (Fig. 8, EDB) was derived, consisting of enormously large specimens averaging 153 microns in diameter, and 17·54 in spine number. These large specimens received particular attention, but in spite of every care they all died after several generations. In the meantime, measurements of their chromatin masses were made, and these were found to be almost exactly equal in size to those in the small specimens from which they were derived. Death in this case may have been due to the failure of the chromatin masses to increase along with the cytoplasm, and hence due to an excess of cytoplasm over chromatin.

Chromidia in Arcella.—Arcella is one of the first organisms in which extra nuclear granules, now known as chromidia, were discovered. A band of these granules was described by R. Hertwig in 1887, and the same investigator twelve years later (Hertwig, 1899) recorded the formation of secondary nuclei from this " extra-nuclear chromatin net." These nuclei, according to Elpatiewsky (1907), become the centres of amebulæ of two sizes, which are macrogametes and microgametes, and which conjugate in pairs. The conjugation of the entire chromidial nets of pairs of Arcellas was reported the following year by Swarczewsky (1908). Chromidia have been described in many groups of Protozoa, and seem to play an important part in their life-cycle. For this reason it seems worth while to mention the apparent lack of influence of this extra-nuclear chromatin during the bisection experiments performed on *Arcella dentata.* These experiments demonstrate that, when

from one-tenth to three-fourths of the entire chromidial net is removed, the descendants of the part that remains invariably attain the normal condition of the family. When a large Arcella is bisected and both nuclei are present in one-half, the other half, without nuclei, always dies within a few days without any visible attempt on the part of the chromidia to form new nuclei. This is true even when only the nuclei are removed and all the cytoplasm is left in the shell. The evidence justifies the conclusion that the chromidia play no rôle in vegetative reproduction, and at this time have no influence upon the size of the organism nor upon the characteristics of the shell.

All the evidence derived from these extensive and intensive investigations of Arcella indicates a definite quantitative relation between nucleus, or rather chromatin, and cytoplasm. This evidence was obtained by accurately measuring these materials in the living organisms, and is therefore more dependable than similar measurements of fixed and stained tissues. Present theories of the reactions between chromatin and cytoplasm, with their widespread influence on the biological and medical science, are based on insufficient and often erroneous data. This situation can be improved by the direction of investigators to the study of fundamental cell problems with living organisms.

Conclusion.—The investigations described above all prove that in five species of Protozoa (*Stylonychia pustulata*, *Difflugia corona*, *Centropyxis aculeata*, *Arcella dentata*, and *A. polypora*) heritably diverse branches may be isolated from the descendants of a single specimen obtained by asexual reproduction. These heritably diverse branches resemble the diverse strains that in each case were shown to exist in nature. The latter may probably have originated in nature in a similar manner. The chromatin-cytoplasmic studies of *Arcella dentata* and of *A. polypora* are of particular interest because they deal with the internal changes that accompany the differences in external heritable characteristics. The correlation of these external heritable characteristics, with the quantity and distribution of the chromatin within the cell, is a considerable step forward in our knowledge of the method of evolution. These researches, however, leave many interesting problems for future investigations, but " to travel hopefully is a better thing than to arrive, and the true success is to labour."

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EXPLANATION OF FIGURES

FIG. 1.—*Stylonychia pustulata.* Diagrams of successive fissions among the progeny of a single specimen illustrating the variations in fission rate. The left side of the figure traces a series of "fast" selections, the right side a series of "slow" selections, showing that at a given moment we may have, among the progeny of a single parent, individuals of the fourth and of the second generation. (From Middleton, 1915.)

FIG. 2.—*Stylonychia pustulata.* Curves indicating the difference in rate of fission between "fast" lines (continuous line) and "slow" lines (broken line). (From Middleton, 1915.)

FIG. 3.—*Difflugia corona.* Typical specimens from five different strains, showing the character of the diversities observed. (From Jennings, 1916.)

FIG. 4.—*Difflugia corona.* Typical specimens from five heritably diverse branches of a single family, showing differences in size of body and length of spine. (From Jennings, 1916.)

FIG. 5.—*Centropyxis aculeata.* Typical specimens from three different strains. (From Root, 1918.)

FIG. 6.—*Centropyxis aculeata.* Typical representatives of the high and low branches obtained by selection within a single strain. (From Root, 1918.)

FIG. 7.—A. *Arcella dentata.* Surface view of a typical specimen, with eleven spines and a diameter of 116 microns. (From Hegner, 1919.) B. *Arcella polypora.* Surface view of a typical specimen, with six nuclei and a diameter of 119 microns.

FIG. 8.—*Arcella dentata.* Diagram showing the most important heritably diverse lines derived from a single specimen by fission. The character used was spine number. The letters indicate the designations of the lines, and the numbers are the mean spine numbers. (From Hegner, 1918.)

FIG. 9.—*Arcella dentata.* A. Binucleate specimen, with 13 spines and diameter of 150 microns, with line showing where it was cut in two. B. Uninucleate specimen, with 9 spines and a diameter of 112 microns, produced by one-half of the binucleate A. C. Binucleate specimen, with 13 spines and a diameter of 152 Microns, descended from the uninucleate B.

POPULAR SCIENCE

THE SILENT ZONE IN EXPLOSION SOUND-AREAS

By CHARLES DAVISON, Sc.D., F.G.S.

Various explosions during the present century have revealed a phenomenon which has an important bearing on the mode in which sound is transmitted by the atmosphere. In some great explosions, though not in all, the sound-area consists of two detached portions. One portion, the inner sound-area, surrounds the source of sound. The other, the outer sound-area, is at its nearest point from fifty to a hundred miles or more from the source. Between them lies a tract—the silent zone—within which the sound is rarely or never heard.

The accompanying maps will give some idea of the forms and positions of these sound-areas. In each map, the source of sound is indicated by a small cross, and the boundaries of the sound-areas are represented by the dotted lines.

Fig. 1 represents the silent zone and outer sound-area of the minute-gun fired at Spithead on February 1, 1901, when Queen Victoria's body was borne from the Isle of Wight to Portsmouth. Owing to the general absence of observations from the sea-covered district, no attempt can be made to map the inner sound-area. Its boundary, however, certainly passed within a very few miles—possibly within one mile—to the north of the line of battleships. Then, still farther to the north, there lay the broad silent zone, within which not a sound of the reports was heard. Indeed, the nearest point on land at which they were recorded is fifty miles from Spithead. From this point, there spreads outwards the great outer sound-area, the sullen booms being unusually loud from sixty to ninety miles from Spithead. Even more than a hundred miles away, the reports were noticeable, the greatest distance being 139 miles towards the north-east. In the south of Hampshire, the wind at the time was mainly from the shore; in the outer sound-area,

the air was more nearly calm, but what wind there was came from a southerly direction.[1]

The sound-areas of the great munitions explosion which occurred in East London on January 19, 1917, are drawn more completely, and with much greater accuracy. They are shown in Fig. 2. The inner sound-area contains 3,390 square miles, and is of unusual form. It is distorted in two directions, extending to the east-south-east as far as Canterbury (48 miles from East

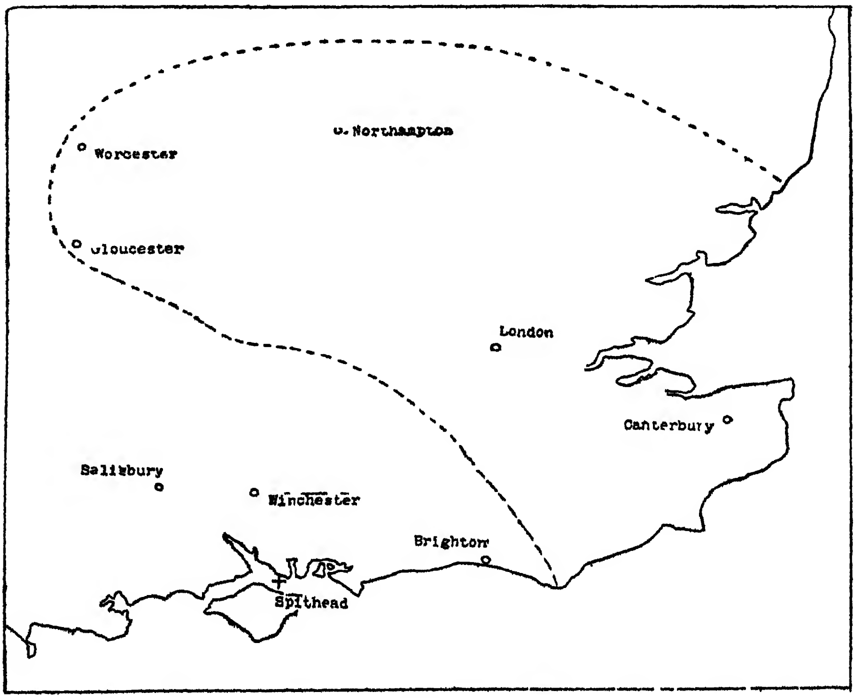


FIG. 1.

Sound-area of Spithead minute-guns, February 1, 1901.

London), and to the north-west as far as Wellingborough (66½ miles); while, towards the north-east and south, its boundary passes within 20 and 19 miles respectively of the ruined factory at Silvertown. The outer sound-area is of greater extent. Its length from near Nottingham to the east coast of Norfolk is 131 miles, and the area covered by it is 5,000 square miles. The most distant place at which the explosion was audible is 128 miles from East London. The width of the intervening

[1] "On the Audibility of the Minute-guns fired at Spithead on February 1, 1901," *Knowledge*, vol. xxiv, 1901, pp. 124–5.

silent zone varies from 28 miles at the western end to 48 miles close to the eastern coast, while the outer margin of the zone is comprised between distances of 65 and 95 miles from the source. Over the greater part of the sound-area, the air was either still or there was a light wind from the north-east.[1]

The sound-areas of a very different kind of explosion are represented in the remaining map, Fig. 3. From the plateau which occupies the centre of the main island of Japan rises

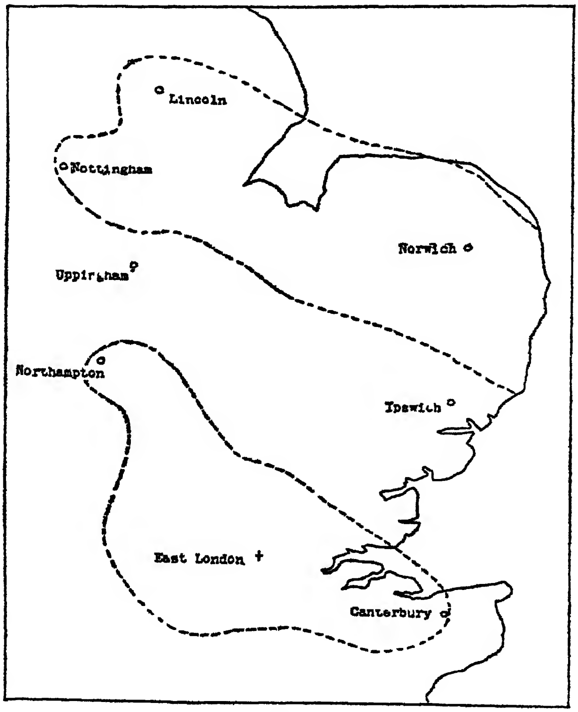


FIG 2.

Sound-areas of East London explosion, January 19, 1917.

the Asamayama, one of the greatest of Japanese volcanoes. Its return to activity during the present century, after a long interval of quiescence, has been distinguished by a series of explosions, the loudest of which are heard for a hundred miles or more from the volcano. In the explosion of December 25, 1910, the double sound-area was noticed for the first time in

[1] "The Sound-waves and other Air-waves of the East London Explosion of January 19, 1917," *Edinburgh Royal Society Proceedings*, vol. xxxviii, 1918, pp. 115–29; see also the *Quarterly Review* for July 1917, pp. 51–60.

Japan. Both areas, it will be seen from Fig. 3, are elongated from north to south, the Asamayama itself being close to the western boundary of the inner area. The distance from the volcano of the nearer margin of the outer area is 54 miles. [1]

Properties of the Silent Zone

(1) As a rule, the silent zone is free from all sound of the explosion, but, here and there within it, there may be places at which the reports are heard by one or more persons. The silent zone is then, strictly speaking, a zone of diminished

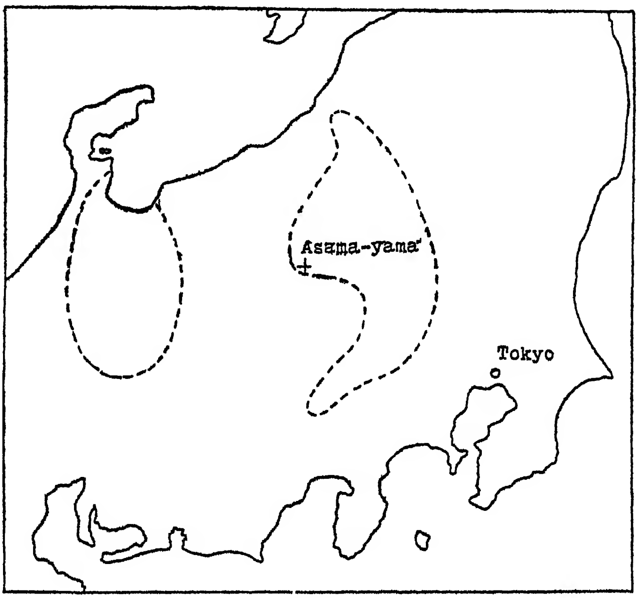


Fig. 3.

Sound-areas of the Asamayama explosion, December 25, 1910.

audibility. For instance, in the dynamite explosion which occurred on the Jungfrau railway in 1908, the reports were heard on isolated mountain-summits within the silent zone. In the East London explosion of 1917, the boom was heard at Ipswich, five miles to the south of the outer sound-area; possibly also at Uppingham and Lilford in the narrow and more lofty western end of the silent zone. In addition to the sound-waves

[1] F. Omori, "The Eruptions and Earthquakes of the Asamayama," *Bulletin of the Earthquake Investigation Committee*, vol. vi, 1912, pp. 1–147; vol. vii, 1914, pp. 1–215; "The Sakura-jima Eruptions and Earthquakes," *ibid.*, vol. viii, 1916, pp. 35–179.

of this explosion, there were other air-waves, inaudible to man, but manifested by the shaking of windows and the quivering of trees on which pheasants were roosting. These air-waves evidently travelled along a somewhat less lofty course than the sound-waves. They spread, not only over the two sound-areas, but across the intervening silent zone, at the western end of which their effects were frequently noticed. There can be little doubt, I think, that, if the sound-waves had taken a lower path, the report would have been heard right across this end of the zone. The two sound-areas would then have coalesced, and the silent zone would have been merely a deep gulf in a continuous sound-area. An explosion at Kobe, in southern Japan, on April 3, 1910, seems to have provided a sound-area of this type, but the observations are insufficient to place the matter beyond doubt.

(2) The number of explosions in which the form of the sound-area has been traced is comparatively small. In Europe, the silent zone has been mapped seven times during the present century—namely, for the Spithead minute-guns on February 1, 1901, an explosion of dynamite at Förde (Westphalia) on December 14, 1903, of nitroglycerine at Hayle (Cornwall) on January 5, 1904, of dynamite on the Jungfrau railway on November 15, 1908, and of gunpowder at Wiener-Neustadt on June 7, 1912, the bombardment of Antwerp on October 8, 1914, and the munitions explosion in East London on January 19, 1917. On the other hand, no trace of a silent zone has been discerned in the sound-areas of the Spithead naval reviews of July 17, 1867, and June 26, 1897, the Avigliana (Northern Italy) explosion of January 16, 1900, or the Cherbourg naval review of July 17, 1900.

In Japan, Prof. Omori has studied the sound-areas of twenty-two important explosions of the Asamayama, from 1910 to 1913, and he finds that single sound-areas and double sound-areas occur with equal frequency. Of the single sound-areas, nine were those of explosions which occurred in the six winter months, and two of explosions in the six summer months; of the double sound-areas, ten were those of explosions in the summer half of the year, and one of an explosion in the winter half.

The materials are perhaps insufficient to lead to any general law. It is clear, however, that the silent zone is not present in all explosion sound-areas, that roughly in great explosions it is absent as often as it is present; and it would seem probable that in European explosions it is chiefly manifested in the winter half of the year, and in the Japanese volcanic explosions in the summer half.

(3) A third property of the silent zone, of no less significance than its intermittent existence, is the positions of the two

sound-areas with regard to the source of sound. It is only in the third of the accompanying maps that the normal relations are clearly shown. For the Spithead minute-guns (Fig. 1), the inner sound-area cannot be mapped. In Fig. 2, the form of this area is exceptional. As a general rule, the source of sound lies close to one side or one end of the inner area (as in Fig. 3), and the outer sound-area lies, not in the direction of greatest extension of the inner area, but in exactly the opposite direction.

That direction, as will be seen from the maps, is by no means uniform. The outer sound-area lies to the north of the origin for the Spithead minute-guns and the East London explosion, towards the east in the Förde and Hayle explosions and the Antwerp bombardment, and towards the west in the Wiener-Neustadt explosion. In the Japanese volcanic explosions, it lies on the west side of the Asamayama, but on the north side of the Sakura-jima in the south of the empire.

How closely the directions of the Asamayama sound-areas are connected with the direction of the wind in the upper atmosphere has been clearly shown by Prof. Omori. The column of ashes from the volcano is shot up to a height of two or three miles, to a region of the air in which the wind is generally from the west. The ashes are thus carried in an easterly direction, and are deposited along a narrow band, sometimes as much as one hundred miles in length. The boundary of the inner sound-area may be deformed slightly by local winds, but, as a whole, the area is arranged more or less symmetrically about the band of ashes.

There is no hard-and-fast rule binding the position of the outer sound-area with regard to the direction of the ashes-zone. But it is noticeable that, when the ashes are carried due east, the centre of the outer area lies to the west of the Asamayama. When the ashes drift to the east-south-east, the centre of the outer area is shifted towards the north; and, when they are borne to the east-north-east, the centre is deflected to the south. Thus, as a rule, the direction of the outer area is nearly opposite to that in which the band of ashes is drawn out by the wind.

(4) The dimensions of the silent zone, and especially those of its outer boundary, have an important bearing on the origin of the zone. The mean radius of the boundary varies between rather wide limits. In European explosions it may be as low as 50 miles (Spithead minute-guns), or as high as 112 miles (Jungfrau railway explosion). In the volcanic explosions of Japan, the distance lies between 54 and 105 miles, or about the same limits as in Europe. The average of all known measurements of the outer radius is 87 miles.

(5) One more property, not so much of the silent zone as of the outer sound-area, may be noticed. Considering its distance

from the source, the intensity of the sound heard within that area is extraordinary. At 84 miles from Spithead, the reports of the minute-guns in 1901 were so loud that men labouring in the fields stopped their work to listen. In the East London explosion, the sound at no part of the outer area was so loud as in the immediate neighbourhood of the ruined factory; but the area of moderate loudness within it was nearly three times as great as that within the inner sound-area.

Origin of the Silent Zone

If the sound-waves were to travel continuously along or close to the ground, they would be audible over the whole district to the farthest point reached by them, and there would be no silent zone. If they were to follow a course not far above the ground, secondary waves would spread downwards from their lower edges, and would be heard here and there by persons who are capable of hearing low sounds of slight intensity. Thus, the mere existence of the silent zone is evidence that the sound-waves cross it by an elevated route; and, to discover the origin of the silent zone, we have to ascertain the cause or causes which lead the sound-waves to desert the neighbourhood of the ground at the boundary of the inner area, and afterwards return in but slightly diminished strength to the outer sound-area.

It is well known that, in the neighbourhood of the ground, the velocity of the wind increases with the height. If a sound-wave were travelling along the ground in any direction inclined to the horizon, the effect of this increased velocity would be to tilt the upper part of the wave forward if it were advancing with the wind, and backward if against it. As the wave travels in a direction at right angles to its surface, it follows that the sound-waves are tilted upwards if moving against the wind, and soon pass over the heads of observers on the ground. In the opposite direction, waves at first proceeding on an upward course are deflected downwards, and are therefore heard along the ground to a much greater distance than in the opposite direction.

The diagram (Fig. 4) is designed to represent approximately the paths of the sound-waves. The dotted lines are supposed to be everywhere at right angles to the surfaces of the waves, and therefore to represent sound-rays indicating at any point the direction in which a wave is moving. They are drawn without regard to scale or exact form. The object of the diagram is simply to represent the facts that, if the velocity of the wind increases upward, the sound-rays proceeding with the wind are concave, and those against it convex, towards the ground. In the line below the diagram, O represents the source of sound, OA the range of the sound with the wind, OB the range against it.

As every part of the region from A to B receives sound-rays, it is clear that, under such a condition of wind, there can be no silent zone.

If, on the other hand, the velocity of the wind were to decrease upward, the sound-rays would be curved in the opposite directions. Those with the wind would be convex, and those against it concave, towards the ground. The diagram (Fig. 4) will represent the corresponding sound-rays, except that the arrow showing the direction of the wind should be reversed, while OB will represent the range of sound with the wind, and OA the range against it. There would, again, be no silent zone.

Now, let us suppose that the velocity of the wind increases with the height up to a certain level, and then continually decreases, there being no change in the direction of the wind.

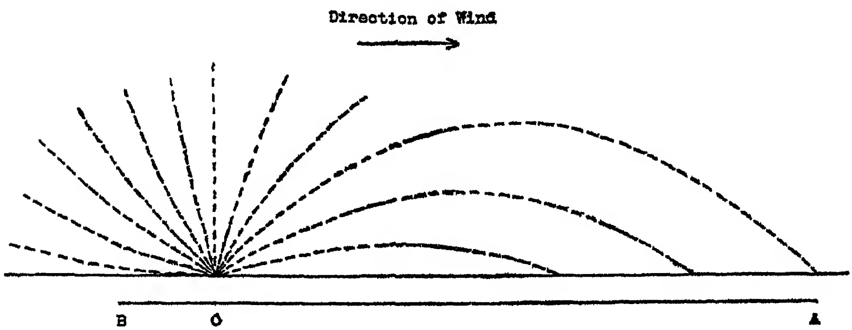


FIG. 4.

Diagram showing the course of the sound-rays when the velocity of the wind increases with the altitude.

Below the level LL at which the change takes place, the form of the sound-rays would be as in Fig. 4, the rays *a* (Fig. 5) being those with the wind, and the rays *b* those against it. Above the level LL, the form of the rays would be, as in Fig. 4, reversed, the rays *c* with the wind would become convex, and those *d* against it concave, towards the ground. The former (*c*), it will be seen, rise upward and are lost in the upper air. The latter (*d*) return to the level LL, and, entering the lower atmosphere (*e*), become convex again towards the ground. Thus the course of the sound-rays would be roughly as shown in Fig. 5. In the line below the diagram, O represents, as before, the source of sound, OA the range of sound in the inner area with the wind, and OB the range against it, BC the width of the silent zone, and CD that of the outer sound-area. The corresponding map of the sound-areas would resemble very closely that shown in Fig. 3; for the source of sound occupies a place within the

inner area not far from that side from which the wind comes, and the outer sound-area also lies on that side of the source which is opposite to the direction of the wind.

One other point is worthy of notice before leaving this diagram. It is evident that many of the sound-rays (*c*) proceeding with the wind enter the upper atmosphere and are lost to observers on the ground. In the opposite direction, a large proportion of the sound-rays (*d*, *e*) finally reach the outer area; and this perhaps accounts for the extraordinary intensity of the sound which is developed within the outer area as compared with that near the margins of the inner area.

If the temperature of the air and its velocity at every point were known, it would, of course, be possible to calculate exactly the form of the lines shown in Fig. 5. But, as we are ignorant

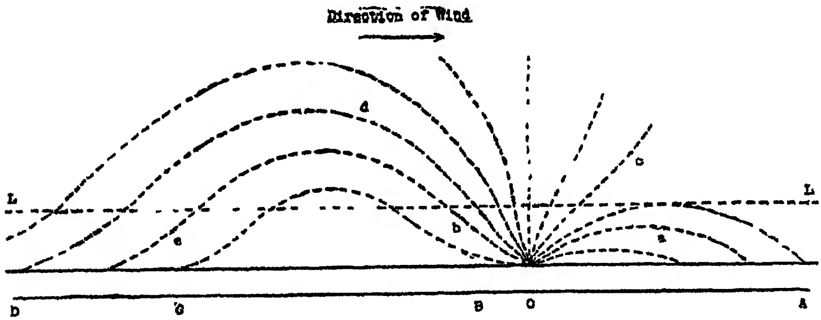


FIG. 5.
Diagram showing the course of the sound-rays when the velocity of the wind first increases and then decreases as the altitude increases.

of these essential data, all that we can do is to make certain assumptions, which, so far as we know, do not differ very widely from the truth. Taking certain values which are of not uncommon occurrence for the rates of increase and subsequent decrease of wind-velocity, and for the height at which the change takes place, Mr. S. Fujiwhara, a Japanese mathematician, finds that the outer boundary of the silent zone would lie about seventy miles from the source of sound, a distance which does not differ materially from that observed in some explosions.

The atmospheric conditions are not always, perhaps not often, so simple in character as those described above. Sometimes the wind remains nearly steady in direction and velocity up to heights, it may be, of four or five miles or even more. Or the velocity of the wind may decrease upwards; and, in both cases, we could not expect to find a silent zone developed. Frequently, also, as may be seen by watching the movements of the

higher clouds, great changes and even reversals in the direction of the wind may take place in the lower or upper regions of the atmosphere. Such conditions favour the production of a silent zone. On the whole, taking all the various conditions into account, Mr. Fujiwhara has shown that a silent zone should be manifested quite as frequently as a continuous sound-area.

Mr. Fujiwhara has also considered the relations which exist between the state of the weather and the forms of the sound-areas in Japan. Under normal winter conditions, he finds that the sound-areas should be single, but that under abnormal conditions, when cyclones exist to the north, east, and south of the volcano, there might be a silent zone. In summer, on the other hand, the usual conditions would favour the production of a silent zone, and the exceptional conditions the occurrence of a continuous sound-area.

NOTES

Awards for Medical Discovery.

Science Progress has frequently referred to the need which exists for some form of awards for successful medical investigations. In our last two numbers it was stated that a strong Conjoint Committee of the British Science Guild and the British Medical Association had been formed to consider the matter, and that this Committee had asked Sir Ronald Ross, one of its members, for a report. The report was duly presented, and was fully accepted by the Committee on December 22, 1919, with the emendation that the State should give £20,000 a year for the awards suggested, instead of the £15,000 advised by Sir Ronald Ross, and with some small adjustments. The final report of the Committee will be found in *Nature*, January 8, 1920, and in the *Lancet*, January 10.

We now place upon record here, for convenience of reference, Sir Ronald Ross's original report to the Committee, together with copies of two letters to *The Times* of about the same date in which he explained the subject to the general public. These are placed first because they afford a brief purview.

From "The Times," December 10, 1919

Sir,—Now that the war has awakened the nation to the value of medical discovery, considerable attention is being given to the question as to how best to stimulate still quicker advance in the future. The subject is of importance to every living person, and indeed to generations unborn; and I therefore hope that you may be able to spare me some little space in which to set forth my own analysis of the matter for consideration by others.

The object is to encourage successful research wherever possible. Not one, but two ways of doing this are open to us. The first is to help those who are willing to undertake research, but cannot afford to do so, by providing laboratory accommodation, working expenses, and part-time or whole-time salaries. Much in this line has already been done here and abroad by establishing research-scholarships, and even fully-paid appointments, either confined to research, or with only small teaching duties attached; and since 1914 the Medical Research Committee has been expending in this way an annual grant of about £50,000 provided by Parliament—with results that are well known. Similar grants have also been given for many years past by the Royal Society and other learned bodies.

Everyone will approve of this expenditure; but it is made on the principle of payment for prospective benefits. Research is generally a lottery—it may or may not succeed, and the money thus granted may or may not be lost. Hence the Committees concerned seldom (and rightly) allot the funds except for researches on simple and straightforward questions admitting of immediate reply, thus greatly limiting the fields of inquiry. Moreover, the grants are often short-timed, and are sometimes withdrawn just as good results are beginning to emerge; while, lastly, even enthusiastic workers often feel that they are losing time which might be expended much more

lucratively for themselves in professional practice, and therefore desert their investigations before completing them.

Now, nearly all the greatest medical discoveries were made on quite another basis. The investigations were not subsidised in any way; they were not originated or directed by committees; they were not limited to a year or two; they were often entirely changed in course and objective by some chance observation; and they sometimes lasted for half a lifetime before success was achieved. In fact, most of the greatest medical discoverers were practising medical men who conducted their researches during their own leisure and at their own expense—for example, Küchenmeister, Jenner, Simpson, Lister, Koch, Laveran, Bancroft, Manson, Bruce, Mackenzie, and a score of others. Much of the best work has been due to mere clinical observation, and more to hard thinking; both of which require neither laboratories nor subsidies. I submit, then, that it would be wise for my country to encourage this great kind of work also; by paying, not only for prospective benefits, but for benefits actually received.

Look at it from another point of view. We have some 40,000 medical men, many of whom possess the brains, knowledge, time, and opportunity for research. Some of them already make such clinical observations as tend to improve practice and to enhance clinical reputation. But the more difficult medical investigations usually require an immense expenditure of time, energy, and thought, which, if bestowed upon clinical practice, would harvest considerable pecuniary reward, but which, even if they result in capital discoveries, may notoriously bring in no professional remuneration whatever. For example, Jenner's researches on vaccination lost him his practice, and would have ruined him, but for the fact that Parliament was in those days (1802 and 1807) wise enough to pay him compensation. Similar cases exist to-day—without the compensation. I have often heard it asked, What would not the world give to him who should discover the cure of consumption or cancer? My answer is—Nothing. Honours perhaps; but similar or greater honours are given for professional eminence of a much less important class; and people usually enter the medical profession in order to make a living. Is it to be wondered at, then, that medical men hesitate before plunging into abstruse life-long labours which may never succeed at all, and which may ruin them even if they do succeed? And I may add, is it a wonder that people still continue to die of consumption and cancer?

My suggestion is that Parliament would now be wise to give (in addition to its grant to the Medical Research Committee) the comparatively small sum of, say, £15,000 a year to be divided into life-pensions for distinguished medical discoveries already made, or which may be made in future. This sum would provide ten pensions of £500 a year each, and ten of £1,000 a year—sufficient to confer, not wealth, but independence and provision for old age; and would help to bring the whole profession (as well as other biological workers) into a national scheme of research, which has as yet been only partly created by the grant to the Medical Research Committee. The awards would also discharge a moral obligation which the public owes to professional men who confer benefits upon it without other professional payment. I do not enter into details here because I am placing them before a Conjoint Committee of the British Science Guild and the British Medical Association which has been formed to consider the subject, and of which I am a member; but I should like to call public attention to the scheme in general outline.

I think, also, that the precedent of Jenner ought to be resuscitated, so as to allow of compensation by Parliament for direct pecuniary loss incurred by a worker during the progress of successful but unremunerative investigations; and have indeed made efforts in this direction myself, but hitherto without result.

Lastly, we should, I think, plead for similar awards for great work in literature, exploration, and indeed all branches of Science and Art; that is, for work which has been of value to the general public without being remunerative to the worker himself. The nation allows patents, and rewards soldiers, sailors, and inventors. If this principle be sound, it should be extended to other classes of work; and if genius be one of the greatest assets of a people, then something should be done, more than is done at present, to direct it towards the noblest objects.

I am, sir, yours faithfully,

RONALD ROSS.

December 8, 1919.

From "The Times," December 24, 1919

SIR,—My scheme for the encouragement of medical discovery by State awards has received considerable attention since you were so good as to publish my letter on the subject in *The Times* of December 10. Many of your readers will therefore, I believe, be glad to learn that the scheme has been approved by the Conjoint Committee to which I referred; and is being submitted to the proper authority for action; and you will doubtless receive the Committee's report in due course.

A contemporary of yours, while it gives welcome support to my suggestion, doubts whether the present time is appropriate for asking Government for grants. But surely this is supposed to be the great period of reconstruction, and matters of life and health should, I think, head the programme. I asked for £15,000 a year to be distributed in life-pensions among men who have actually benefited the Empire by important medical discoveries: one-thousandth part the sum proposed to be allotted for aeronautics; one-twentieth that which Parliament grants to its own members, or provides for political and legal work; one-quarter what it gives as a subsidy for current medical investigations; and less than the income of many wealthy individuals.

But I argue that there is also a point of honour involved. Is it right for a great empire to accept the wide benefits of medical discovery from private professional men without making the smallest attempt to give them professional payment in return? If a patient owes a fee to his doctor, surely the nation owes similar remuneration to men whose investigations improve medical or surgical practice on a large scale. Yet since the time of Jenner this nation has given nothing whatever for such work. Like the man in the parable, the world has often fallen among thieves; but after that Good Samaritan, the medical investigator, has bound up its wounds, the world forgets all about him, and does not even trouble to pay him back the two pence he advanced to the innkeeper! My whole plea is that the world would be wiser to pay that little debt in future—as a premier obligation.

I am, sir, yours faithfully,

RONALD ROSS.

December 22, 1919.

A REPORT ON REWARDS FOR MEDICAL DISCOVERY

Presented by request to the Conjoint Committee of the British Medica Association and the British Science Guild on Awards for Medical Discovery, by Colonel Sir Ronald Ross, K.C.B., K.C.M.G., F.R.S. (Dated December 1, 1919).

I. Definitions

Medical Discovery may here be defined as being:

(1) The ascertainment of new facts or theorems bearing on the human

body in health, and the nature, prevention, cure, or mitigation of injuries and diseases of human beings.

(2) The invention of new methods or instruments for the improvement of sanitary, medical, and surgical practice, or of scientific and pathological work.

II. Reasons for Rewarding Medical Discovery

These are:

(1) To encourage medical investigation.

(2) To discharge a moral obligation incurred by the public for its use of private effort.

III. Various Possible Types of Rewards

(1) Titles and honours given by the State, by Universities, and by other Public Bodies.

(2) Prizes and Medals.

(3) Patents.

(4) Promotions and Appointments.

(5) Pecuniary Awards by the State.

IV. General Principles of Assessment

It will probably be agreed that in the interests of the public all Medical Discoveries should, if possible, receive some kind of acknowledgment or recompense. But in view of the very variable conditions, nature, and effects of particular investigations, it will often be difficult to assess the kind of recompense most suitable for each. I will therefore now set down my own analysis of the subject for consideration by the Committee.

In the first place we should distinguish carefully between *Compensation* and *Reward*. By Compensation I mean an *act of justice* done for the purpose of reimbursement of losses; by Reward an *act of grace* in appreciation of services rendered. A plea for compensation is of course stronger than one for reward only.

The following different cases should next be considered:

(*a*) Discoveries involving pecuniary or other loss to an investigator, either by direct monetary sacrifice, or by expenditure of time, or by diminution of professional practice, without corresponding pecuniary gains. A great example is that of Edward Jenner, who occupied himself so closely with the investigation of vaccination against smallpox that he lost most of his medical practice, and also considerable sums in expenses. The plea for compensation in such cases is unanswerable; and in 1802 and 1807 Parliament fully acknowledged its obligations under this head by giving Jenner compensation in two sums of £10,000 and £20,000.

(*b*) Discoveries which have increased the professional emoluments of the investigator by enhanced practice, University or other appointments, or other means. Such are, frequently, improvements in surgical operations or medical treatment, which often lead to large practice. Another case is that of serums, etc., which have been patented and put on the market. Here compensation cannot be demanded, and pecuniary awards will, I think, be generally held to be unnecessary. On the other hand, honours are often, and justly, bestowed upon such work.

(*c*) Discoveries which involve neither gain nor loss to the investigator. This class includes most of the good, and sometimes great, clinical, pathological, and sanitary discoveries achieved in the world. Here also compensation can scarcely be demanded, and honours are already often given; but pecuniary awards should, I think, be sometimes bestowed as an act of

grace when the value of a discovery to the public (or to a Government) greatly exceeds the emoluments of the investigator, and I consider that this principle should hold even in the case of men who were directly paid in advance for the researches which led to their discoveries—especially when such payment was (as usual) small, and the resulting discovery great.

The following particular cases, which sometimes occur, should be specifically noted:

(1) Men who have refused lucrative posts in order to complete their researches.

(2) Men who have refused to patent their work for fear of limiting its application.

(3) Men who have carried out investigations for Governments for little or no payment, on patriotic grounds.

The following considerations must generally be borne in mind:

(*a*) Honours (which are always much esteemed) are usually given as much (or more) for clinical success as for medical discoveries, though the latter possess a far wider influence and application than the former do.

(*b*) When given for clinical work or for discoveries under Class B, honours often confer distinct pecuniary advantages by enhancement of practice; but for discoveries under Classes A and C, they have no such effect, and I have even heard of cases where they tend to reduce emoluments by unfitting recipients for certain posts.

(*c*) Most people enter the medical profession (at considerable expense), not only from altruistic motives, but also in order to make a living; and it is usually only at a later period that they take up scientific investigation—either from a sense of duty, or from predilection, or merely because a good opportunity offers. When, therefore, a man finds that his scientific work, however successful and important it may have been, has actually yielded him less emolument than he might have obtained from ordinary clinical work, he feels naturally disappointed; and his experience prevents young men of ability from following his example, and therefore tends to check the prosecution of studies which are of the highest value for humanity.

(*d*) In the public interest, then, I think that the Committee should insist upon the principles:

(1) That no medical discovery should be allowed to entail financial loss upon him who has made it.

(2) That the compensation or reward which he deserves should be assessed as being equal to the difference between the emoluments which he has actually received, and those which he might reasonably expect to have received if he had devoted all his time to a successful clinical practice.

This is obviously the principle which was accepted by Parliament in the case of Jenner in 1802 and 1807—to whom, however, I think Parliament gave both *compensation* and *reward*.

Additional reasons for insisting upon this principle are:

(1) That few medical discoveries are patentable.

(2) That such discoveries seldom give good grounds for promotion or for administrative appointments in the public services.

V. Particular Assessments

Whether a particular discovery should receive a large or small assessment will depend, not only on the considerations given above, but also on the following:

(1) *Width of Application.*—For example, the work of many of the older anatomists, physiologists, and parasitologists, of Pasteur and of investigators of immunity, has affected most recent discovery. Discoveries on widespread diseases, such as the work of Lister, of Laveran, and of Koch, are

often, though not always, more important than those on more limited maladies.

(2) *Difficulty of the Work Done.*—For example, the solution of a difficult problem requires more study, and therefore deserves more recompense, than a lucky chance observation.

(3) *Immediate Practical Utility.*—A strong plea for State remuneration can be made on behalf of cases of this kind, unless they come under Class B. It is strange that at present they never receive it—while academical recognition is also often not forthcoming for them. (Of this I know many instances.)

(4) *Scientific Importance.*—Discoveries which are not of present practical utility may become so at any moment, and should obviously be included in the scheme, if they are sound and of wide application.

Medical discoveries made by persons who do not themselves belong to the medical profession should, I think, be included in all schemes of reward.

Of course each case must be judged on its merits, and the assessment will not always be easy.

VI. State Awards for Medical Discovery

Honours, prizes, and medals, being bestowed by H.M. the King, or by public bodies and learned societies, are acts of grace which are generally given after much consideration; and I presume that the Committee does not propose to consider them. But the subject of *pecuniary awards* lies entirely within its province.

Considerable pecuniary awards are already sometimes given in foreign countries by various societies as the result of private generosity. The leading example is that of the Swedish Nobel Prizes, amounting to about £8,000 each. Of these, four are or may be bestowed annually—namely, one each for Medicine, Chemistry, Physics, and Literature; and the Nobel Committee has, I calculate, distributed about half a million pounds in this way throughout the world during the last eighteen years. But one such annual prize does not suffice for the whole of medical discovery.

These prizes are given upon the principles of payment for *benefits already received*. But during the last few years the British Government has disbursed an annual grant of about £60,000, under the Medical Research Committee, for subsidising *investigations in progress*, authorised by the Committee and carried on by workers selected by it. This grant does not remunerate discoveries *already made*, but proceeds upon another principle, namely that of payment for *prospective benefits*.

I think that both principles are sound. But they apply to two different classes of research, and are indeed complementary of each other. Payment for prospective benefits is "good business" only when some return is almost certain; and for this reason subsidised researches must almost always deal with simple and straightforward questions, admitting of immediate experimental reply. But, as a matter of fact, most of the greatest medical discoveries were built upon a much more speculative and uncertain basis; and were achieved by men who were engaged at the time upon their ordinary medical duties in addition to their investigations, and who neither sought nor received subsidies for those investigations—as, for instance, Küchenmeister, Jenner, Simpson, Lister, Koch, Laveran, Bancroft, Manson, Bruce, Mackenzie, and a score of others who have so greatly improved medical practice. Surely the State should encourage this class of investigation also—partly because it costs the State nothing in the doing, and partly because it seems to achieve the greatest results. And there is only one way to encourage it—by paying for discoveries when made. Payment for benefits received is always not

only "good business," but also a moral obligation. There are at present hundreds of medical men and others in this country who possess the knowledge, the brains, and the opportunity for private independent discovery, without subsidies, but who do not attempt it because medical research does not pay even when brilliantly successful. Let these men also be brought into the fold of research by offering them some small reward when they succeed.

I would therefore suggest that, in addition to assisting investigations in progress, it is proper for every civilised State to remunerate those of its subjects who have already conferred the benefits of medical discovery upon it—just as it is proper for a wealthy patient to pay his doctor. And this policy will be not only an act of justice, but an act of wisdom.

My proposals are, in detail:

(1) That Parliament should resuscitate the precedent of Jenner by paying *compensation*, when due, for losses incurred in achieving medical discoveries. The procedure should be, as in Jenner's case, by direct petition to the House of Commons through the Chancellor of the Exchequer; and I am informed by high legal authority that nothing has happened since Jenner's time to invalidate such right of petition, which still belongs to all His Majesty's subjects. Possibly some cases might be open to adjudication by the Royal Commission on Awards to Inventors.

(2) That Parliament should provide an annual sum, say of £15,000, for life-pensions to be given as rewards to such of His Majesty's subjects as have made worthy medical discoveries, and to the widow of such after his death; there being, say, ten pensions of £1,000 a year each, and ten pensions of £500 a year each at disposal, all the pensions being free of income-tax.

I think that such pensions would be preferable to donations in capital; and that the sums suggested would be sufficient—because men of science do not desire wealth, but only such independence as will enable them to employ their talents in the manner they think best.

The procedure of allotment should be similar to that used for the Nobel Prizes, and for the honours and medals of learned societies; that is, full particulars of the work of all applicants should be kept and considered.

Parliament grants large subsidies to soldiers and sailors, has appointed a Commission to consider awards to inventors, and allows patents. It will not, I am sure, complain if the Medical Profession, which has done so much for the nation during the war, now asks for some similar consideration.

VII. Conclusion

The Committee requested me also to prepare a statement embodying a list of worthy medical discoveries as supplied to me by its members. I purpose to submit this list *separately and confidentially* as soon as it is ready. The present part of my report deals only with the general principles of the subject, and therefore might be considered first.

(Signed) RONALD ROSS.

THE CONJOINT COMMITTEE

For the British Science Guild
Sir Alfred Keogh, G.C.B.
Sir Ronald Ross, K.C.B., K.C.M.G.
Prof. W. Bayliss, F.R.S.
Sir Richard Gregory.
Lt.-Col. O'Meara, C.M.G.
Dr. D. Somerville.

For the British Medical Association
Sir T. Clifford Allbutt, K.C.B., F.R.S.
Prof. Benjamin Moore, F.R.S.
Prof. J. S. Haldane, F.R.S.
E. B. Turner, F.R.C.S.
Dr. R. T. Leiper.

Science Masters' Association (G. H. J. Adlam, M.A., B.Sc.).

The annual meetings of the Science Masters' Association were held in January at the London Day Training College. The effect of the recent changes in the rules, removing restrictions as to membership of what was formerly the Association of Public School Science Masters, was at once apparent, for nearly two hundred attended the various meetings. The Association is much strengthened by its larger and more representative membership, and thus another step has been taken towards the elimination of that misleading term "public school," which for a long time has been little more than a catchword, and has tended to create an artificial distinction between men belonging to the same profession and doing the same kind of work.

The President (Mr. W. W. Vaughan, Master of Wellington College) chose as the subject of his address "The Responsibilities of Science Teachers." During the years of the war, many educational ideals had to be set aside; technical efficiency was all-important; the need was urgent, and consequently the shortest and quickest road had to be taken; instruction rather than true education was to a great extent the order of the day, and systematic teaching had to give way to short intensive courses for special purposes.

The habits acquired even in four or five years are not easily shaken off, and there is a danger that some teachers, particularly Science teachers, may be tempted to respond to the noisy clamour for technical efficiency, forgetting that at the basis of national prosperity there lies something which is higher and greater than this, and which cannot be measured by the volume of trade returns.

What the ideals of education are, and how constant they have remained throughout the ages, was brought out very forcibly and clearly by four quotations given by Mr. Vaughan. The first, from Plato, is very appropriate, particularly at a time such as the present, when lawlessness is rife and when deeds of violence figure so prominently in the daily papers: "Man, if he enjoys a right education and a happy endowment, becomes the most divine and the most civilised of all living things; but he is the most savage of all the products of the earth if he is inadequately and improperly trained." The second quotation was from Milton's letter to Master Samuel Hartlib: "A complete and generous education fits a man to perform justly, skilfully and magnanimously all the offices both public and private of peace and war." To Milton, of course, a generous education meant fear of God and love of the Classics; technical instruction, apart from education, probably did not enter into his thought; yet "justly, skilfully and magnanimously" covers all requirements, because the greater includes the less.

The third quotation was from Dr. Johnson: "Whatever makes the past, the distant, or the future predominate over the present advances us in the dignity of thinking beings." The last quotation, from the speech of a London schoolmaster delivered only a few years ago, sums up in few words the most exalted aim of education: "Whatever subject we may be called upon to teach, our real object is always the same: to give the soul a chance."

With these clear and indisputable precepts in mind, schoolmasters would hardly consent to a course of action which, as the President said, would be tantamount merely to exchanging the God of Mammon for the God of War.

Organic Chemistry.—The teaching of Organic Chemistry in schools was discussed, and it was pointed out that, of the boys who leave school having "done Chemistry," only a few have any knowledge of the carbon compounds, since the division between "organic" and "inorganic" is still retained just as rigidly as it was in the days when belief in a "vital force" existed. This is doubly unfortunate, for many of the common commodities of life are

carbon compounds, and very little can be taught about structure in inorganic Chemistry, while in organic Chemistry it cannot well be avoided.

The cause of the omission is due mainly to the unintelligent nature of examination schedules, in which all other considerations seem to be sacrificed to that of the examiner's convenience or his supposed capacity for the work he has undertaken. There is no evidence to show that anything more than the gross structure of these schedules has ever been considered, or that the claims of, say, soap and glycerol have ever been weighed against those of the oxides of nitrogen.

Biology.—The amount of Biology taught in schools at the present time is not considered to be adequate. In some of the larger schools Botany and Zoology are well taught to a few senior boys who are going to be doctors, but as a rule, the rank and file are not taught even the simplest facts about the lives of plants or animals. It cannot surely be urged in mitigation of this that ignorance is bliss. Prof. Hickson, of Manchester, pointed clearly to the main cause of the omission. The investigators of the Secondary Schools Examination Council state in their report that Natural History and Zoology are unsuitable subjects for a pass with credit in Science. It comes to this then, that any subject, however important and necessary, is to be ruled out if it does not lend itself to experimental treatment and to the crude mechanical process of examination. University professors, who compile these wonderful examination schedules, say, of course, that Biology is more suitable for the University course; there the matter ends, and the great majority of people go through life totally ignorant of the mechanism of their own bodies.

Laboratory Management.—Two problems in laboratory management—the training of assistants and the present high cost of apparatus and material—gave rise to an interesting discussion. In some schools it is the practice to engage boys at a small wage straight from the elementary schools. This is not fair to the boys, for, under the circumstances, the work of laboratory assistant becomes a " blind alley " occupation. The training which they get is not generally sufficiently good to ensure further progress in the same profession, and their knowledge is of little practical value in following other occupations. From the school point of view the arrangement is unfortunate, for the boys leave just when they are capable of doing good work. If boys must be employed, it seems only fair that some arrangements should be made to give them a definite training; even then the Science master is training his assistants not to keep them, but to lose them. It would be more satisfactory to take up the question of the status of the laboratory assistant, to employ men (men who have done a term of service in the Navy make excellent laboratory assistants), to press for a living wage, and to urge the advisability of including the trained laboratory assistant in the pension scheme.

Cost of Apparatus.—The high cost of apparatus and material has become a very severe handicap to the teaching of Science in schools, and threatens to be even a greater obstacle than the former " neglect of Science." After five years of war and the services of inefficient substitutes for the regular Science master, most school laboratories are sadly in need of new apparatus; the sum of money allocated for upkeep has not been increased in anything like the same proportion as prices, and, what is more, pupils are coming to the Science classes in increasing numbers. It comes to this, that makeshifts have to be adopted, and the character of the practical work suffers in consequence.

Preparatory School Science.—If anything worthy of this title existed in the majority of these schools, it would be most helpful to those who have to carry on the work in secondary schools. It cannot be done, however. Science does not pay in the Entrance Scholarship Examination, and has no

weight in the Common Entrance Examination; yet preparatory-school masters are anxious to teach Science, and preparatory-school boys, like most others, are eager to learn something, particularly about the mechanical sciences. It is clear that some radical alteration ought to be made in the syllabuses of these two examinations, but probably nothing less weighty than a Royal Commission could bring about the change.

Separation of Laboratory and Class-room Courses.—In the opinion of many competent teachers, laboratory work has rather tended to become a fetish, for there are some who go so far as to say that the only work of any value in Science is that which is done in the laboratory by the boy himself. This does not say much for the inspiration which the competent teacher should be able to impart, but it does tend to relegate to the specialist part of the curriculum sciences like Biology, which do not lend themselves to practical work done by the younger pupils, while the simple facts of Astronomy are for the same reason not included at all. Hence it comes about that school Science of the day is practically confined to Chemistry and Physics—useful and fundamental sciences, no doubt, but limited in their outlook. The only remedy for this is the reinstatement of the lecture-demonstration for those branches of Science which are best dealt with in that way. There would still be ample opportunity for inculcating the scientific method and habit of thought.

Some New Botanical Memoirs (T. G. H.).

Botanical Memoirs is the title of a new botanical publication initiated and edited by Mr. A. H. Church, of the Botany School, Oxford. Published by the Oxford University Press, the Memoirs are complete in themselves and appear at irregular intervals. Such a mode of publication has much to recommend it, the chief disadvantage being a possible restricted circulation unless advertisement be employed. The following notes are little more than an indication of the contents of each number: it is hoped to publish reviews of the more important memoirs in the future.

No. 1. *The Building of an Autotrophic Flagellate.* A. H. Church, 1919. (Pp. 27. 2*s.* net.) This is the most important of those memoirs which have appeared. An essay in speculative biology: vigorous, deeply interesting, highly suggestive. Problems concerned with the evolution of life from the point of view of botany have not been better formulated. The author has achieved no small measure of success in his attempt "to indicate the more obvious requirements of the problems of the evolution of such an organism as an autotrophic flagellate of pelagic plankton, comparable with that from which the ancestral forms of the Phæophyceæ may have been derived."

No. 2. *Gossypium in Pre-Linnean Literature.* H. J. Denham. (Pp. 24, 4 text figures. 2*s.* net.) An interesting account of the references to the cotton plants in literature from Herodotus to Linnæus. The tracing of the literary history of a single genus has much to recommend it; as the author truly remarks with reference to his subject, "The history of *Gossypium* as a genus epitomises the history of Botany." The text figures are not good; better reproductions of the woodcuts of Fuchs, Matthiolus and others ought to have been possible.

No. 3. *Thalassiophyta and the Sub-aerial Transmigration.* A. H. Church, 1919. (Pp. 95. 3*s.* 6*d.* net.) In this memoir Mr. Church returns to an earlier love, the Algæ. The essay is a consideration of the probable evolution of land vegetation from marine, and is a valuable contribution. Certain of the views put forward were held by the late Dr. Sarah Baker, who derived them from her study of the brown seaweeds of the salt marsh.

No. 4. *Elementary Notes on Structural Botany.* A. H. Church, 1919. Twelve Lecture Schedules. (Pp. 27. 2*s.* net.)

No. 5. *Elementary Notes on the Reproduction of Angiosperms.* A. H. Church, 1919. Ten Lecture Schedules. (Pp. 24. 2*s*. net.)

We do not much like the circulation of lecture notes: although of use and sometimes of value to the student, they inevitably suggest cramming, and call to mind the evils connected with "payment by results." In the present instance, however, it would appear, from the note on "Minimum Botany," that the author's object is to illustrate the inadequate amount of time allocated to Botany for medical and forestry students by the University authorities. The author concludes: "At this time of the world's history it is remarkable that in a university of primary importance, the teaching of Plant-biology should be of such a meagre description.

"The fundamental laws of all living organism, including extensions to sociology and theology, are based on biological problems; and biology, or a knowledge of the laws of life, in some form, should be part of the mental equipment of every educated person. It is again to Botany, as dealing with the primary life of the world—the great independent kingdom of autotrophic vegetation, the base of the pyramid of life, whether in the sea or on land, and on which we ourselves, as animals, are still dependent for our supplies of food and energy—that one must look, not only for the interpretation of the primary laws of existence, but also for the broader views rendered possible by the wider range of plant-races, as expressed, for example, in the elementary mechanism of phyletic progression."

New Scientific Periodicals.

We are glad to welcome several new or comparatively new publications. Mr. Murray is bringing out a monthly popular journal of knowledge called *Discovery*, edited by A. S. Russell, M.C., D.Sc., under a trust of which Sir Joseph Thomson is one of the members. The work is likely to interest readers very widely, and the first number contains many good articles. The Macmillan Company, New York, started the publication last October of the *Journal of Industrial Hygiene*, which will evidently be valuable to all Health Officers and other public workers. Last year the Commonwealth Institute of Science and Industry (Dank's Buildings, Bourke Street, Melbourne) began their monthly journal; and, lastly, our Medical Research Committee continues the invaluable summaries of medical literature which it issued during the war, in the form of monthly abstracts and reviews called *Medical Science*, published for the Committee by the Oxford University Press —a most ably conducted work. We wish them all success.

A Simple Apparatus for High-power Photomicrography (R. E. Slade, D.Sc., F.I.C., and G. I. Higson, M.Sc., A.I.C.).

The apparatus has been designed for the rapid production of photomicrographs in the course of an investigation on photographic emulsions. A magnification of 2,000 has been found most successful for the work in hand.

The apparatus is mounted on a block of ash three feet long, three inches thick, and eight inches wide. This optical bench rests on two partially inflated air-cushions, and is prevented from falling sideways by a rubber ball. This arrangement for eliminating vibration, which was suggested to the authors by Prof. Bragg, has been entirely successful.

The apparatus proper consists of a microscope in the horizontal position which is pushed home against stops fitted on the bench. The source of illumination is a 100 c.p. Pointolite lamp on a universal joint for centring. This lamp is contained in a light-tight box between which and the substage condenser of the microscope is a light-tight connection provided with a

slide for holding a light-filter. There is no optical system or heat-absorbing cell interposed between the Pointolite lamp and the condenser, as these are quite unnecessary with this source of light. No camera is used, as the apparatus is always worked in a room illuminated with red light. The plate is supported in a plate-holder rigidly fixed to the bench, usually about one foot from the eyepiece of the microscope. Focussing is done direct onto a piece of white card already in the plate-holder. A simple, specially designed vibrationless shutter is fixed to a support independent of the bench. After focussing, this shutter is brought down just in front of the eyepiece of the microscope.

In practice a green light-filter is used with green sensitive plates. Green light is used because the eye has the maximum sensitivity in this region, and therefore focussing is facilitated. A process plate is used, because it gives the best definition. The exposure required with a magnification of 2,000 diameters obtained by means of a 2 mm. oil-immersion objective × 14 eyepiece is from 5 to 10 seconds.

Octaval Notation for Inch Fractions

A rule for engineers' precision measurements has just been issued which embodies a proposal for a logical notation for the inch binary fractions. Such notation is not new, but for the first time is put into a practical form for the workshop.

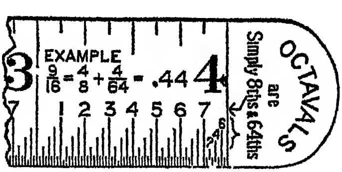
It is based on the observation that although (the tendency of computators being towards decimals) all micrometers and fine measuring instruments have been made with decimal divisions of an inch, the instinct of the craftsman or mechanic is towards a binary division into halves, quarters, etc., down to 64ths.

In practice the vast majority of English and American engineers standardise their machine parts, down to 64ths, in such binary fractions, although for their measurements they have to use decimal fractions, no other measuring notation having been provided.

In place of the radix of 10 used for decimal notation, this rule (and proposal notation for finer measurements) uses a radix of 8.

Just as in decimals $0{\cdot}234 = \frac{2}{10} + \frac{3}{10^2} + \frac{4}{10^3} = \frac{2}{10} + \frac{3}{100} + \frac{4}{1000}$,

so in octavals, $0_{\circ}234 \quad = \frac{2}{8} + \frac{3}{8^2} + \frac{4}{8^3} = \frac{2}{8} + \frac{3}{64} + \frac{4}{512}$.



The rule here illustrated divides the inch into 8 equal parts numbered 1 to 8, and each of these parts is sub-divided (by stepped divisions for the even values) into 8 parts again, making the finest divisions 64ths. The "staircase" divisions make it easy to know the value of each sub-division by its length.

The "pip" $_\circ$ which takes the place of the decimal "point," is the printer's degree mark upside down, and obviates confusion with decimal fractions.

With this notation all binary fractions down to 64ths are expressed in two figures accurately. A table of these octaval equivalents, and some

simple instructions for adding, multiplying, etc., and also for converting other fractions to octavals, accompany the rule.

The following illustrates their simplicity in comparison with decimal fractions, which have the disadvantage that each successive halving adds a decimal place.

Vulgar.	Octaval.	Decimal.
$\frac{3}{8}$	$_{0}3$	·375
$\frac{3}{16}$	$_{0}14$	·1875
$\frac{3}{32}$	$_{0}06$	·09375
$\frac{3}{64}$	$_{0}03$	·046875

The makers of this rule (Messrs. Watkins Meter Co., Hereford) issued it in steel, 4 inches long, and have one 8 inches long almost ready. They propose issuing in the same notation micrometers, gauge-blocks, and vernier calipers for precision work, as they find this notation makes it possible to mark and figure each division, which was not possible before with the much-used binary fractions. For example, a calipers with the inch divided into 8 numbered parts, and with a vernier scale with 8 numbered parts occupying 7 of the $\frac{1}{8}$-inch divisions, not only reads down to 64ths with large divisions, but the first figure of the octaval value is read directly on the main scale, and the second figure on the vernier scale. It becomes easy to construct micrometers with a separate scale for each place in the octaval fraction. Advantage can also be taken of the perfection of a binary series for addition. In a set of precision blocks or thickness gauges of an octaval series, only three blocks (4, 2, 1) need be provided for each place of octavals, as from these three figures, values from 1 to 7 can be made up; there is also the advantage that the value for each octaval place is taken from a separate group of blocks, and the different places are never mixed up as in decimal engineering measurements. The following is the complete set of thickness gauge-blocks from which any measurements can be taken in 64ths ($_{0}01$) from $\frac{1}{64}$ to $1\frac{63}{64}$ inch ($1_{0}77$).

$$1,\ _{0}4,\ _{0}2,\ _{0}1,\ _{0}04,\ _{0}02,\ _{0}01.$$

For example, the value $1\frac{15}{32}$ inch, which is $1_{0}36$ in octavals, is made up with blocks as follows: 1 for the integer, $_{0}2$ and $_{0}1$ for the first figure in the fraction, and $_{0}04$ and $_{0}02$ for the second figure of the octaval fraction.

The 3rd place in an octaval fraction is $\frac{1}{512}$, and half of this being so near the familiar thousandth of the mechanic makes the substitution of octavals for decimals in the "limits" for fine mechanical work quite easy.

It is only proposed to apply this notation to fractions of a unit. All to the left of the "pip," that is all integers, remain in the ten grouping.

The Institute of Physics.

The need has long been felt for a corporate body analogous to the Institute of Chemistry which could represent the profession and strengthen the position of workers engaged in physics. The professional physicist is scarcely recognised as such. During the war, in a certain Government Department, when the need arose of appointing research workers in physics, it was found necessary to style them research chemists. Probably the physicist himself is to some extent responsible for this state of things. He has drifted toward the academic rather than the technical side of his subject, and has left the applied side to engineers on the one hand, and to chemists on the other. The war has taught him that there are technical things with which he is most qualified to deal, and he is now stirring himself to get his claims recognised.

Only the co-operative action of all physicists in the country will succeed

in securing this recognition. The new Institute of Physics is created toward this end. The movement began with members of the Council of the Physical Society of London, who obtained the support of that Council, and also those of the Faraday Society and the Optical Society. Representatives were appointed by all these councils to draw up recommendations for the constitution of the new Institute. Many deliberations have proceeded during the last two years; but it was decided to make no public announcements until the arrangements should be complete. This is now the case; and probably before this notice appears the Institute will be a fully incorporated body.

Although a perfectly independent body, the desire is to secure the co-operation and help of other societies which deal directly or indirectly with physics. Societies that participate in the scheme secure representation upon the Board of the Institute, and at the same time obtain certain privileges for their members. The three societies named above are the first participating societies, but it is expected that other societies will also come in. The aim will be eventually to obtain a central building where the meetings of all the participating societies may take place, and where their libraries may be stored and made easily accessible.

There will be two kinds of members. All members of participating societies may be elected as *Ordinary* members without payment of any fee; others will pay a fee. They will have no vote except on questions involving any proposed change in subscriptions or entrance fees. The management of the Institute is vested in the *Corporate* members, who alone can serve on the Board and vote in general meeting on general questions affecting the Institute. These Corporate members are of two kinds: Associates (entrance £1 1*s*., annual subscription, £1 1*s*.) having one year's experience in research or manufacture as a physicist; Fellows (entrance £2 2*s*., annual £2 2*s*.), having five years' similar experience. The passing of an examination is required from associates excepting when other sufficient evidence of fitness is forthcoming.

Arrangements have been made by which members of the Institute belonging to two or more participating societies pay reduced subscriptions to these societies.

In all cases a high standard of professional knowledge and conduct will be required from members, and it is confidently hoped that the diplomas and titles granted will have substantial value to the recipients.

The creation of the new Institute has already received support from the leading physicists and directors of industrial concerns in the country. A guarantee fund has been raised more than sufficient to carry it through the first few years. Sir Richard Glazebrook, K.C.B., F.R.S., has kindly consented to be the first President, and Sir Robert Hadfield, F.R.S., the Honorary Treasurer. Further information can be obtained from the Honorary Secretary, Alfred W. Porter, D.Sc., F.R.S., or from the Secretary, F. S. Spiers, 10 Essex Street, Strand, W.C.2.

Notes and News.

The New Year Honours List contained only five names that need be recorded here. Sir Bertrand Edward Dawson, Dean of the Faculty of Medicine in the University of London, was created a Baron; Sir Richard T. Glazebrook was awarded a K.C.B.; Knighthoods were conferred on Prof. A. Schuster, F.R.S., lately Secretary to the Royal Society, and James Kemmal, a Fellow of the Royal Society of Edinburgh; while Dr. F. S. Lister, Research Bacteriologist to the South African Institute for Medical Research, was raised to the degree of Knight Bachelor.

The list of candidates selected by the Council of the Royal Society for election to the Fellowship of the Society is as follows: Dr. E. F. Armstrong

(Managing Director of Messrs. Crosfield & Sons, Warrington); Sir Jagadis Chunder Bose; Dr. Robert Broom (Zoologist and Medical Practitioner, South Africa); Prof. E. P. Cathcart (Professor of Physiology, University of London, London Hospital Medical College); A. C. Chapman (Consulting and Analytical Chemist); Dr. A. P. Chattock (formerly Professor of Physics, University of Bristol); A. W. Hill (Assistant Director of the Royal Gardens, Kew); Dr. C. G. Knott (Lecturer in Applied Mathematics in the University of Edinburgh); Prof. F. A. Lindemann (Professor of Experimental Philosophy in the University of Oxford); Dr. F. H. A. Marshall (Lecturer in Physiology in the University of Cambridge); Dr. T. R. Merton (Lecturer in Spectroscopy in the University of London, King's College); Dr. R. C. L. Perkins (Consulting Entomologist to the Hawaiian Sugar Planters' Association); Prof. H. C Plummer (Royal Astronomer of Ireland); Prof. R. Robinson (Professor of Organic Chemistry in the University of Liverpool); Dr. J. W. W. Stephens (Professor of Tropical Medicine in the University of Liverpool).

Dr. Theodore W. Richards, Professor of Chemistry at Harvard University, has been elected a foreign member of the Royal Society.

Dr. A. E. Shipley, of Christ's College, Cambridge, has been elected an honorary member of the Société Royale Zoologique et Malacologique de Belgique; and Sir Ronald Ross, a Member of the Belgian Société de Biologie.

The Pontécoulant prize of the Paris Academy of Sciences for 1919 has been awarded to Prof. Eddington, and the Lalande prize to V. M. Slipher, of the Lowell Observatory.

The Henry Draper Gold Medal has been awarded by the American National Academy to Prof. Fowler, of the Imperial College of Science, for his work in celestial and laboratory spectroscopy.

The first award of the Charles Graham Gold Medal for medical research has been made to Dr. C. Bolton, in recognition of his work in experimental pathology carried out at University College Hospital Medical School.

The La Roquette Gold Medal of the Geographical Society of Paris has been awarded to Stefansson, in recognition of the discoveries made by the Canadian Arctic Expedition during the years 1913–18.

Mr. R. H. Hooker has been elected President of the Royal Meteorological Society for the current year. The Symons Memorial Gold Medal of this society has been awarded to Prof. H. H. Hildebrandsson, of the University of Upsala, for his distinguished work in meteorological science.

Dr. Eric K. Rideal has been appointed visiting professor of chemistry at the University of Illinois for the session 1919–20.

Among those who have passed away during the past quarter we note with regret the following: Joseph Barrell, Professor of Geology in Yale University; Prof. E. H. Bruns, of the University of Leipzig; Sir William R. Fraser, F.R.S.; Mr. F. W. Headley, naturalist and bird enthusiast, of Haileybury School; Dr. Cyril G. Hopkins, Head of the Department of Agronomy in the University of Illinois; Dr. C. R. C. Lyster, Head of the X-ray and electro-therapeutic departments at Middlesex Hospital; Prof. G. Macloskie, professor of biology, Princeton University; Dr. Francis P. Moreno, the Argentine geographer and naturalist; Sir William Osler; Admiral Peary; Prof. A. Riccò, Director of the Observatory of Catania and Etna; W. von Siemens, head of the Siemens-Halske companies at Zürich; Father J. N. Strassmaier, the distinguished Assyriologist; Dr. John Wilson, lecturer in agriculture and rural economy in the University of St. Andrews.

The International Research Council has finally been established, after a number of meetings in London and foreign capitals. Geophysics, mathematics, astronomy and (provisionally) biology and chemistry sections have been founded; each section will be organised on an international basis, so as to attain full co-operation in the different departments of science.

A special meeting of the Conjoint Board of Scientific Societies was held on January 8, to discuss the future co-ordination of international scientific action. In the end an indeterminate attitude was adopted, it being resolved that the Executive Committee be requested to appoint committees for the purpose of considering the desirability of forming international unions connected with the International Research Council, as recommended by the Brussels Conference, or of joining such unions if formed independently.

At a meeting of Congregation at Oxford, on February 10, a new form of Responsions Statute was accepted under which Greek will no longer be compulsory in responsions, and will not be required at any stage from candidates taking Honours in the final schools in Mathematics or Natural Science. This amended form of the Statute has still, however, to pass Convocation, where it is likely to be strongly opposed by the misguided mediævalists whose banner bears the legend "Compulsory Greek"!

At a Congregation held at the Senate House, Cambridge, on February 14, it was agreed to establish a Professorship of Physical Chemistry.

Colonel Sir Frederic Nathan, K.B.E., has been appointed Power Alcohol Investigation Officer under the Fuel Research Board of the Department of Scientific and Industrial Research. This appointment has been made as a result of the report of the Interdepartmental Committee on the Production and Utilisation of Alcohol for Power and Traction Purposes, which recommended the establishment of a small permanent organisation for the investigation of this problem. It does not seem likely, however, that any immediate results will follow from the new departure, for, before any research on an extended scale into methods of production and utilisation is undertaken, a "more or less accurate" estimate of the possible sources of alcohol within the Empire and of the cost of its production is to be made. It would seem to be quite obvious by now that power alcohol can be produced in sufficient quantities at a cost far below the present outrageous price of petrol, and that an immediate and extensive research is most urgently necessary.

The Council of the Glass Research Association has appointed Mr. R. L. Frink, Lancaster, Ohio, U.S.A., to be Director of Research. In a letter to *Nature* (February 5), Dr. Morris W. Travers comments very unfavourably on the appointment, for he is informed that in the United States "Mr. Frink is not known as a research man or in research circles; but that he is highly spoken of by practical glass-workers 'as a man of long experience in the window-glass trade, who is accustomed to be called in as first aid for furnace troubles, colour troubles, and like technical difficulties. This trade he has pursued for some years with success, and his reputation in this domain is among the best. He . . . has gathered together a considerable amount of rough-and-ready wisdom, which has found extensive application in an industry where research laboratories have hardly been thought of until recently.'" If Dr. Travers' information be correct, the Glass Research Association has made a most unhappy selection. Such a man is not likely to be of the type to work comfortably with, much less direct, a band of Englishmen trained in the methods of scientific research.

We are pleased to be able to record the gift of several handsome donations towards the cost of University education in this country during the last three months. Messrs. S. B. and J. B. Joel have given £20,000 for the endowment of a University Chair of Physics tenable at the Middlesex Hospital Medical School; Lord Cowdray has contributed £10,000 towards the sum of £100,000 required for the reconstruction of the engineering department at University College, London, and has promised a further £10,000 when the total subscribed shall have reached £70,000; finally, the Worshipful Company of Goldsmiths has presented £15,000 to the London Hospital Medical College for the endowment of a University Chair of Bacteriology. In America the golden shower which never ceases to fall on Science, Education and the Univer-

sities has developed into a veritable deluge. Mr. John D. Rockefeller heads the list with a total contribution of $110,000,000. Of this vast sum $60,000,000 goes to the Rockefeller Foundation, and $50,000,000 to the General Education Board. In a letter accompanying this latter gift, Mr. Rockefeller expressed his wish that the principal as well as the interest might be used promptly and largely for co-operating with *higher institutions of learning in raising sums specifically devoted to the increase of the salaries of the teachers*, and he further desired that his previous gift of $20,000,000 (recorded in these Notes only last quarter) might be used for promoting medical education in Canada as well as in the U.S.A. The late Mr. Henry C. Frick left the greater part of his estate for public, charitable and educational purposes in the States. It is estimated that about $145,000,000 will be available, and of this sum Princeton University will receive about $15,000,000, Harvard $5,000,000, the Massachusetts Institute of Technology $5,000,000, and the Educational Fund Commission, Pittsburgh, another $5,000,000. Yale receives $200,000 for the general endowment of the School of Medicine as a result of the death of the widow of the late Dr. Levi Shoemaker.

The idea that University teachers are worthy of a reasonable wage seems to be becoming quite widespread on the other side of the Atlantic, for not only has Mr. Rockefeller emphasised it in the admirable manner related above, but the City of Rochester raised $800,000 in *less than one week* for increasing the salaries of the staff of its University!

Manchester University is appealing for £500,000 to enable it to proceed with much-needed extensions, and the Manchester College of Technology asks for a further £150,000 for the same purpose.

It has been decided to devote the sum of £240,000 left by the late Mr. Thomas Cawthorn, of Nelson, N.Z., for the founding of a technical institute in New Zealand, for the erection and endowment of a research institute in that country. A site of twenty acres has been secured in a position overlooking Tasman Bay, about three miles from the town of Nelson, and it is anticipated that the buildings will soon be in course of erection. Prof. T. H. Easterfield, of Victoria College, Wellington, has been appointed to be first Director of the new Institute.

The house and laboratory built on the banks of the Susquehanna River, in Pennsylvania, for Joseph Priestley when he emigrated to America in 1794, has been purchased by the graduate students of the Pennsylvania State College. It is a frame house, and is, even now, in so excellent a state of preservation as to permit of its re-erection in the grounds of the College as a permanent memorial of its original owner.

In a magnificent monograph (part i, vol. xi, of the *Memoirs of the American Museum*), President H. F. Osborn undertakes a revision of the fossil horses of North America found in rocks of Oligocene to Pliocene age. All the type specimens are figured, either by reproduction of the original or by new drawings. Finally, in fifty-four plates, Osborn gives a series of comparative figures which present objectively the stages of the evolution of the horse from Mesohippus to Equus. The closeness of the stages, about forty of which are figured in correct time or order, makes the whole work most impressive, and its study of vital importance to any student of evolution and biological theory.

A new book on the *Amœbic Parasites of Man* (J. Bale, Sons & Danielsson) has just appeared. It is written by the well-known protozoologist, Prof. Clifford Dobell. The author has taken great pains completely to elucidate many interesting problems associated with the amœbic fauna of man, and this book will prove a valuable addition to protozoological literature. It is regrettable, nevertheless, that Mr. Dobell should so summarily dismiss the work of many other protozoologists.

A valuable paper by Dr. A. D. Imms (Transactions of the Royal Society B. 361, 1919), has just come into our hands. The writer of this interesting paper has studied the biology of Archotermopsis, one of the most primitive of living Termites. Dr. Imms believes that polymorphism is not adequately explained as being the direct or indirect effect of special nutrition.

In the latest numbers of the *Transactions of the Royal Society*, Dr. D. M. S. Watson has published several of his analyses of the origin and evolutionary changes of certain groups of fossil vertebrate animals. These papers will form a valuable contribution to our knowledge of Vertebrate Palæontology.

We acknowledge the receipt of six parts of the *Memoirs of the Asiatic Society of Bengal*, containing the "Zoological Results of a Tour in the Far East," edited by Dr. N. Annadale, F.A.S.B. The main object of Dr. Annadale's tour was to obtain material for comparison with the fauna of Indian fresh-water lakes and lagoons; Japan, the Kiang-su Province of China, and the Malay Peninsula were visited. Great activity has been exhibited by Dr. Annadale and his associates in the working out of the material procured. Lake Biwa in Japan, as might be expected from its geographical position, seems to be (so far as the Mollusca are concerned) the meeting-place of two lines of migration, one from the North, the other from the South. The Oligochæta have been treated by Prof. Stevenson, Hirudinea by Dr. A. Oka, of Tokyo; three of the Hirudinea were of special interest, one (*Ancyrobdella biwæ*) is a curious Glossiphonid, much resembling an Ichthyobdellid in external appearance. Annadale's work on the molluscan fauna of Tai-Hu, or Great Lake (Kiang-su), is of much interest. The Mollusca found are peculiar for the fact that they are small in size, and have a strong estuarine element in their composition. Other parts of these volumes have been taken by Dr. Prashad (Echiuroids), Dr. Chopard (Orthoptera), Dr. Kemp (Crustacea), Dr. Annadale (Sponges), and Sir Ch. Eliot (Mollusca, Nudibranchiata), etc. We believe that Dr. Annadale's expedition has been very useful and successful.

As is well known, before the war, British optical firms imported most of their glass from Germany. The declaration of war on August 4 threw our optical manufacturers on their own resources, and we owed our success in the making of optical glass largely to the enterprise of Messrs. Chance Bros., of Birmingham. The end of the war now brings new conditions; optical glass is no longer needed in large quantities, and as the amount of optical glass in any one instrument is quite small, it is doubtful whether it will pay large firms to undertake small meltings and to undertake the necessary research. Dr. W. E. S. Turner pointed out at the Symposium of Microscopical Societies, which met at the Royal Society on January 14, that the obvious remedy lies in the use of the joint resources of the British Optical Instrument Manufacturers' Research Association, under Sir Herbert Jackson, and of the Department of Glass Technology in the University of Sheffield. We hope that this suggestion of Dr. Turner will be carried into effect.

We note also that several British firms have undertaken the manufacture of dyes and stains suitable for delicate histological and cytological work. We trust that the heads of our University and other laboratories will note this, and see that every encouragement is given to this new departure. We have ourselves tried some British acid fuchsin, and found it to be perfect even for staining such difficult objects as the Mitochondria.

Dr. Ruggles Gates recently gave a paper at the Linnean Society entitled, "On the Existence of Two Fundamentally Different Types of Characters in Organisms," which cannot fail to interest those who are concerned in any way with Evolution and Heredity. The point of view resulting from modern work on mutation and Mendelism is frankly antagonistic to the views of palæontologists and anatomists, and others who deal with orthogenesis and the inheritance of acquired characters. Dr. Gates suggests that higher organisms exhibit two contrasted types of characters, which differ funda-

mentally (1) in their manner of origin, (2) in their relation to the structure of the organism, (3) in their relation to recapitulation, adaptation, and inheritance, (4) in relation to geographic distribution. To the first category belong cell-characters, which arise as mutations, and are usually inherited as distinct entities, and, being borne by the nuclei, may be called "Karyogenic" characters. To the second category belong organismal characters which arise gradually through impact with the environment or through orthogenic changes, may modify localised parts of the life-cycle, and may not be incorporated in the germ-plasm from the first.

Dr. F. W. Aston, in a letter to *Nature* (December 18, 1919), gives a preliminary account of some of the results he has obtained with an improved positive-ray spectrograph. He finds that carbon and oxygen show no signs of possessing isotopic forms; that chlorine consists of two isotopes of atomic weights 35 and 37; that neon, as is well known, is a mixture of two, whose atomic weights are 20 and 22 (correct to a tenth of 1 per cent.); while mercury is a mixture of at least three or four. Experiments giving more than forty different values of atomic and molecular mass show that all are exact integers (taking C = 12 and O = 16). The case of hydrogen, which clearly becomes of special interest if these results are confirmed, has not yet been investigated.

Quite the most interesting exhibit at the Physical and Optical Societies Joint Exhibition, which was held at the Imperial College of Science on January 7 and 8, was the Sheringham Daylight device. This invention is one of the simple why-didn't-I-think-of-it-before type, which should be of material assistance in the colour industries. It consists merely of a large umbrella-shaped reflector placed above a high-candle-power electric lamp, the reflecting surface being covered with small squares of coloured paper arranged in chessboard fashion. These squares are of many different colours, blues and greens predominating, so that the excess of yellow in the artificial light is eliminated. The energy wave-length curve of the light diffused from the reflector can be plotted from measurements obtained with a spectro-photometer, and thus the proportions of the different colours on the reflector can be adjusted to match any desired quality of daylight with any specified source of illumination. The usefulness of the new illuminator in the dye trade, in drapery stores, in the lighting of art galleries, etc., is self-obvious, and it will make it possible to specify a standard "daylight" in which all colour-matching processes may be carried out.

The Bureau of Standards, Washington, has just issued a revised list of its various publications (Circular No. 24). Since the first paper was printed in 1904, the staff of the Bureau has been responsible for 338 papers of purely scientific interest, 138 technical papers, and 82 circulars. Among the latest of these is a paper by Coblentz and Kahler dealing with the optical and photo-electric properties of Molybdenite, which is conspicuous for its high photo-electric sensitivity to infra-red rays extending to 3 μ; a paper dealing with the effects of glucose and salts on the wearing qualities of sole leather, and the first of a series of investigations intended to establish, by laboratory methods, the relative values of different deposits of stone for building purposes. The whole of these publications (with the exception of a very few now out of print) can be obtained from the Bureau at a small cost which varies with the length of the paper.

The Report on Engineering Standardisation written by Mr. Gerald Lightfoot for the Australian Institute of Science and Industry rather startles us at the outset by holding up Great Britain, along with the United States, as an example of a country where standardisation in the engineering industry has reached a high stage of development. Nevertheless, on reading further through his pages we are able to realise that, as compared with Australia, the conditions in this country form an ideal to be striven after. It appears

that in the Commonwealth there is not one single engineering material for which one standard specification is in general use. Thus there are no fewer than eight specifications for cement laid down by various railway and Public Works departments. Electric power is supplied from different generating stations at twenty different voltages, and electric fittings are of many different sizes. This not only involves a large unnecessary expenditure in carrying stocks, but also renders it a matter of great difficulty to establish home industries for the manufacture of electric lamps and appliances. In these circumstances it is not surprising that the leading engineering firms are supporting a movement for the formation of a Commonwealth Engineering Standards Association to work on the lines of the similarly-named English Association or of the American Society for Testing Materials. The 1918 Report of this latter society shows that the list of standards covers 132 engineering materials, most of which have been revised at least once since their adoption, and some as often as six times; a fact which answers the charge that standardisation tends towards crystallisation, and so impedes progress. The Report under review contains a full list of the English and American Sub-committees, and outlines a working scheme for use in Australia.

In addition to the new scientific periodicals mentioned on p. 645, we are also glad to welcome the *British Journal of Experimental Pathology*, edited by a number of distinguished pathologists, of which the first number was issued in February 1920 (H. K. Lewis & Co., 136 Gower Street, W.C.1).

The Sociological Society announces that it is moving from the London School of Economics to 65 Belgrave Road, S.W.1. As its new quarters afford more accommodation than it needs, the Society will be glad to hear from similar societies which might desire to rent one or more rooms. The situation is very convenient, being within five minutes' walk from Victoria Station.

The National Union of Scientific Workers (19 Tothill Street, S.W.1) finds it necessary, owing to its rapid growth, to employ a full-time secretary, and Major A. C. Church, D.S.O., M.C., B.Sc., has been chosen for the post. Since last November two new branches have been formed, and several other branches are in process of formation.

With reference to the matter of Awards for Medical Discovery given on p. 635 et seq., Mr. Balfour, Lord President of the Council, received a deputation consisting of the members of the Conjoint Committee mentioned on p. 641, together with several medical and other Members of Parliament, on March 2. The deputation was introduced by Sir William Watson Cheyne, Bart., and the business done will be referred to in our next issue.

ESSAYS

VERIFIABLE KNOWLEDGE (George Shann)

The theory of knowledge has often been treated as a branch of metaphysics; the following is an attempt to deal with the subject of the nature and function of knowledge from a scientific point of view.

It will be convenient to start with certain hypotheses and to leave the question of their validity for later consideration. First, then, it is supposed that the changes in consciousness which go to form the knowledge we are considering are, at least in part, direct reactions of a nervous organism to stimulus. Secondly, that reactions to simultaneous stimuli tend to become associated together in consciousness and that, since each reaction occupies an appreciable interval of time, a somewhat similar association may take place between reactions whose durations overlap. Also, that in consequence of this, the experiences of which we are conscious come to be felt as a continuum, even though the stimuli should be discontinuous like pictures in a cinematograph. Thirdly, that when two reactions have been thus associated a more or less permanent connection remains in memory, so that if one of the stimuli be subsequently repeated the reaction to it may include traces of that reaction to the other stimulus by which it was accompanied on the previous occasion. In virtue of such connections long series of nervous reactions can be linked together without any other nexus than their having frequently occurred in close succession; thus it is common for Oriental students to learn and to repeat the sounds of whole chapters of their sacred books, the language of which is unknown and conveys to their minds no meaning whatever.

Knowledge depends upon mental arrangements in series, but the nervous reactions of a highly organised animal are innumerable; no single series can include them all; there must be some selection. Even the rudimentary kind of knowledge above mentioned requires that the sequence of auditory reactions should be segregated from the various other reactions by which they must have been accompanied. The nature of the selection is usually determined by certain indirect, or secondary, reactions, which are called suppositions, or hypotheses, because they are *put under* the sequences of direct reactions to bind them into the fabric of knowledge. Thus if certain visual sensations at one moment be followed by certain others shortly afterwards, the sequence may be associated in series by the hypothesis that the stimuli to which the sensations respond are due to something outside consciousness, called matter, and that the two stimuli come from the same material body, seen successively in two different places because it has moved from one to the other.

It is not, I think, too fanciful to compare these direct and indirect reactions with the warp and woof of a textile fabric; the warp being laid down as a foundation, while the woof, determining which threads of the warp shall appear in a given place and which shall be hidden, not only binds the whole together, but also produces the pattern.

A theory of the construction of mental series having been propounded,

it remains to enquire how they are utilised for the benefit of the organism. This is effected by means of the farther hypothesis that they will *recur*, so that, if the earlier parts of a series should subsequently be repeated, the rest of the associated reactions will follow in their original order. This expectation of recurrence is the only basis on which forecasts of the future can be drawn from the experience of the past.

Now each of us includes in many of his mental series elements which he must regard as being his own actions, and is aware, moreover, that the subsequent experiences in some of these series are desirable, in others undesirable. An expectation of the recurrence of these series would be manifested by repeating actions which had been followed by desirable experiences and refraining from the others. This is characteristic of the conduct of all the more highly organised animals, and is to be noticed in the actions of human infants even while very young. Its appearance at an early age in a great variety of organisms gives the idea that the aptitude for developing the expectation of recurrence is inherited and might be classified under the rather ill-defined heading of instincts.

Actions, then, can be guided by forecasts of future experience, drawn from the experience of the past in virtue of the hypothesis of the recurrence of series; but we find that this is often applied too indiscriminately, every sequence of reactions mentally arranged in a series being expected to recur. Man's experience, as it widens, teaches that there are many exceptions, and it becomes evident that before knowledge can be a trustworthy guide it must distinguish between the series which are, and those which are not, recurrent.

The method by which hypotheses are tested is called verification; it consists in framing forecasts on the basis of the hypotheses and then comparing them with actual experience; the more uniform the correspondence between forecast and experience, the more complete is the verification, and the greater will be the confidence that the hypotheses are valid. Those propounded at the beginning of this article lead to forecasts of the way in which ideas will be found to originate; their validity must be tested by experience of what actually takes place.

There is one fundamental recurrence-hypothesis which, when it can be submitted to verification, has never been known to fail, and which is therefore regarded as absolutely beyond question—namely, that if all the elements essential to a series be repeated, the series will recur. Hence, if all the elements of a series be abstractions by whose nature every extraneous element is excluded, one single experience may be sufficient for a verification, giving certain assurance of its recurrence.

For instance, three parallel rows of four dots each can be arranged in a parallelogram, and it is seen that the same dots may be regarded as four rows of three. It is at once felt as a certainty that a forecast of repetition will hold good, and that multiplier and multiplicand will always be interchangeable provided that they represent the same abstractions (integral units) which are exemplified by the dots. This is no guarantee of recurrence when the elements of the series are other than unit quantities; in quaternions abstract factors are used which have a different relation.

In the case of concrete sequences verification is more difficult, because the mental associations of the series cannot include more than a finite number of the noticeable elements of the sequence, and it is never certain that these will be the only ones affecting the recurrence of the series. When a gardener, taking seeds from plants of a particular species, sows and tends them according to a certain plan, he forecasts approximately what will be the outcome of his work; he expects that some of the seeds will develop into plants because he knows that seeds of that species have often done so under conditions not obviously different, and relying on the hypothetical

generalisation that like begets like, he expects that the plants will have a resemblance to those from which the seed came. If a sufficient number of trials be made, the average of the subsequent experiences will doubtless correspond fairly with the forecasts; but there is no certainty that any individual seed will germinate; various unknown conditions may prevent it; and of those plants which do grow, no one will be like another in every respect, nor will it be exactly like those from which it is descended; each has its individual peculiarities, which in some instances may be so pronounced that the plant will be regarded as a "sport."

Forecasts from hypotheses which do not exclude unknown elements are never quite free from such uncertainty, and verification is more or less satisfactory according to the degree of uniformity in their approximate agreement with subsequent experiences. Owing to the indeterminate nature of verification in such cases, the idea of doubt has become so closely associated with the word *hypothesis* that objection may be taken to its use in reference to mental series whose recurrence is considered to be certain; the mental operations, however, are similar in all cases and it is convenient to denote them by the same word.

When a series whose principal elements are known is subject to variation under the influence of minor unknown elements, limits can in some cases be assigned to this variation and the *probability* of a particular forecast can be found. Thus in the tossing of a coin the known elements are supposed to be such that either a "head" or a "tail" will be uppermost when it comes to rest; the moving forces, other than gravity, are undetermined, but the variation introduced by them into the series is limited to the appearance of one or other of two alternatives, whose probability is equal. It will be seen, then, that probability both implies a basis of knowledge and is in itself a modified kind of knowledge, since it lays down boundary lines in regions where ignorance would simply leave a blank.

Beside the division of mental series into abstract and concrete, another distinction is to be noted. The natural and, as it were, automatic formation of series from experiences which follow one another closely, is common to man and to many animals; mankind has acquired, in addition, the power of connecting, by mental association, sequences of experiences divided by considerable intervals of time, such as the appearance of a plant where seed was sown several months earlier.

It has already been pointed out that, by mental association, a present reaction may arouse the memory of some different reaction in the past; if this in its turn should revive the memory of the same sequence having been in the mind previously, the idea of recurrence will be suggested. The expectation that the sequence will always recur needs verification, but if this be obtained by a sufficient number of repetitions, the connection of experiences may be regarded as empirical knowledge.

To make such knowledge systematic an hypothesis must be framed by which the discontinuous experiences can be represented as forming parts of some process already known and held to be continuous. The recurrence of the series can then be verified more effectively than before, since it becomes possible to forecast that—If the empirical sequence corresponds with the known process, not only in the observed portions, but also throughout its whole extent, then this-and-that will be experienced. The more uniformly subsequent experiences bear out these forecasts the greater will be the confidence in the validity of the hypothesis.

Illustrations of this may be found in the history of astronomy. Very early ages had bare records of what was, from time to time, noted concerning such things as the successive phases of the moon, the recurrence of sunrise in particular positions, or the repetition of eclipses at regular intervals. Then these discontinuous experiences were combined into a system by

hypotheses based on the known process of rotation about a fixed axis. After it had been shown that the forecasts thus obtained were not sufficiently accurate, Newton applied the hypothesis of gravitation, suggested by the known process of the acceleration of iron toward a magnet, and of heavy bodies toward the earth. The forecasts made on this latter hypothesis have corresponded remarkably well with subsequent experience.

Lastly, what is the relation of knowledge to causation and to external reality?

Present ideas of causation rest upon the theory of energy, to which Newton gave the first definite shape in the scholium to his third law of motion. His enunciation is paraphrased by Lord Kelvin in the words: "The whole work done, in any limited material system, by applied force is equal to the whole effect, in the forms of potential and kinetic energy, produced in the system, together with the work lost in friction." (*Natural Philosophy*, Thomson & Tait, § 287).

About the middle of the nineteenth century, however, it was shown that each unit of work has its exact equivalent in a unit of heat, the two being interchangeable; so that work employed in overcoming friction is not lost, but only changed into the form of heat; or putting the theory into more general terms, that energy always remains the same in amount, however much it may change its form.

For modern thought this is almost as important as the older hypothesis of the constancy of the quantity of matter; it supplies an intelligible connection between the changes in a physical process by giving definition to the idea of causation, which had previously been extremely vague.

The connection is of two slightly different kinds. In one the cause and the effect are simultaneous, and the idea of the transformation of energy forms a mental link between two different aspects of a single process, which is called the cause when it is regarded as a change *from* the initial condition, while as a change *into* the final condition it is called the effect. For example, the combination of carbon and oxygen is the cause, whose effect is the production of carbon dioxide; the link between these aspects being the idea of the liberation of energy which is transformed and given out as heat.

In connections of the other kind the idea is that energy is set free by one process, called the cause, for transformation in a subsequent process called the effect. In this sense the pulling of a trigger may be the cause of the discharge of a gun.

The question of our knowledge of substance, or external reality, has been much discussed, and has generally been treated as though it differed from all other questions, or was something mysterious. Locke says that we neither have nor can have the idea of substance by sensation or reflection (Essay on Human Understanding, Bk. I, iv. § 18), yet he also says that we have the ideas of its "primary qualities" as they really are (Bk. II, xxxv. § 2). Berkeley asserted that the external reality corresponding to our idea was a thought in the mind of his deity, and had no other existence. Reid and the Scotch metaphysicians denied this, but their positive views about our knowledge do not seem to have been much clearer than those of Locke. Kant again deals with reality in phrases concerning categories and schemata and the-thing-in-itself, which could never have afforded any confirmation of the world-wide hypothesis of external reality. On this subject neither the writers mentioned, nor any others that I know of, have put their views in such form that they can be tested by comparison with experience.

In the solution of the problem two considerations are necessary. The first concerns the meaning of reality, which in its ordinary, non-metaphysical sense has reference to the fulfilment of forecast. Just as we say that gold is real if the forecasts be fulfilled which are made from a particular hypothesis about its nature, so we must admit that substance is real if the forecasts be fulfilled which are made from the hypothesis of its existence.

The second consideration is that when this hypothesis of the existence of real external substance has been sufficiently verified it does not differ in any respect from the rest of our knowledge. As in the case of our other concrete hypotheses, repeated tests are necessary before the verification can be deemed adequate. But these are so superabundant, even in early life, that if certainty be denoted by unity, the difference between this and the validity of the hypothesis of reality might be denoted by that between unity and the recurring decimal $0{\cdot}\dot{9}$; a difference which is less than any assignable quantity. Reality, therefore, cannot be excluded from the scope of our knowledge unless everything else, beyond bare sensation, be also excluded.

Very concisely, then, the core of the theory now propounded is that the mental processes concerned in the production of knowledge are of two kinds, the direct reactions of the organism to stimulus, and the hypotheses by which the remembrances of these reactions are associated in series; that the immediate object in view is the making of forecasts for the future on the basis of the hypothesis that the series will recur; and that the success of forecasts is the sole test of the validity of hypotheses. Those of the present theory are framed for the purpose of forecasting how fresh knowledge will be attained, and must be subject to verification by farther experience of its progress.

It is the ultimate object of knowledge to be the guide of action.

Unless a theory can in some way conduce to that object I attach little value to it; my hope is that this one will serve as a guide in future methods of education.

THE PURIFICATION OF MOORLAND WATER SUPPLIES

(Ernest G. Blake, M.R.S.I., A.B.I.C.C.)

A CONSIDERABLE difference frequently exists in the composition of water which is obtained in certain districts, although they may not necessarily be widely separated, the variation arising from purely local causes. The character of the water naturally exerts an appreciable influence on the health and well-being of those who use it for drinking and culinary purposes, and the origin of various more or less serious disorders is commonly ascribed to this source. Hard water—i.e., water which contains an excess of lime—is popularly supposed to be the predisposing cause of goitre or Derbyshire neck, gravel, etc., from the fact that the system becomes clogged with the lime after continuous use. It is also responsible for various troubles apart from the human frame, and is the direct cause of the "furring" which occurs in boilers and cooking utensils, when the lime collects on the sides and bottom of the apparatus in the form of a hard, insoluble incrustation, commonly known as scale. Water which contains less than six grains of lime to the gallon is considered to be soft, and is less troublesome in this respect. In this condition it is decidedly preferable for domestic purposes.

The supply which is collected on a peaty gathering-ground may, on the contrary, act in a reverse direction, and may be of such a nature as to destroy almost any metal with which it comes into contact, its corrosiveness varying with the class of soil from which it is drawn. This destructive tendency is caused by the excessive softness of the water, combined with the presence of humic acid, which is gathered from the vegetable constituents of the soil as it percolates down through the ground. Peaty water is usually of a clear brown colour and is very similar in appearance to weak tea, although it does not thereby necessarily produce dangerous consequences when consumed; but when it is strongly acidic the pipes and metallic fittings deteriorate so rapidly as to constitute a very serious problem, as to how these effects are to be neutralised without going to undue expense. The trouble is much more

apparent in the hot-water system than it is in the cold, especially when the boiler is driven at a considerable pressure, as the corrosive action is intensified by the higher temperature, and the resultant damage is increased in a proportionate degree. It is important that all valves, taps, and fittings should be of the best quality gun-metal which is procurable, and even then, in many cases, the seatings are rapidly destroyed and become ineffective in a comparatively short space of time, necessitating re-seating, perhaps, every few months. This may appear to be inconceivable to many who have not experienced the possibilities of this class of water, but those who are familiar with its effects will recognise that it is not an abstract proposition by any means. Common brass fittings are quite useless in a case like this.

It will be seen that a very troublesome situation is created when the only available supply is of such a nature as this, and suitable steps should be taken to eliminate the solvent properties of the water.

There are several safeguards which are commonly employed under these circumstances, most of which consist in the use of materials which do not deteriorate in so marked a degree as others. Iron pipes and fittings are of little value, as they are quickly attacked and rendered useless, while the provision of brass fittings will not surmount the difficulty for a similar reason. Copper is less subject to corrosion than either iron or brass, and is often used in better-class work. The acid, however, has the effect of dissolving a minute portion of the metal, thus imparting a green tinge to the water after it has passed through the circulating system. In this state it is dangerous and unfit for human consumption. To obviate this difficulty the pipes are frequently lined with tin, which is immune from the solvent action of the water, and gives excellent results, but is is extremely costly, as all portions of the system, including pipes, circulating cylinder, and tanks, etc., must be similarly protected on the inside. Lead is obviously out of the question, especially for hot water, both on account of the excessive expansion of the pipes, and the risk which is incurred of lead poisoning when it is taken into the system.

It has been found possible of late years to manufacture glass-lined pipes and receptacles, and this is, perhaps, the ideal method of storing corrosive liquids, but the obstacles which are experienced in bending the pipes, and in making effective joints between the interiors of the various sections, seriously handicap the efficiency of the work. Here, again, also, the question of expense is a vital factor when deciding as to which method is the most suitable under given conditions, and it will probably be found that glass-lined materials are too costly for any ordinary domestic work.

Although it is practically impossible to guard against the unfortunate consequences arising from these conditions, by any mechanical means which are really worthy of serious consideration from the point of view of efficiency and reasonable economy, it is, at the same time, quite essential that some steps should be taken to reduce the unnecessary waste which inevitably occurs if the situation is neglected, and to eliminate the danger to those persons whose bodily health would necessarily suffer, in some degree, from the continuous use of water possessing such injurious properties.

Most country residences are provided with their own water-supply, especially when they are situated at some little distance from the nearest available town main, and this supply is almost invariably obtained from a well sunk in the vicinity. The best site for the well is on the higher open spaces of the estate, as a purer quality of water is usually obtainable, on account of the improbability of contamination arising from the proximity of cultivated land and of inhabited dwellings. This class of land is often unfit for cultivation owing to its stony and unproductive nature, and may be comparatively unencumbered with buildings of any kind. It is frequently covered with a top layer of peaty loam, and is almost destitute of vegetation beyond quantities of gorse and heather, which thrive under

these conditions; but, although it may be free from organic pollution in the ordinary sense, it will, in all probability, be charged with the undesirable acidity already mentioned. A permanent and unlimited flow can often be obtained on these moorland gathering-grounds quite near to the surface, and the choice lies between this source of supply and the sinking of a deep well which will tap the purer water lying beneath the upper impermeable stratum in the soil, on some other portion of the estate. The latter method involves a heavy outlay for boring the well and lining the bore-hole with iron tubes, and the capital sum which is required for the execution of the work will give considerable food for thought before a decision can be arrived at as to which scheme will be the best to adopt under the circumstances.

When all these factors are taken into consideration it will be found that better results may be ensured by dealing with the origin of the trouble than by the provision of mechanical means for neutralising at the most a portion of its objectionable effects after they have materialised. In the case under discussion the renewal of all metal pipes and fittings at frequent intervals constitutes a heavy drain on the purse of whoever is responsible for the upkeep of the property, the expense recurring with irritating persistency; and then even ideal conditions are not obtainable.

In such a case the most effective remedy which can be adopted is to remove all traces of acid from the water before it comes into contact with any metal, and this can easily be accomplished with little trouble or expense. The process by which the separation can be carried out consists of the filtration of the water through broken chalk immediately it is discharged from the pump, and before it passes into the pipe by which it is conveyed to its destination.

When the supply is drawn from a well it may be necessary to provide a reservoir of suitable capacity for storing a sufficient reserve of water against the needs of the dry season, and a hand or power-driven pump will be indispensable for raising the water. Occasionally the distribution may be effected by gravitation direct from the well if sufficient fall in the land is available; but, as a rule, the storage reservoir will be required in order to obviate the possibility of overtaxing the supply in the day-time when large quantities are being consumed.

When an exceptionally large volume of water is drawn daily from the well, the motive power for driving the pump can be obtained from a petrol- or oil-engine, or an electric motor, the capacity of which will be determined by the amount of work which has to be performed, and the period of time during which it can be conveniently operated. It is more advantageous to instal a plant of fairly large capacity rather than a smaller one which needs to be run almost continuously in order to maintain an adequate supply. Less wear and tear will be entailed with the larger plant, and a considerable economy in the cost of labour will be effected.

If the well is situated on high and open ground, and the normal quantity of water to be raised daily is not excessive, the case can be satisfactorily met by the provision of a windmill, which develops the motive power for operating a force-pump fixed at a suitable level in the well. These windmills are in use in many parts of the country, and being quite automatic and self-regulating, they are especially useful for a small house supply. It must be borne in mind, however, that, as the quantity of water which is raised is entirely dependent on the velocity and persistence of the wind, it is most essential that ample storage should be provided, possibly for several weeks' consumption, or the supply may fail in the summer months just at the very time when an increased demand is being made on the reserve. The apparatus requires very little attention beyond oiling once a fortnight, and an occasional new bucket-leather for the pump. Its appearance is not particularly ornamental; but this is a point which must be ignored, as it

is counterbalanced by the low cost of the motive power which is derived from its use. The foot-valve at the bottom of the suction-pipe will require periodical examination, as it is liable to become choked by the sand which is silted into the well by the flow of water. If this is neglected the pump will ultimately be put out of action, or the sand will be drawn up into the barrel, and will damage the bucket-leather.

The storage reservoir can be constructed either above or below the ground according to the difference in level between the well and the highest draw-off tap which has to be served. The maximum fall which can be obtained should be given to the supply-pipe from the reservoir in order to expedite the distribution of the water by gravitation. When the point of delivery is at a higher level than the well, the water can be raised into an elevated tank, which must be constructed at a sufficient altitude to generate the necessary pressure. If this is impracticable the only alternative will be to drive the water to its destination by mechanical means. This necessarily increases the initial outlay as well as the working expenses, and should be avoided if other arrangements can be made.

Particular care should be observed when laying the pipe-line, especially when it is of considerable length, as neglect may result in perpetual annoyance through the interruption of the flow arising from air-lock. The pipe should slope continuously from the reservoir to the house as far as possible; but if the contour of the land is undulating and precludes the possibility of obtaining an unbroken fall over the whole distance, the inclination of the pipe should be divided into sections with variable gradients, each proceeding in the same direction. When a rise is unavoidable, an automatic valve should be inserted at the highest point, to permit the escape of accumulated air which naturally collects at the apex of the angle formed by the two sections of the pipe, and which, if not vented, might entirely obstruct the flow of water in either direction. If the fall is continuous along the whole line, the air is automatically vented into the reservoir, even though the gradient varies, and no trouble can arise from this source.

The construction of the reservoir can be carried out in brick, stone, concrete, or any other suitable material which can successfully withstand the stresses to which it will be subjected. Concrete is peculiarly adapted for the purpose, and if carefully manipulated and reinforced with expanded metal, will make a thoroughly strong and satisfactory job. The interior will need to be plastered with a 1-in. coat of Portland cement and sand to prevent leakage of the contents, or considerable waste may take place. Absolute security against percolation can be ensured if the cement is waterproofed by the addition of 5 per cent. of "Pudlo" before it is used. This closes the pores of the mortar and makes it quite impervious, and is an effective preventive against the admission of foul matter from the soil outside, even when it is under considerable pressure.

A small sand-filter, 4 ft. or 5 ft. square, can be arranged in one corner of the reservoir by means of thin dividing walls. Openings should be left at the bottom connecting with the main chamber, for the passage of the water, which enters at the top and percolates downwards through the filtering material. The most suitable filtering medium is clean, sharp gravel of varying sizes, the largest being at the bottom, and graded to pass a ¾-in. ring. This layer may be 1 ft. in depth, and should be covered with a number of old slates laid loosely over the top to separate the pebbles from those of a smaller grade. The intermediate layer, graded to the size of peas, will need to be of a similar depth, and the top should consist of 18 in. of coarse, sharp sand, which has been thoroughly washed to remove all dust and vegetable matter. Sand filters are more in the nature of mechanical strainers in their effects; but, after being in use for some little time, a film is deposited on the surface, which assists in arresting the passage of bacterial life.

Before the water is admitted into the reservoir it must be relieved of any acid which is held in solution, this being the primary treatment to which it is subjected. For this purpose it will be necessary to construct a small subsidiary receptacle in which the operation can be performed. Concrete can be used in this case also, and should be similarly plastered with "pudloed" cement, this being entirely unaffected by acids. The dimensions of the tank may be approximately 8 ft. long by 4 ft. wide and 4 ft. deep, or larger as circumstances demand. The interior should be divided into two compartments of equal size by the introduction of a partition wall across the centre, extending from the top of the external walls down to within 6 in. of the bottom. The water is delivered directly from the well into one end of the tank, and the further compartment is connected with the main reservoir by means of an outlet-pipe a few inches down from the top. This tank can be constructed in one corner of the reservoir, if desired, in a similar manner to, and connected with, the sand filter by the outlet-pipe, thus making the whole scheme compact and self-contained; but this will necessarily entail increasing the dimensions of the main reservoir. The rising main from the pump can be connected either through or over the top of the concrete wall.

The preliminary filtration of the water for the extraction of the acid is effected in the primary or small tank, both compartments of which have been nearly filled with clean chalk broken to pass a 3-in. ring, and with all dust carefully excluded. Chalk has a decided affinity for acids, and as the water is delivered into the first compartment and is compelled to pass down under the partition, and up to the outlet at the opposite end, it is subjected to both downward and upward filtration, and cannot possibly obtain access to the reservoir without first coming into intimate contact with the chalk. The effect of this treatment is that all traces of the acid are removed from the water during its passage to the sand filter, while its alkalinity is improved, and it will exert little, if any, deleterious action on the metallic pipes and fittings through which it circulates. As both the primary and secondary tanks are partially exposed to the weather, it is imperative that suitable provision should be made against the effects of frost, any valves or connecting pipes being well wrapped round with non-conducting material. When the tanks are situated below the ground the open tops can be covered in with reinforced concrete supported on light iron joists, entrance to the tank for the purpose of cleaning or repairs being obtained through manholes formed in the concrete, and covered with a well-fitting slab.

Although this process is simple, it is remarkably effective in eliminating the corrosive properties of the water, and little cause for dissatisfaction should be experienced if care and forethought are applied to the measures which are taken. It is obvious that the periodic renewal of the chalk will be essential, as it becomes inert owing to its absorption of the acidic element in the water. The length of time which should elapse between refilling the tank will depend on the composition of the water and its effects on the pipes and fittings.

A simple test for determining the purity or otherwise of the water is to immerse a piece of blue litmus paper in a sample drawn from the outlet of the primary rank. The least trace of acidity will cause the paper to change from blue to red, and the intensity of the tint will indicate the condition of the water, and the comparative urgency of the necessity for renewing the chalk.

Very little consideration is required to demonstrate that the only extra expenditure involved by the adoption of this plan is the outlay incurred in constructing the primary filter and its connections. This is really a trifling matter compared with the whole system of water-supply; and as the results which accrue from the treatment are perfectly satisfactory in every

respect, the expenditure is fully justified both on economic and hygienic grounds.

A GREAT DEFAULT (Sir R. Ross)

I have long intended to publish a short but consecutive review of certain efforts of my own since 1899 for the practical reduction of malaria in British dominions; and the recent death of Sir William MacGregor, who helped me largely in the work, now compels me to write the paper—not quite a pleasant task. I should like to add a few personal memories of a good and great man, as a duty to be discharged in his honour.

The Right Honourable Sir William MacGregor, P.C., G.C.M.G., C.B., M.D., LL.D., the son of a Scottish farmer, was born in 1847 and died in July 1919. He took his M.D. at Aberdeen in 1874 and first served as Government Medical Officer of the Seychelles, Mauritius, and Fiji. Very soon, however, he was selected for general administrative work and became the first Governor of British New Guinea (1895), and then Governor of Lagos (1899), Newfoundland (1904), and Queensland (1909). He retired in 1914, and bought the property of Chapel-on-Leader, Berwickshire, where he died. Lady MacGregor survived him only for a few months.

I think that I must have met him first in 1899, when he was passing through Liverpool on his way to Lagos. At that time I had just returned from India, where I had recently ascertained that protozoal parasites, like higher parasites, may be "metaxenous," that is, may live in two alternate species of "hosts"—in other words I had shown that certain parasites of malaria of men and birds reach their "definitive stage" in certain species of mosquitoes (see also Science Progress, April 1917, pages 669–672, and April 1919, pp. 636–7). This meant, not only the finding of the exact manner in which malaria is communicated from man to man, but also how the disease may be prevented on a large scale. The various guilty species of mosquitoes could now be easily identified by feeding them experimentally on cases of malaria, and then, when we had studied their habits by the usual means, we should be able to control them and the disease carried by them wherever we wished to incur the comparatively small expense required. On the voyage home in 1899 I had said to myself, "In two years we shall stamp malaria out of every city and large town possessing Health Officers and Sanitary Departments in British possessions." For the breeding-places of the Anopheline mosquitoes consist generally of small pools or puddles of certain types, mostly easily manageable by ordinary coolies instructed by sanitary inspectors, or, in many other cases, by such minor engineering work as any municipality or town, or even village-council, can do. I had spent years of toil on the subject; not because of its interest (because I prefer other pursuits) nor for the sake of "pure science," but in order to relieve human suffering. My success had been achieved by what I think was almost a miracle of luck; and now I gloried in imagination over what the world was going to get for it all. Think of it: a disease which kills its millions a year and torments its hundreds of millions!

This was not the dream of a visionary. I had served for eighteen years in the Indian Medical Service, for more than two years of which I had been employed by Government to reform the sanitation of a great Indian station—hard, plain, practical work. I knew all about sanitary organisation, drains, town-cleansing, sanitary engineering, houses, yards, sewers, official procedure, and the rest of it; and I had been studying the habits of mosquitoes for many more years. The thing could be done—almost everywhere: wherever there were a white man to command and a few coolies to work: a little work every day, a few extra pounds on the estimates, a trained engineer and a larger sum occasionally, some good town ordinances, and a man who was not a fool to co-ordinate measures and to keep statistics. A word

from the head governments—from the dozens of commissioners, administrators, governors, and other well-paid "proconsuls," from the India Office, the Colonial Office, the Foreign Office—could set the whole machinery in motion (by telegram) within a few months. In a few more months, perhaps in a year, or in two years, the death-dealing pests would begin to fall under control, would begin to diminish, even to disappear entirely in favourable spots; and with them, slowly, the ubiquitous malady would fly from the face of civilisation—not in this town or that town, nor in this or that colony, but almost everywhere throughout the British Empire—nay, further, in America, China, Europe, and the isles of east and west. Not disappear entirely, of course (an impossible ideal at the time), but be banished from the most crowded centres of civilisation.

Men of science, and indeed all humanitarians, should know what really happened. There was no doubt about the discovery. In 1898 my work was confirmed by the great Robert Koch and by Dr. Daniels, and was pirated by distinguished Italian writers. Almost every detail of the life of the parasites of malaria in mosquitoes had become known; and I had infected healthy birds in Calcutta by the bites of mosquitoes in 1918, and the Italians had subsequently infected healthy men in Rome. My work had been published by myself officially in India, and by Dr. Manson in England; and I had reported my new method for reducing malaria to the Government of India. The habits of mosquitoes were well enough known to justify an immediate attack upon them; and in 1899 there was no earthly reason why such an attack should not have been commenced everywhere. On arrival in England I lectured on my new method and then set out at once for West Africa to try to put it into practice. Hurrying back, I described the whole situation in four articles in the *British Medical Journal* (1899, vol. ii.), published a Report, gave more lectures, and wrote more articles. In 1901, finding that nothing was being done, I went again to West Africa (at my own expense) to give an object lesson on my method with £2,000 supplied to us by a philanthropist of Glasgow for the purpose; and in 1902 revisited Sierra Leone to see how the work was progressing. Between these visits I wrote two little books on the subject, gave innumerable lectures, refuted innumerable people who "exposed" us in the daily press, and argued with professors who assured us that it was as absurd to try to reduce mosquitoes as it would be to try to reduce the atmosphere. I visited high officials, wrote to Secretaries of State, enlisted the aid of several great ladies, and even made laborious mathematical studies of the subject—see my *Prevention of Malaria* (Murray).

The only result was that at the end of the two years, by which time I had hoped for so much, the enlightened municipalities of Calcutta and Freetown had each employed one native, at a salary of about £1 a month, to remove all their mosquitoes; but as the result was disappointing they soon stopped this extravagant expenditure; and, instead of it, Freetown gave £500 a year as salary to a local medical man for general sanitary work without the removal of the mosquitoes—which were the main cause of its sickness! But in 1901 the Americans discovered that yellow fever also is carried by mosquitoes, and, unlike the plantigrade British, immediately attacked the insects in Havana, with the result that that disease was entirely banished there and malaria was largely reduced. In the same year Sir William MacGregor, advised by his capable Senior Medical Officer, Dr. Henry Strachan, commenced similar work at Lagos, where I visited him; and Dr. Malcolm Watson began his admirable campaign (still being continued) in the Federated Malay States. In 1902 I was invited by the Suez Canal Company to rid Ismailia of malaria; I went there with Sir William MacGregor; and in a year the disease was banished by the labours of no more than four workmen (as Dr. Pressat has stated)! But after this the world

thought it would rest from its labours and did nothing more for a long time—except some good work at Hongkong, Khartum, and Durban. In 1904 I went to Panama in order to advise the Americans regarding the Canal, which was then being started—and the subsequent work under Surgeon-General Gorgas is famous. In 1906 I visited Greece for the Lake Copais Company; and, at last, in 1907, a British Colony, Mauritius, invited me to get their house in order (this action was due principally to the French planters there). In 1911, I attended a medical congress at Bombay, where a number of smart young men proved to me with some contumely that the reduction of mosquitoes is always an impossibility. Lastly, in 1913, the Colonial Office actually sent me to Cyprus, where the malaria has been steadily diminished by Dr. Cleveland ever since. Then the War began. It has been said publicly that I made my fortune by all these journeys. I may therefore tell those who are interested in rewards for science that for my three visits to West Africa I received not one penny and not even thanks; that for ridding Ismailia of malaria the wealthy Suez Canal Company gave me £100 for expenses in 1902 and nothing since; and that for the rest of my visits abroad I was paid a total of £1,200, less some out-of-pocket expenses. For going to Panama I received nothing; but I was robbed of £100 worth of greenbacks on arrival! I was able to continue the work owing entirely to receipt of a Nobel Prize from Sweden in 1902 (for my previous work in India).

What was the cause of the delay? The machinery was already in existence in 1899. All that the innumerable medical and sanitary officers had to do was to read the pamphlets, articles, and directions, to ask for the small funds required, and to set to work. If they failed, there were numerous Commissioners and Governors and Sanitary Councils to provide funds and to urge them on; and if these failed, there were the India Office, the Colonial Office, the Foreign Office, and Parliament itself, whose duty it was to see that such work was done. And all this time the people of these innumerable towns were suffering or dying of the disease, which, moreover, was causing an enormous waste of money, was paralysing many industrial pursuits, and giving whole countries an evil reputation. But no, almost nothing was done. I call this the Great Default; and I resent it—not for myself, because it is nothing to me personally—but for the sake of the thousands upon thousands who have suffered because of it.

The proper thing for a competent Government to have done in 1899 was to have formed at once a strong interdepartmental committee to deal with the new method of malaria prevention throughout the Empire, and to have employed me and other experienced sanitary officers to carry out the work; and I have no doubt that if this had been done an immense saving of life would have been effected. I tried in vain to get a hearing for the proposal. Instead of such a body the usual futile advisory committees were appointed, consisting of men who had no real knowledge of the subject and cared nothing for sanitary matters, and who did practically nothing. At that time, moreover, there were a number of people who had been little heard of before but who were making quite a good thing out of my work—and who opposed everything I suggested. They advised that instead of reducing mosquitoes it would be better for everyone in malarious areas to take quinine *ad infinitum*, or to protect their houses with wire-gauze, or to wear veils, gloves, and "mosquito-boots" (*sic*—in the tropics!). But not content with this some of them suddenly brought up a resolution at a committee of the Royal Society, demanding an enquiry (managed by them) into my gratuitous work in West Africa (which they had done nothing to help) and suggesting that some of the money given for our object-lesson had adhered to our fingers! Fortunately we had kept all the accounts and vouchers, and Mr. Joseph Chamberlain quashed the "enquiry" at sight

on receipt of a personal letter from me. That is all that I ever received for showing the West African Colonies how their malaria is carried and how it should be reduced—needless to say, I never went there again. But, although he helped me on that occasion, Mr. Chamberlain (then head of the Colonial Office) was never allowed to see the practical bearing of my suggestions; and when, at a deputation to him, I begged him to appoint Sanitary Commissioners for West Africa on the usual public-health model, to see that real sanitary work was being done there, he gave the typical politician's reply that he did not care to appoint spies (*sic*) over the work of his local officers! So much for the Colonies. As for India, it had done almost nothing since I left it in 1899 except to conduct a bogus experiment at Mian Mir (Lahore) to prove that mosquitoes could not be reduced, or that, if they could, their reduction would not affect the malaria (though it was admitted that the latter was due to the former!). The fact was that mosquito reduction was unpopular because it forced local sanitary staffs to work, and local governments to expend some small funds. Money spent on reducing the death-rate has little to show for it in comparison with money spent on new post-offices, hospitals, or colleges—to which the names of great local administrators may be attached as a perpetual memorial. It was always easier, not to spend money on mosquito-reduction, but to issue instructions to the people adjuring them to take quinine, protect their windows, or wear veils: this would show that Government was doing something and would save them money at the same time. Of course the people would pay no attention whatever to the advice—but that didn't matter. . . . Well, at last I determined to make a final appeal to the head of the India Office in London himself. I spent an hour alone with him pleading my cause on behalf of the million people who are said to die of malaria every year in India alone, and of the millions more, mostly children, who suffer from it. He sat before me like an ox, with divergent eyes, answering and asking nothing. Of course he did nothing. He was the personification of the British nation in the presence of a new idea; and as I left I could almost fancy seeing the prophetic handwriting on the wall over his head, *Mene, mene, tekel, upharsin.* A little later the Americans abolished malaria from the whole Panama Canal Zone, chiefly by my methods, and thus won the real honour of having been the first to develop a great new sanitary measure. The British have many good qualities, but also grave defects, which, if not corrected, will certainly lose them that hegemony of the world which their forefathers earned for them.

Sir William MacGregor was not a politician but a genuine administrator, careful of the real interests of the peoples entrusted to him. The only medical man who has been a British governor of recent years (and we ought to have many such governors), he was aware of the superlative value of sanitation in the development of a colony. Wise, grave, but humorous, bearded, thick-set, with wrinkled forehead and a high and somewhat conical bald head, his kindly manner and low voice filled all with trust in him. He drank no wine and did not smoke; but was no fanatic in these respects, and kept an hospitable table. Every night he read from his Greek Testament, and was also skilled in French and Italian and in several indigenous languages. He was a mathematician, a practised surveyor, a lapidary, and a master of many arts, but always proud of his medical upbringing and of his nationality. When I visited him at Lagos during my second visit to Africa in 1901 he himself accompanied me on my inspections, in order to understand what I proposed to do. Dressed in a kilt and solar topee—a somewhat incongruous mixture—he introduced me politely to the large black ladies keeping market stalls in the bazaar; because he wished to start a "Ladies' League" against malaria in Lagos. Indeed, he gave us a great luncheon to inaugurate the League, where the coloured "quality" made

very merry. He ordered all the Europeans in the Colony to take quinine regularly and nearly poisoned himself twice a week with a large dose in order to set an example. Counselled by his able Chief Medical Officer, Dr. Henry Strachan, he took up my teaching with enthusiasm and wide understanding; and next year accompanied me to Ismailia on the Suez Canal, where the first and most striking success of "mosquito-reduction" was obtained. I remember that when we were at Cairo he wished to buy a genuine ancient scarab. A dealer showed him a number on a tray; but he took up one of them and exclaimed, "How very odd! Is this a genuine scarab? Why, it is made of a kind of stone which is found only in Australia!"

Unfortunately, Sir William's health broke down in Lagos, and he was promoted to Newfoundland. But he always helped me with his influence, and when he went to Australia he founded the Queensland Institute of Tropical Medicine there. Owing to him and to Colonel Seely I was at last made a member of certain medical committees of the Colonial Office, from which my friends had hitherto taken care to exclude me; but I resigned them later for the reason given in SCIENCE PROGRESS for July 1916, p. 147. He also endeavoured to obtain a Colonial Governorship for me, so that I might give effect to my sanitary methods—of course, without avail. He was keen to become a kind of Imperial Sanitary Commissioner for malaria after his retirement from Queensland. I saw him frequently before he died; and we concluded that we had done what we could, that our great schemes were beyond the vision of poor Mr. John Mule, and that the future of malaria prevention had now become a part of ordinary sanitary work and must be left to the competence or incompetence of local authorities. Some advances are now being made, but not a tenth of what ought to be done. I have just received a report on the subject regarding Freetown, Sierra Leone, where I taught them exactly what to do in 1899—and we call ourselves a civilised nation! To learn what fools men may be, show them how to save their own lives.

In the whole of this work the British medical profession has given practically no help at all. Dominated by persons of small consequence, it possesses little power or influence in the country and fails to insist upon necessary reforms. Thus Laveran's discovery of the microscopic parasite of malaria was neglected as a means of routine diagnosis for twenty years; and even to-day thousands of soldiers suffering from the disease have often to be treated by doctors who have never been properly instructed about it, and who, apparently, seldom read the literature. In my opinion medical degrees and diplomas should be renewable by examination every ten years or so. Sir William MacGregor was, I think, the only high British official who ever grasped the real importance and signification of the general anti-malaria scheme which I proposed in 1899.

The fact that malaria is carried by mosquitoes is one of prime importance for civilisation, now and hereafter; and we should have expected that an empire which always lauds itself for its humaneness and practical good sense would have done more to utilise it. Moreover, the empire paid nothing for the discovery in the making and has hitherto refused to pay anything for the boon when it was obtained—see SCIENCE PROGRESS, January 1914, and October 1915. Altogether a melancholy picture of incompetence and ingratitude. More noble, therefore, by contrast, that figure of the last of the medical Governors.

ESSAY-REVIEWS

LOGIC AND MATHEMATICS, by the late P. E. B. JOURDAIN, M.A.: on—

- **Lectures on the Philosophy of Mathematics,** by JAMES BYRNIE SHAW. [Pp. viii + 206.] (Chicago and London: The Open Court Publishing Company, 1918. Price 6*s.* net.)

An Introduction to Mathematical Philosophy, by BERTRAND RUSSELL. [Pp. viii + 208.] (London: George Allen and Unwin, Ltd.; New York: The Macmillan Company. Price 10*s.* 6*d.* net.)

THE object of Prof. Shaw's *Lectures* "is to consider the whole field of mathematics in a general way, so as to arrive at a clear understanding of exactly what mathematics undertakes to do, and how far it accomplishes its purpose; to ascertain upon what presuppositions, if any, which are extra-mathematical, the mathematician depends" (p. vi). With this end in view, Prof. Shaw describes in some detail the subject-matter of mathematics and the predominant characters of the objects with which mathematics concerns itself. He finds that the objects it studies are numbers, figures, arrangements, propositions and propositional functions, operations, hypernumbers, processes leading to transmutations, and deductive systems; the chief characters of the objects are those of form or structure, invariance, functionality, and inversion. But no one of these different kinds of objects or principles—or even the sources or methods or regions of validity of mathematics, which we shall mention afterwards—can furnish a satisfactory definition of mathematics which would include the entire subject (pp. 154, 6–11).

The second chapter discusses number and the arithmetisation of mathematics. The five stages in the development of the idea of number are the invention of integers, of fractions, of incommensurables, of what Prof. Shaw calls the "ensemble" (p. 24), and of the very general ranges that appear in the work of Fréchet, Moore, and Volterra (p. 29). "For our present purpose it is simply sufficient to cite these [last] investigations, in which physical intuition is helpless, to prove our general thesis that mathematics is a creation of the mind, and is not due to the generalisation of experiences or to their analysis; nor is it due to an innate form or mould which the mind compels experience to assume; but is the outcome of an evolution, the determining factors of which are the creative ability of the mind and the environment in which it finds the problems which it has to solve in some manner and to some degree" (p. 30). Every one of the different branches of mathematics leads to the same conclusion, as is shown in chapters iii–ix, which are respectively on space and the geometrisation of mathematics—this geometrisation being "that every problem of analysis has a geometrical interpretation, and every problem of geometry may be formulated analytically" (pp. 38–9); arrangements of a group of objects, and mathematical tactic; logistic and the reduction of mathematics to logic (which has already

been noticed in Science Progress, 1918, **12**, 541); operators and the reduction of mathematics to algorithms—to purely arbitrary modes of combinations of operators (p. 95); hypernumbers and the reduction of mathematics to algebra—to statements in terms of hypernumbers (p. 113); the reduction of mathematics to "processes"—to modes of transition from a given object or arrangement of objects to another object or arrangement of objects (p. 117); and "the active exercise of the thinking function of mind" (p. 122) as shown in systems of inferences. Chapters x–xiii find that mathematics cannot be defined by reference to any particular one of its four distinct and independent principles: form (cf. pp. 9, 130, 132, 135), invariance (cf. pp. 9, 140–1), functionality (p. 146), inversion—the consideration of problems leading to the creation of new mathematical entities, inversion being the principle of "creating a class of objects that will satisfy certain defining statements" (p. 152; cf. pp. 9, 149). The last three chapters are inquiries into the source of the reality that is in mathematics, its method of arriving at truth, and the realm of validity of its conclusions.

On the whole it is, I think, impossible not to assent to Prof. Shaw's conclusion that it is impossible to reduce the whole of what most of us mean by "mathematics" to any one of the categories marked off by him. It is only by tacitly limiting the significance of the word "mathematics" to the subject-matter alone that Russell and others came into conflict with Poincaré, who tacitly supposed—like most of us—that "mathematics" denotes something which includes certain mental activities, and which has thus a life and a history and contains methods of discovery as well as logical subject-matter. But, while Prof. Shaw seems right in one of his main contentions, I cannot help feeling that his distinction between a "statical" and a "dynamical" part of mathematics (pp. 8, 117, 126) rests on an analogy which is misleading: a transmutation, for example, can be defined as a class of relations in as "statical" a way as an irrational number. Also the point of the book is not, I think, made any clearer by the extraordinary rhetoric (cf. pp. 1–8). For example, we read: "Could a powerful telescope show us the antipodes, or could an electron wind its tortuous way according to a law expressed by the Weierstrass non-differentiable function?" (p. 3).

On pp. 25–7 there is a mistaken attempt to discredit Cantor's proof that there are more irrationals than rationals, by pretending that the proof depends on the purely arbitrary assignment of an infinite set of coefficients. Cantor's proof of 1892 explicitly avoided this arbitrariness, and, of course, is not affected by the other "objection" (p. 27) that one and the same infinite collection can be arranged to correspond completely *or not* with one another collection. What Cantor proved is that, where R is *any* one-one correlation of a collection M with the collection of parts of M, there always can be defined, as a function of R, a definite part of M to which no member of M is correlated by R.

If, indeed, Russell alone—as seems a mistake which Prof. Shaw shares with many people—is to be credited with the theory that mathematics is capable of a reduction to logical terms expressed by symbols of "logistic," it is not difficult to understand the dislike which responsible mathematicians have to this "logistic" in view of the frivolous and unedifying remarks on truth and falsehood published by Russell in 1904, and quoted with deserved scorn on pp. 162–3. It is hardly to be wondered at that no attempt should have been made to settle clearly the merits of the dispute between Poincaré and Russell.

In former days, such subjects as the nature of infinity and continuity belonged to philosophy: now, thanks to some mathematicians of the last half of the nineteenth century, they belong to mathematics. The logically exact definition and investigation of such fundamental concepts as number,

mathematical space, infinity, continuity, and serial order, has led Georg Cantor, Dedekind, Frege, Peirce, Schröder, Peano, and others, to results among which some found by Dr. Whitehead and Mr. Russell are probably permanent; and to which, in the second book under review, Mr. Russell attempts to give a brightly written and popular introduction.

By "mathematical philosophy" Mr. Russell apparently means what other people have called "the philosophy of mathematics," and, on the whole, the name used by these others seems preferable: there is nothing in Mr. Russell's philosophy, apart from the applications, that strikes one as particularly *mathematical*. Even the elaborate symbolism used in Dr. Whitehead and Mr. Russell's large *Principia Mathematica*, which looks something like the usual symbolism of mathematics though it has a different purpose, is not used in the present volume, while a philosophy of mathematics need not be mathematical any more than a philosophy of art need be artistic. The purpose of the Russell-Whitehead symbolism—an attempt to combine the advantages of those of Peano and Frege—is quite different from that of mathematical symbolism. In fact, "mathematics is a study which . . . may be pursued in either of two opposite directions. The more familiar direction is constructive, towards gradually increasing complexity; from integers to fractions, real numbers, complex numbers; from addition and multiplication to differentiation and integration, and on to higher mathematics. The other direction, which is less familiar, proceeds, by analysing, to greater and greater abstractness and logical simplicity; instead of asking what can be defined and deduced from what is assumed to begin with, we ask instead what more general ideas and principles can be found, in terms of which what was our starting-point can be defined or deduced. It is the fact of pursuing this opposite direction that characterises mathematical philosophy as opposed to ordinary mathematics" (p. 1).

The book consists of eighteen chapters, which deal, in a somewhat popular way, with the following subjects respectively: Peano's derivation of the whole theory of the natural numbers from three primitive ideas ("zero," "number," and "successor"), and five primitive propositions in addition to those of pure logic; Frege's (1884) definition of number which was rediscovered by Mr. Russell in 1901; definitions of all of Peano's primitive ideas in logical terms, and considerations on finitude and "mathematical induction"; the essential characteristics of a relation which is to give rise to "order," and a suggestion of a generalisation of serial relations (p. 41); various kinds of relations; ordinal "similarity" of relations, and "relation-numbers"; logical definitions of rational, real, and complex numbers; the theory of transfinite cardinal numbers as it results from a combination of the discoveries of Georg Cantor and Frege; more general consideration of infinite series, ordinal numbers, and Alephs; the (ordinal) notions of *limit* and *continuity*; the same notions as applied to *functions*; selections and the "multiplicative axiom"; Mr. Russell's "axiom of infinity" and his theory of "logical types"; the theory of deduction; propositional functions; descriptions (the word "the" in the singular); classes ("the" in the plural); and the relation of mathematics and logic.

Since Mr. Russell and Dr. Whitehead began working (1900) at the reduction of mathematical conceptions to those of logic, they have steadily become more and more symbolical, in the narrower sense, and less and less capable of expressing matters about the principles of mathematics in ordinary language. The present volume seems to mark a culmination of this incapacity. For example, by a "proposition" is meant "a form of words which expresses what is either true or false" (p. 155); and, on the same page, a "propositional function" is also said to be an "expression." But, on the very next page (cf. pp. 157 ff.), Mr. Russell seems to find his restriction so awkward that he speaks, as we all do, of a proposition as being true or false. This is

gross carelessness; it is like the habit of those mathematicians—whom nowadays we would not call precise—who used to say that a "function" is an "expression." These careless people were rightly corrected by Frege, and it is inexcusable for any modern writer on the logical principles of mathematics to revert to pre-Frege errors and confusions. Many years ago Mr. Russell himself, in *Mind* for 1906, criticised this error in H. MacColl; when Whitehead and Russell's *Principia Mathematica* began to appear in 1910, Mr. Russell acknowledged the justice of the present writer's strictures in the *Cambridge Review* on a crop of similar errors, but said that they were all due to Dr. Whitehead; now Mr. Russell himself repeats (for at least the third time) some of these errors.

The book does not contain much that is new except disavowals. There is a disavowal (in the note on p. 203) of a fundamental characteristic of Dr. Whitehead and Mr. Russell's *Principia Mathematica* which, it may be necessary to add, imitates a book of Newton in title, but in title alone. And there is a disavowal of the method which Mr. Russell used in his *Principles of Mathematics* of 1903 to show that there are an infinity of things in the world of mathematics. This "proof" of 1903 was, of course, fundamental to the whole attitude Mr. Russell then adopted towards the question of infinity; but "the first thing to observe is that there is an air of hocus-pocus about it [this argument]: something reminds one of the conjurer who brings things out of the hat" (p. 135). The argument relied on forming the complete set of individuals, classes, classes of classes, etc., but now Mr. Russell's "theory of types" forbids us to put together individuals, classes, etc., which are of different "logical types." So Mr. Russell parts company with his former self—and also, it may be mentioned, with Plato (p. 138). It is apparently what Mr. Russell calls "a robust sense of reality" (pp. 135, 170; cf. p. 169) that has led him to this theory. Indeed, Mr. Russell remarks with some approval that "the reader will feel convinced that it is impossible to manufacture an infinite collection out of a finite collection of individuals, though he may be unable to say where the flaw is in the above construction" (p. 135). But Mr. Russell's process for preserving part at least of the numbers of ordinary mathematicians strikes one as quite as much of a conjuring trick as his former method. In the first place, of individuals there may be only a finite number or even none at all (p. 134). Then we have another type (classes of individuals); then another type (classes of classes), and so on. We manufacture all these types out of classes or classes of classes . . . of no individuals, and get any finite number we wish, however large, by simply going up the hierarchy of "logical types." But, then, "classes" are (p. 182) said to be fictions, even if there are individuals, so that it is difficult to see how numbers at all can be preserved if they are only to belong to fictions, or fictions of fictions, or so on—perhaps even the more so as there may not be any individuals to give rise to such fictions. The case as regards individuals comes to a very lame conclusion. Mr. Russell is finally reduced to defining "individuals" (p. 142) by means of the kind of symbols by which they are symbolised, in order to avoid a plunge into metaphysics. This would be about as satisfactory as to define a species of animal by the name we may give to it, so that the species might vary if one had a cold in the head. The curious belief in the power of the word is also shown when a *proposition* is defined, as has already been remarked, as a "form of words," even though Mr. Russell finds it convenient to speak of propositions as *true* or *false*. Mr. Russell has sometimes enthusiastically praised Humpty-Dumpty's way of treating words, but it seems that in his later works he has fallen far behind Humpty-Dumpty.

To sum up, if some theory of "logical types" is necessary, most mathematicians would be grateful if the grounds were stated, not as Mr. Russell has stated them here, but convincingly, undogmatically, and freed from

appeals—which have been shown to cut both ways—to a "robust sense of reality."

Mr. Russell assures us (p. v) that he has made "the utmost endeavour to avoid dogmatism on such questions as are still open to serious doubt." However, on p. 117, he asserts that the "multiplicative axiom . . . can be enunciated, but not proved, in terms of logic." If Mr. Russell's boast (p. 167)—which seems to mean that his love for truth caused him to rise superior to the discomforts of a prison—is not humbug, it is impossible to account for the fact that the proof of this axiom in SCIENCE PROGRESS (1918, **13**, 178, 299–304) should be overlooked. Dr. Whitehead and Mr. Russell had both seen this proof—an amended form of an earlier attempt which was defective through a mistaken belief that a certain essential link, which happened to be known to the author of the above paper, had been already supplied by F. Hartogs, and that therefore it was not necessary again to supply this link—in July 1918—while this book was yet unwritten, or, at least, while it was yet in manuscript. The criticisms made on both Dr. Whitehead and Mr. Russell were obviously based on inattention, and were naturally superficial: there was a total disregard of the fact that an unambiguous rule was provided for arranging the "chains" of M "in classes of direct continuations," in such a way that there cannot be a chain of M which continues *all* of the chains of any particular class of direct continuations; and also that, for each class *of direct continuations*, it is obviously possible to conclude that if the class has chains respectively of all types less than ω, for example, M has a chain of type ω. In most cases there would be no grounds to find fault with either Dr. Whitehead or Mr. Russell for ignoring a particular piece of work; but in this case the work was a serious attempt, against which there have been advanced no sound or even intelligent criticisms, which would make superfluous large parts of Dr. Whitehead and Mr. Russell's own *Principia Mathematica*, and would also give a necessary foundation, which neither of the authors mentioned could discover, for Dr. Whitehead's chief results on the arithmetic of cardinal numbers. It cannot, then, it seems, be denied that Mr. Russell has allowed other motives to interfere with his love of truth. Mr. Russell once remarked that he loved his country, but loved the truth still more: paradoxical as it may seem in view of recent action under the Defence of the Realm Act, it is, perhaps, the case that Mr. Russell loves his country more than he loves the truth.

That limits and continuity of functions can be defined purely ordinally, whereas they are usually defined in terms involving number, is stated (p. 107) to have been shown by Dr. Whitehead. The facts of the case are these: In 1902 the present writer communicated to Dr. Whitehead a definition of a continuous function which had certain very great advantages for purely ordinal treatment. In his book of 1903 Mr. Russell remarked that continuity of functions necessarily involves number; and so the present writer developed his purely ordinal treatment of continuous functions in one of the best-known mathematical journals—*Crelle's Journal* for 1905. Copies of this paper were sent to Dr. Whitehead and Mr. Russell; and up to 1912 the present writer published much, extending the scope of purely ordinal conceptions in various other parts of mathematics. In 1913 Dr. Whitehead and Mr. Russell published a purely ordinal definition of a continuous function, without mentioning any other work. This definition was practically the same as the one published by the present writer eight years previously, and is the definition referred to by Mr. Russell in the above passage. The matter is not of great importance except that it throws doubt on the good faith of Dr. Whitehead and Mr. Russell.

Lastly, on pp. 203–5 there are conclusions as to the logical nature of mathematics which would provoke a smile. This is mainly owing to the

choice of a word to describe "the characteristic of logical propositions"—namely, "tautology." The present writer cannot help feeling that the emphasis on the tautological aspect of mathematics arises from Mr. Russell's present attitude towards infinity: he tends to be unduly sceptical about infinity, much as he was in 1896, when he hastily denied the validity of Cantor's work. There is, it seems, a failure to grasp what was sound in the contentions of such men as Poincaré, who maintained that the infinity in the principle of mathematical induction avoided tautology.

This book is a lamentable performance. Lamentable because many of us have a vivid recollection of good service which Mr. Russell has done in the past to the cause of logic, before he took to journalism in 1912.

The book is pleasantly written. But if the symbolic language of Dr. Whitehead and Mr. Russell really does express anything that is both true and not trivial, that Mr. Russell has utterly failed to express in ordinary words.

THE PALÆONTOLOGICAL HISTORY OF THE GYMNOSPERMS on: Fossil Plants. A Textbook for Students of Botany and Geology. Vol. 4, Ginkgoales, Coniferales, Gnetales. By A. C. Seward, M.A., F.R.S., Master of Downing College, Cambridge. (Pp. xvi + 541, with 190 illustrations.) (Cambridge: at the University Press, 1919. Price £1 1s. net.)

The fourth volume of Prof. Seward's monumental book on Palæobotany is the final one of the series. On its title-page, however, it reveals the disappointing fact that the work ceases with the Gymnosperms, and it does not touch the Angiospermic flora at all. As the present-day flora which catches the eye consists almost entirely of members of the Angiospermic family, it may seem as though it were impossible to complete a book on the history of plants and leave out the flowering plants. Nevertheless, owing to the nature of the material on which the science is based, and owing to the comparatively short history of the flowering plants (which did not appear until well on in the Cretaceous), the reputable data of Palæobotany are almost all to be found concerning the Coniferæ and lowlier groups.

The author himself says: "It is with a keen sense of the incompleteness of my task that Volume IV is concluded without any attempt to deal with the abundant, if, in very many cases, undecipherable records of Angiosperms. . . . What is needed . . . is the co-operation of trained systematists." The difficulty, however, in obtaining this co-operation of trained systematists is that the modern botanist bases his classification on the floral details and fructifications of plants, whereas one may safely say that 95 per cent. of the remains of fossil Angiosperms consist of foliage leaves in varying degrees of incompleteness. The points on which classification must therefore depend are primarily venation and the marginal outline of the leaf. By an unfortunate chance even the "cuticle preparations" which are now so useful, particularly in specimens from the Lower Mesozoic, are extremely rare for the Higher Mesozoic and the Tertiary deposits containing dicotyledonous and other leaves, and they have not yet been used with any practical effect in work on dicotyledons. In recent years the only output of work of any magnitude on the Angiosperm fossil flora comes from America, where the deposits are of first-rate geological importance; but we do not lack excellent fossil material in this country, were workers in this field to arise. By the untimely death of Clement Reid, research on the late Tertiary Angiosperms was prematurely cut off, and the earlier Tertiary collections in this country are still lying as undescribed specimens awaiting the research worker to handle them. However, in the middle of last century, much has been published by various European scientists, such as Heer, Ettingshausen, and others, so that there will be a very voluminous critical work awaiting

one who must some day arise to deal with the whole question of fossil dicotyledons.

Our author leaves this theme alone, for he modestly considers himself inadequately equipped to attack it; but we feel that such a work from his pen, even if not ideal, would be of great use, and that he is indeed sufficiently equipped for this task, although he may not feel an interest acute enough to drive him to undertake such a toilsome labour of love.

The present volume appeals pre-eminently to palæobotanists and botanists, but geologists should be glad to turn to it for records of genera with which they must from time to time deal in various geological horizons. To anyone engaged upon research in any of the three above-named sciences the possession of this text-book is essential.

The specialist might be tempted in every chapter to discuss, and perhaps to criticise, the detailed arrangement, the nomenclature, or the proportionate space allotted to each genus. Detailed criticism, however, is out of place in the present review, the main theme of which must be congratulation to the author on the accomplishment of his very laborious and exhausting work, and thanks to him for the service which he has rendered to the science.

Although a textbook, one finds in its pages quite a number of species and genera described or named for the first time. Some of these, of comparatively minor importance, are adequately dealt with in the few lines allotted to them. Sometimes the fact that they are new species has tempted the author to give them space a little out of proportion to their importance. Among the new generic names several are created for the renaming of already described and known genera, such for instance as *Mesembrioxylon*.

With Prof. Seward, we think that it is well to drop Gothan's two-genera *Podocarpoxylon* and *Phyllocladoxylon*, but one doubts whether the newly instituted *Mesembrioxylon* will "catch on." It carries with it the haunting suggestion of quite another affinity, and, as our author points out, the anatomical characters of the plants it covers are particularly difficult to handle.

Until Palæobotanical science, however, is very much more advanced than it is at present, the terminology must depend on the view-point of the individual workers who describe and deal with details so minute as to be in general overlooked by the recent botanist, who has the much easier task of classifying complete material.

Professor Seward is on the whole sane, and not too much of a purist as regards nomenclature, although in one or two places he tends to use terminology in a way which one hopes will not receive universal acceptance. For instance, his rechristening of various fossil woods and his eliminating the useful name *Araucarioxylon* would seem to impoverish terminology.

Almost every important fossil plant in the families covered by this volume finds a place in it. One exception to this, however, is noted in the absence of the beautiful Japanese species from that interesting genus *Prepinus*, of which only two other species are known.

Owing to the unsatisfactory nature, and the comparative rarity of good impressions of the foliage, a very large proportion of the fossil members of the Gymnosperms are represented by petrified anatomical specimens, in particular of their woods. A large section, therefore, of this book deals with the subtle details of Gymnospermic wood anatomy, and discusses usefully the structural points on which specific determinations can be made.

The writer says very truly: "No branch of palæobotanical research makes greater demands upon the patience and self-control of the student," and yet, as Prof. Seward goes on later to say, "The main point is that the student cannot afford to neglect this line of enquiry if he desires to obtain a comprehensive view of the changing combinations of structural features preceding their distribution among existing genera."

As the botanical world well knows, the American school, headed by Jeffrey and his students, tends to maintain the rather startling view that the Abietineæ preceded the Araucarineæ in evolution. As the details given in Prof. Seward's text-book clearly demonstrate, the facts both of geological distribution in time and the structural details of the woods of the different epochs demonstrate the remoteness of the possibility of such a view receiving enduring support.

Those who deal with recent plants can no longer afford to dogmatise or generalise on plant distribution or family history, and at the same time to ignore the results of Palæobotany. The disastrous effects of such an omission to dig their foundations in the rocks of the past has been very evident in certain recent and popularly accepted theories of distribution, which display a lamentable ignorance of the true histories of plant groups, and would never have received even the temporary recognition which they have had, had the botanical public been educated adequately in the fundamentals of plant history. Prof. Seward's book will do much to further such an education.

REVIEWS

Modern Science and Materialism. By HUGH ELLIOT. [Pp. viii + 211.] (London: Longmans, Green & Co., 1919. Price 7*s*. 6*d*. net.)

THE rapid strides of modern science are largely due to specialisation and division of labour. Specialisation, however, has its defects as well as its virtues. One may fail to see the wood for the trees. Yet man owes it to himself to take his bearings in the vast universe in which he lives. Implicitly, perhaps, each man has a philosophy of his own. Unfortunately it is not always closely related to the science of his day, not even when he himself is a man of science. It is obviously desirable once in a way to piece together the main results of the principal sciences, and to sketch in broad outline the general pattern of the world as it would present itself to a scientist who took the whole world to task. In *Modern Science and Materialism,* Mr. Elliot has attacked this extremely difficult task of looking at the universe as a whole from the standpoint of recent science, and he is to be congratulated both on his boldness in making the attempt and on the measure of success which has crowned his enterprise.

The first four chapters of Mr. Elliot's book give a lucid and interesting survey of the more comprehensive theories of modern stronomy, Physics, Chemistry, and Biology, in so far as they have an obvious bearing on a philosophical orientation. To the majority of ordinary readers these chapters will probably prove the most helpful. The author himself presumably attaches more importance to the last two chapters, on Materialism and Idealism, in which he expounds his own philosophical ideas. But to the present reviewer, Mr. Elliot's discussions of psychological and philosophical problems do not compare favourably with the rest of the book. The last chapter in particular is not sufficiently clear to be judged adequately, and not satisfactory in so far as it can be judged at all. What is clear is that Mr. Elliot is utterly opposed to Spiritualism, while he is prepared to come to terms with most of the other philosophical " isms." His aim apparently is to effect some kind of fusion or reconciliation between Agnosticism, Materialism, Idealism, Monism, and Sensationalism by means of various compromises. To Agnosticism he concedes that a complete knowledge of the universe would require an infinity of senses, as against the meagre half-dozen which man possesses. To Materialism he allows that the final units of which everything (including the organic and the conscious) is composed are electrons subject to uniform mechanical laws. Idealism and Sensationalism are conciliated by the admission that all knowledge, and therefore all science, begins with sensations. But since these sensations are only reactions of complex molecules of protoplasm it is unnecessary to posit more than one kind of ultimate unit, and Mr. Elliot is therefore a Monist.

These bald data ought to be enough to make the reader turn to Mr. Elliot's book, which will certainly be found stimulating. For Mr. Elliot (notwithstanding his hostility to teleology) wrote his book with a purpose, namely, to promote " reconstruction " in the realm of ideas. Like Huxley before him, our author evidently believes that mankind will never find salvation until they learn to dispense with every kind of make-believe, and face every problem in the true spirit of Science.

A. WOLF.

MATHEMATICS

An Elementary Course of Infinitesimal Calculus. By HORACE LAMB, M.A., LL.D., F.R.S., Professor of Mathematics in the Victoria University of Manchester. Third Edition. [Pp. xiv + 530.] (Cambridge: at the University Press, 1919. Price 20s. net.)

THE new edition of Prof. Lamb's Treatise is extremely welcome. This work has for a long time past been regarded, by the majority of teachers of Mathematics, as the best account in our language of the Calculus as designed for students whose aim is less the study of its logical principles than the cultivation of a facility for applying its methods to problems which arise in the application of mathematics to other branches of Science. In this regard the book has never had a serious rival. We do not wish to imply that logical proofs have been neglected. For Prof. Lamb's treatise, embracing as it does the Differential and Integral Calculus and the more important types of Differential Equations, is quite unusually logical in its procedure, avoiding, at the same time, the repellent atmosphere suggested too often to a physicist or an engineer, by the attempt to give really rigorous statements of the underlying assumptions in theorems which he would often prefer to take for granted. We regard it as the only work on this subject which has as yet succeeded in striking the proper note, and the call for a new edition is a sign of the fact that those whose work involves applications of mathematics are beginning to a greater extent to be really interested in the subject itself, and to appreciate some of its beauty and generality without too close a study.

The work is too well known for it to be necessary to indicate its main outlines, and we may confine our attention to the main respects in which it differs from previous editions. At the outset, we may say that all the changes made are to the advantage of the book, having regard to its special purpose. The earlier treatment of the more algebraic questions, such as the properties of series and their convergence, has now been restricted almost exclusively to power-series. This is an advantage in that these are the only series with which the physicist or engineer is ordinarily concerned. This modification is of the greatest value, and is the most important difference introduced. Some of the more physical applications of integration, such as the determination of mass centres, have been deleted, being contained in other treatises on the special subjects since published by the author. But we are pleased to observe that the character of the illustrative examples, and of the problems proposed at the end of each chapter, has been preserved, for experience has shown that they constitute, for the elementary student, one of the fundamental merits of the book in a sense surpassed by no other work yet brought to our notice.

The new edition is, like those preceding it, in every way worthy of the best traditions of the Cambridge University Press.

DOROTHY WRINCH.

Unified Mathematics. By L. C. KARPINSKI, Ph.D., Associate Professor of Mathematics, University of Michigan; H. Y. Benedict, Ph.D., Professor of Applied Mathematics, University of Texas; and J. W. Calhoun, M.A., Associate Professor of Pure Mathematics, University of Texas. [Pp. viii + 522, with numerous diagrams.] (Boston: D. E. Heath & Co.; London: George G. Harrap & Co., 1919. Price 10s. 6d. net.)

IN this little book, the authors present a course of mathematics covering the elementary parts of algebra, trigonometry, and analytical geometry. The book is interesting as an attempt to weld together those portions of

the three subjects which have to be treated in elementary lectures at Universities into one coherent whole, while allowing the important differences of method between analysis and geometry to become apparent. This aim is indeed a worthy one, and a successful account of how unity of treatment could be attained in the teaching of mathematics suitable for pass students or students reading for honours in physics or economics or other sciences would be of very great value. It will be remarked that the calculus is untouched in this course. And this, indeed, appears to be the cause of some of the defects of the book. The authors in the preface remark that "no attempt has been made to introduce the terminology of the calculus, as it is found that there is ample material in the more elementary field which should be covered before the student embarks upon what may properly be called higher mathematics." In spite of this infinite series are introduced, and indeed are introduced without adequate explanation. The reason for this can doubtless be traced to the view that the notion of an infinite series presents serious difficulties to the elementary student. But the modern theory of limits is involved in so fundamental a manner in the theory of convergence and can be explained so simply that it seems a great mistake to arrange elementary courses so as to avoid its introduction. The remarks made about the geometrical series

$$1 + r + r^2 \ldots + r^n + \ldots \qquad r < 1$$

are all entirely correct, but do not seem to be full enough to meet the case of the student who is unfamiliar with the notion of a limit.

The treatment of complex numbers also seems to be unsatisfactory; i, defined as a number which multiplied by itself equals -1, is introduced at the very beginning. It is stated that complex numbers are combined according to the laws of numbers, and that the combination of any two or more complex numbers . . . gives a complex number. It is not clear whether these statements are postulated or derived from other postulates implicitly introduced in the (very slight) discussion of the geometrical interpretation. But if they are assumed, no hint is given why such results should be introduced. It is never made clear that i is introduced in order that the laws for complex numbers or ordered pairs of numbers may be formally the same as the laws for real numbers. For teaching purposes it seems desirable to discuss first the introduction of ordered pairs of real numbers and the way in which the four fundamental algebraic operations are to be defined; and only subsequently to consider the possibility of there being a meaning for i which will bring the laws of combination of ordered pairs of real numbers (x, y) (x', y'), (x'', y'') under the same formal treatment as numbers such as $(x + iy)$, $(x' + iy')$, $(x'' + iy'')$ . . . . To introduce i *ab initio* as a number which, multiplied by itself, gives -1, is to run the danger of losing the attention of the student, and of letting him assume from the beginning the existence of a number i which will make a uniform treatment of real and complex numbers possible.

Throughout the book there is a wealth of examples and illustrations. These, together with the clear treatment of simple trigonometrical problems, and of the elementary theory of two- and three-dimensional geometry, make the book as a whole (apart from the two important respects already mentioned) useful as an introduction of very simple mathematics to the student.

DOROTHY WRINCH.

A Non-Euclidean Theory of Matter and Electricity. By P. A. CAMPBELL. [Pp. 44.] (Cambridge, Mass.: University Press, 1907. Price 35 cents.)

THE prominent position which Einstein's theory of space and time has recently taken in the realm of scientific thought has resulted in the re-

appearance of various forgotten theories for which the authors claim some kinship with Einstein's fundamental ideas. In general, these are productions of the genus "crank," and are entirely devoid of logical development. The theory under review is not of this nature, in so far as it is an attempt logically to develop the consequences of certain postulates which are clearly stated at the beginning. In one respect the author may be considered to have anticipated Einstein. He assumes that the normal form of space is the Euclidean form; that the presence of matter causes a slight deviation from the homogeneous Euclidean form, and that this deviation is accompanied by appropriate forces in the substance of the ether which tend to restore the spatial relations of its parts to the mathematical relations of Euclidean geometry.

We have to admit, however, that we fail to follow the author's argument. It appears, however, to lead to Newtonian dynamics and gravitation law, though this cannot be interpreted as evidence for the theory, but rather that the argument has been adjusted to the result which it is required to establish. As with other theories claiming to account for gravitation, it is not amenable to testing by observation and is not likely, therefore, to command acceptance.

H. S. J.

ASTRONOMY

Problems of Cosmogony and Stellar Dynamics; Being an Essay to which the Adams Prize of the University of Cambridge for the year 1917 was adjudged. By J. H. Jeans, M.A., F.R.S. [Pp. viii + 293, with 5 plates and 45 figures.] (Cambridge University Press, 1919. Price 21*s*. net.)

The Adams Prize of the University of Cambridge, which is awarded biennially for an essay on some subject in mathematical physics, announced beforehand by the adjudicators, provides a valuable incentive to investigators to collect together their own researches on that subject, to extend them, and to weld them on to earlier work in the same direction, so as to make a homogeneous whole. Attached to the award is the valuable stipulation that the essay must be published. In this way, mathematical physics has been enriched with a number of volumes of high intrinsic merit, several of which have become recognised classics: among such, for instance, may be mentioned Sir Joseph Larmor's "Æther and Matter."

The subject announced for the 1917 Essay was "The cause of evolution of the configurations possible for a rotating and gravitating fluid mass, including the discussion of the stabilities of the various forms." This problem had already been attacked in important papers by Darwin, Poincaré, Liapounoff and others, but divergent conclusions had been arrived at by Darwin and Liapounoff regarding the stability of the rotating pear-shaped figure; moreover, their researches had been concerned only with the two extreme cases of an incompressible or an infinitely compressible fluid. There thus remained a wide field for investigation. At that time Mr. Jeans had for some years been engaged in an attack on the problem, and the opportune choice by the adjudicators of the problem as the subject for the 1917 essay was probably coupled with the hope that Mr. Jeans would be able to find time to pursue further his investigations. If so, they have been well rewarded. The volume under review contains the prize essay enlarged by the addition of some further results which had been obtained in the interval before publication; it will demand the serious study of all who are investigating the general problem of cosmic evolution.

The volume can be divided almost naturally into two parts. The first part, extending to the end of Chapter VIII, contains the mathematical framework on which, in the second part, from Chapter IX to the end of the book, the author has attempted to fit the facts of observational astronomy. The mathematician will probably be interested mainly in the first portion, whereas the practical astronomer will be inclined to take for granted the results derived therein and will concern himself with their application in the second part, which is tolerably free from mathematics: this portion is, naturally, of a somewhat speculative nature.

The introductory chapter contains a general survey of the problem, commencing with a description of the solar system and of the phenomena of binary stars, spiral and other nebulæ, and of star-clusters. Any complete theory of cosmogony must account for these five different types of structure by discovering their origins and proving that they lead to the observed structures. A brief critical survey of various earlier theories of cosmogony then follows, none of these being found free from objections.

The following six chapters contain the theoretical investigation into the configurations of equilibrium of rotating masses, together with a discussion of their stability. The case of incompressible masses is first treated, followed by that of compressible and non-homogeneous bodies. The dynamical principles upon which the investigation is based, amongst which Poincaré's conception of series of continuous configurations of equilibrium plays an important rôle, are lucidly explained in the second chapter. The mathematical analysis involved in this investigation is of great complexity, and, though elegant in principle, very cumbrous expressions have to be dealt with. Each step in the argument is given, though for some of the more lengthy reductions the reader is referred to the original papers. Included in these chapters is a discussion of the stability of the famous pear-shaped figures, and it is shown that, although Darwin and Liapounoff obtained contradictory results, yet both were in error, in so far as they proceeded only to the second order of small quantities, whilst Jeans shows that to this order the stability is indeterminate, so that it becomes necessary to consider third order terms. The complexity of the discussion is thereby very considerably increased, but Jeans has carried it through successfully and has shown that the figures are unstable.

The labour of computation involved in tracing out the sequence of configurations of the pear-shaped series after instability has set in is so heavy as to be prohibitive. Jeans has, however, discussed the analogous problem for the two-dimensional case, and the similarity between the two problems is so close before instability ensues that it is very probable that it persists afterwards. For the two-dimensional case, the pear-shaped figure is found to break up into two separate masses in mutual rotation.

In addition to the rotational problem, Jeans has also discussed what he calls the tidal problem, *i.e.* the motion of a primary mass as tides are raised in it by the continued approach and ultimate recession of a secondary mass, and also the double-star problem, *i.e.* the motion of two stars revolving round one another, a secular change being supposed to occur in their distance apart. In all three problems, the only figures of stable equilibrium are ellipsoids and spheroids. The dynamical motion in the two latter problems is also shown, with some degree of certainty, to lead to fission into detached masses; in the tidal problem any finite number of detached masses may result; in the double-star problem, a large number of small masses are formed.

The discussion is then extended to the much more complex case of compressible and non-homogeneous masses; it is shown that in each of the three different problems there are these two possible methods of break-up, one by fission, as in the case of incompressible mass, and the other, in which

a lens-shaped mass is first formed, matter then being thrown off from the equator. The limit separating the two modes is found with some degree of approximation for each problem.

Chapter VIII is devoted to the consideration of the evolution of gaseous masses, which are continually emitting radiation, rotation now being assumed. The results obtained give a very plausible explanation of Russell's theory of the order of stellar evolution, and this theory is adopted in the remainder of the book. It is generally accepted as correct by astronomers to-day.

The theoretical discussion suggests almost naturally the probable mode of formation of spiral nebulæ, which is considered in Chapter IX. A slight external gravitational field is sufficient to account for the ejection of matter in two opposite filaments, instead of being thrown off in the form of a ring; it is further shown that, with very probable assumptions, the mass of the separate nuclei will be comparable with the mass of our sun.

The second type of structure discussed is the star-cluster. This practically forms a separate section by itself, not being dependent on the previous chapters. The discussion is, in fact, based upon the methods of general stellar dynamics. It is shown that the phenomena in our stellar universe indicate that it is not in a statistically steady state, but that there is a tolerable approximation to such a state. This being so, certain conclusions as to stellar motions in the system can be deduced which are in reasonably good accordance with observation.

The evolution of binary and multiple stars is next considered. This subject bristles with difficulties, but some important results of wide generality are obtained. It is shown that, if a binary star is formed by fission, although the spectral type and eccentricity will vary as the evolution progresses, yet in general the period remains of the same order. It had previously been thought that period and eccentricity increased together, since observation appeared to indicate this. The effect of close encounters with other stars is also discussed, and it is found that, in the present state of the universe, these can have only an infinitesimal effect upon the orbits of binary stars; but that, if the stars were at some previous time much more closely packed than they are at present, the effect would have been to increase not only the eccentricity and linear dimensions, but also the period. The shorter periods of the B-type binary stars are therefore probably to be accounted for by supposing them to have been formed comparatively recently, so that the effect of encounters has not materially increased their periods.

The most difficult of all the structures found in the heavens to account for is our own solar system. Jeans discusses the possible causes at some length. It is concluded that it is very improbable that the rotational theory can account for its formation, but it is shown that the tidal theory offers a not impossible explanation provided it be granted that the outer planets were partially fluid at, or shortly after, their birth. Jeans concludes that "the theory is beset with difficulties and in some respects appears to be definitely unsatisfactory; . . . it appears more acceptable than the rotational theory, or any other theory so far offered of the genesis of the solar system."

We have endeavoured to give the reader a general idea of the contents of the book; space does not permit a critical discussion of the conclusions arrived at. In some measure, however, the author disarms criticism by the frank admission that some of the conclusions are not free from objection; yet alternative conclusions seem to be open to even more serious objections. The volume contains at once the most comprehensive and also the most thorough discussion yet made of the method of evolution of the various types of structure known to us in the heavens. A very large

part of the material of this discussion has been provided by the author's own important researches.

We offer our congratulations to Mr. Jeans and couple with them the hope that he will be able to find time to pursue some of the problems further and succeed in elucidating some of the many difficulties with which the subject bristles.

H. S. J.

Practical Exercises on the Weather and Climate of the British Isles and North-West Europe. By W. F. STACEY, F.R.G.S., F.R.Met.Soc., University College, Reading. [Pp. 64.] (Cambridge: at the University Press, 1919. Price 2*s*. 6*d*. net.)

THE aim of this book is to teach the student something about the weather of the British Isles, by making him construct for himself a series of maps similar to those appearing in the Daily Weather Report of the Meteorological Office, from which publications the data have in every case been taken. The examples have been chosen with a view to familiarising him with the chief types of pressure distribution and the weather that accompanies them. Thus we have illustrations of the Cyclone and Anticyclone in summer and winter, the formation of a secondary depression, the Col and V-shaped depressions. The method of drawing isobars, and their use for estimating the direction and strength of the wind, are explained at an early stage of the book, together with the Beaufort Weather Notation which is in general use all over the world. The concluding exercises deal with the most important types of weather, classified according to the general drift of the wind across the British Isles, namely, the E., S.W., N.W., and W. types.

Teachers of geography should find this carefully thought-out work of great assistance.

E. V. NEWNHAM.

The Study of the Weather. By E. H. CHAPMAN, M.A., B.Sc. [Pp. xii + 131.] (Cambridge: at the University Press. Price 3*s*. 6*d*. net.)

THIS book is intended primarily for use in schools, but the admirable way in which the elementary stages of a difficult subject are treated gives it a wider scope. From exercises in weather study of the simplest kind the student is led gradually to the consideration of more difficult problems. He should finally be in a position to appreciate fully the information conveyed by modern synoptic weather-charts, and will experience no difficulty in passing on to the more advanced textbooks on Meteorology. From the earliest chapters, which deal with the observation and recording of wind and weather, the exercises are cleverly chosen to stimulate thought. Clouds, and their classification, are introduced at an early stage. The remarks on this subject are illustrated by a number of photographs of great beauty, of which two were taken from an aeroplane and show the magnificent cloud panorama that is so often seen when an aeronaut passes through the level of the lower clouds. Weather which can be recorded without instruments, such as fog, mist, dew, hoar-frost, etc., is considered next, and then the commoner instruments, such as the rain-gauge and the thermometer. A whole chapter is devoted to the more difficult problem of measuring the pressure of the air, and a full description of the Kew-pattern barometer, graduated in millibars, is given. Passing from the weather at the observer's own locality, an explanation is given of the way in which simultaneous observations from stations in different parts of the British Isles, and from abroad, are dealt with by the Meteorological Office in London, and the

method of displaying all the information upon a single chart of Europe. The terms "isobar" and "isotherm" are explained, and the method by which these lines are drawn. Buys Ballot's Law, and the estimation of wind-force from the closeness of the isobars, receive due attention, examples being taken from the Daily Weather Report of the Meteorological Office. The concluding chapters deal with Cyclones and Anti-cyclones, and modern methods of anticipating weather.

E. V. NEWNHAM.

CHEMISTRY

Stereochemistry. By ALFRED W. STEWART, D.Sc., Professor of Chemistry in the Queen's University of Belfast. (Textbooks of Physical Chemistry.) Second Edition. [Pp. xvi + 277, with 58 illustrations.] (London: Longmans, Green & Co., 1919. 12*s*. 6*d*. net.)

PROFESSOR STEWART's work on the important topic of stereochemistry is so well known to students that there is no need to reintroduce it, in its second edition, to English chemists.

As the author himself notes, in the first edition certain predictions were made as to the probable lines of development of the subject, and since that date (1907) some of these prophecies have already been fulfilled, namely, the production of optically active compounds containing no carbon atoms, the activity of aniline oxides and corresponding phosphorus compounds, and the preparation of allylene analogues in active forms, so that these matters are now dealt with in the present edition.

Further, a chapter is devoted to a review of the problem of the so-called "Walden Inversion."

The new edition has been considerably altered from its original form and is now rather more of a readable textbook than a work of reference, about one half of the volume consisting of new or rewritten matter; for, although only four new chapters have been added, the old material has been altered or replaced to a great extent; the portion on steric hindrance, in particular, having been cut down considerably, though the numerous references to this subject are included as an appendix.

Prof. Stewart has had the courage of his opinions in the drastic revision to which he has submitted his book and has brought it thoroughly up to date and into line with the trend of modern research, so that it will no doubt, in addition to retaining the old readers, attract many new ones.

F. A. M.

Chemistry from the Industrial Standpoint. By P. C. L. THORNE, B.A., with an Introduction by H. V. A. Briscoe, D.Sc., A.R.C.S., D.I.C. (The New Teaching Series of Practical Textbooks.) [Pp. xvi + 244, with numerous diagrams.] (London: Hodder & Stoughton, 1919. Price 4*s*. 6*d*. net.)

THE volume under review is one of the New Series issued by Messrs. Hodder & Stoughton which have been written with the view of providing teachers with suitable text-books for the use of middle forms of secondary schools, and for continuation classes in particular.

Whatever view may be taken as to the need or desirability of such a series there can be little doubt that Mr. Thorne has achieved a considerable measure of success in the task which he has set himself.

The book is, of course, written essentially from the practical standpoint,

but there is doubtless room for just such a work for those to whom the practical application of the science is of greater importance than its theory.

As Dr. Briscoe writes in his Introduction, "A praiseworthy prejudice against purely utilitarian education causes many teachers to handle their subjects in an atmosphere of entire detachment from their practical application. Chemistry in particular has suffered in this way, and the book Mr. Thorne has now written is the first of its kind. It is a departure from standard practice which is, I believe, of the first importance."

Accepting the book as being intended for those who have to do with the hard facts of chemistry rather than with the more philosophical aspects of the subject, it will be readily admitted that the author has managed to compress into the 244 pages quite a good summary of the existing state of development of applied inorganic chemistry.

There are five sections: (A) Solids, liquids, and gases, describing shortly the general physical methods used for separating substances. (B) Chemical change; giving a little theory and details of a number of important manufacturing processes such as the Leblanc process, metal smelting, etc. (C) Elements and compounds. (D) The rate of chemical change. (E) The use of electricity in chemical changes.

It is doubtful whether so eminently practical a book will commend itself to public school science teachers or to those who have to teach first-year university students, but for technical schools and colleges and for those who desire to obtain a practical knowledge of the world of applied chemistry, no better introduction could be desired.

F. A. M.

The Manufacture of Chemicals by Electrolysis. By ARTHUR J. HALE, B.Sc., F.I.C. [Pp.xii + 80, with diagrams.] (London: Constable & Co., 1919. Price 6*s*. net.)

IT is a little difficult to place Mr. Hale's short monograph, as eighty pages on the whole subject can hardly be regarded as a generous allowance for a practical handbook, whilst, regarded as a student's text-book, it is to be feared that there are too many facts and too little theory to recommend it to teachers.

The book may serve as a useful short outline for those who do not wish to dip deeply into the question, but, clearly, would have been all the better for more extended and more detailed treatment of the processes dealt with.

F. A. M.

Catalysis in Theory and Practice. By ERIC K. RIDEAL, M.B.E., M.A., Ph.D., and HUGH S. TAYLOR, D.Sc. [Pp. xvi + 496, with illustrations and diagrams.] (London: Macmillan & Co., 1919. Price 17*s*. net.)

IN considering the history of chemistry during the past century one is constantly reminded of the way in which all chemical processes are becoming more and more dependent upon the action of catalysts: for instance, the transition from the old chamber process to the modern contact process for the manufacture of sulphuric acid, the gradual replacement of the older sodium nitrate method for preparing nitric acid by the catalytic oxidation of ammonia, and, most recent of all, the production of acetaldehyde from acetylene by the agency of a mercury catalyst, leading thus to the commercial synthesis of alcohol, acetic acid, acetone, synthetic rubber, and a hundred other products.

In the early days there can be no doubt that chemists were largely dependent upon what we may call *forced* chemical reactions, that is to say,

forcing chemical reactions to take place by such energetic—not to say violent—means as high temperatures, the action of strong acids and alkalies, and the like.

It is only recently that we have come to realise that many, if not all, reactions can be made to take place far more smoothly and readily under the influence of suitable catalysts, than by having recourse to chemical "frightfulness," so that we are constantly approaching nearer and nearer to the methods of nature in vital processes, which are indeed catalysed reactions *par excellence*. In fact one feels that the chief problems of to-day are to find the right catalysts.

To attempt to review fully the contents of the book one would need to enter into a discussion on most of the problems of pure and applied chemistry, both organic and inorganic, for there are few branches of the science into which catalysis does not enter, but at all events, whatever errors of omission or commission may have been made by the authors of the present volume, they have undoubtedly succeeded in producing an extremely interesting and eminently readable account of the whole subject.

As they themselves point out in the preface, there have been hitherto no books on the subject of catalysis, other than various short monographs such as those of Sabatier, Woker, Orloff, Vittora, and Jobling, not owing to any lack of interest in, or knowledge of, the subject, but, more probably, because of the very great amount of labour required to summarise the existing information and to present it in a suitable form. For this reason chemists should be duly grateful to Dr. Rideal and Prof. Taylor for their courage in tackling so large and diffuse a subject as catalysis, both in its theoretical and practical aspects.

Possibly the authors might have devoted a little more space to one or two matters such as the great activity of copper as a catalyst in various directions, particularly in connection with the chemistry of the anthraquinone series, or the influence of solvents on reactions, including the very important work of Dimroth on the subject, which finds, strangely enough, no mention in the section dealing with the action of solvents; and the very interesting case of the use of mercury compounds as catalysts for the conversion of acetylene into acetaldehyde, where definite intermediate compounds have been isolated, surely calls for somewhat fuller treatment than it receives.

Bearing in mind the axiom on p. 14 that "the chemical composition of the catalytic agents is unchanged on completion of the reaction process," we may venture to inquire whether the authors are justified in taking up some half-dozen pages with a description of the well-known Grignard reaction, for neither the magnesium nor the organo-magnesium compounds remain unchanged on the completion of the process.

No doubt in later editions of the book it will be found possible to amplify various portions as experience dictates, but meanwhile it remains a very excellent general treatise on the subject of catalysis which will repay careful study.

F. A. M.

The Profession of Chemistry. By RICHARD B. PILCHER. [Pp. xiv + 199.] (London: Constable & Co., 1919. Price 6*s*. 6*d*. net.)

MR. PILCHER, who is Registrar and Secretary of the Institute of Chemistry of Great Britain and Ireland, has had a wonderful opportunity of seeing the development of Chemistry which has taken place in recent years. In this book he reviews all the branches of the profession, from the point of view of the recognised professional body. The book is a mine of information on the subject for parents, for the general reader, for the commercial and industrial man, for the educationalist, and not least for the politician. The first chapter, entitled "Preliminary Education," is written for the consideration of parents

whose boys are still at school; the second chapter, entitled "Pharmacists and Chemists," is a delightful essay on the meaning of these two words. The remaining chapters deal with "Professional Training," "Professional Organisation," "Professional Procedure," and the various branches of the profession of chemistry, including a chapter on "Chemists in War."

Naturally the author's point of view is that of the Institute, which is now the recognised professional body, and to which the vast majority of chemists belong. But we should like to mention that the Institute has gained its position by widening its basis, so as to admit the majority of chemists in this country. Until a few years ago it did not do this, it merely represented the interests of analytical and consulting chemists who have for many years been a minority in the profession. It had little influence until recently outside London, except as a qualifying body for analysts under the Food and Drugs Act. Its regulations were then so drafted that a University Graduate, to join the body, was obliged to forego postgraduate research work in order to obtain manipulative skill to pass the examination of the Institute. We are glad to say that this has all been changed, but we consider it amusingly inaccurate of the author to say, "In recent years, however, the character and the training and examination of the Universities has been modified to render their honours graduates in chemistry better prepared than formerly for the more practical branches of the profession, and *this has been recognised by the new Regulations of the Institute!*" (The italics are ours.)

The author knows well the present economic position of chemists, and reviews their position and prospects with authority. In this connection it is interesting to quote the following from the current number of the *Proceedings of the Institute of Chemistry*: "The Committee (of Scientific and Industrial Research) appears to be apprehensive of a shortage of competent scientific workers, but, so far as chemists are concerned, the demand at present is not sufficient to absorb the available talent released from the services and from war work."

The book is well written, and it appears at an opportune moment, for during the war the profession of chemistry has become recognised as it never was before.

R. E. SLADE.

The Chemistry of Colloids. By RICHARD ZSIGMONDY, translated by ELLWOOD B. SPEAR. [Pp. x + 288, with illustrations.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1917. Price 13*s*. 6*d*. net.)

ONLY a comparatively few years ago it was difficult to find a satisfactory book on Colloids; now, however, there is a considerable number to choose from, and for this reason one cannot help wondering whether this translation was called for, since the book does not present any striking novelty of treatment of the subject. The work of Professor Zsigmondy in the realm of colloids is of course too well known to require any comment, but frankly we find his book disappointing. The volume under review is divided into two parts, the first of which is a translation from the German of Zsigmondy's *Kolloidchemie*, while the second, which is devoted to Industrial Colloid Chemistry, is from the pen of the translator. The table of contents of Part I promises well, but on looking up the subjects in the text one rarely finds a lucid or satisfactory account of the subject. What, for example, is to be made of the following information concerning barium sulphate? "... The solubility of barium sulphate is too great to allow the preparation of the hydrosol without the presence of a protective colloid. Neuburg prepared a hydrosol of barium carbonate by conducting a current of CO_2, into

a solution of barium oxide in methyl alcohol. The gel at first formed dissolved spontaneously on standing in methyl alcohol. In a similar manner the gels of barium sulphate, calcium, and magnesium carbonate have been made." The very important subject of the swelling or imbibition of colloids receives only scant reference in four different places in the book, and we look in vain for an intelligible account of the various hypotheses concerning the structure of gels. Part II, though occupying only some thirty pages, is, in our opinion, much more satisfactory, since it does at least give some definite information on the subjects it deals with; it contains chapters on Smoke Prevention, Rubber, Tanning, Milk, Colloidal Graphite, Clay, and Colloids in Sanitation, the latter contributed by J. F. Norton.

P. H.

The Simple Carbohydrates and Glucosides. By E. FRANKLAND ARMSTRONG, D.Sc., Ph.D. [Pp. x + 239.] Third Edition. (London: Longmans, Green & Co., 1919. Price 12*s.* net.)

THE wisdom of the policy of publishing in the form of monographs is amply illustrated by the frequent reissue of the volumes of the series of the Monographs on Biochemistry to which this volume belongs. In the present edition the material of the last one has undergone a slight re-arrangement in the matter of subdivision into chapters. Amongst the more important additions we note the following. In Chapter I mention is now made of the existence of a third isomeric methyl glucoside, having an α-ethylene oxide instead of the γ-butylene oxide structure possessed by the two well-known α and β modifications first described by Fischer; this author had long ago shown that in the preparation of the two crystalline methyl glucosides by the action of hydrogen chloride on a mixture of glucose and methyl alcohol there resulted also a considerable amount of a syrup which at that time he did not purify. In 1914, however, he subjected this syrup to distillation in a high vacuum and found it to be a compound having the composition $C_7H_{14}O_6$, which is that of a methyl glucoside. Since it was stable to alkalis and Fehling solution, and could be hydrolysed by acids but was hardly attacked by emulsin or maltase, he concluded that it must be a third isomeric methyl glucoside. At a later date Irvine, Fyfe and Hogg showed that this compound was a mixture of isomerides derived from a new variety of glucose; this new glucose has not hitherto been isolated, but, judging by the behaviour of its compounds, its reactivity would appear to exceed that of either α or β glucose. In Chapter I also the section dealing with the more important derivatives of glucose have been rearranged somewhat, and spaced out so as to be more clearly presented, and the chapter is brought to a close with a somewhat fuller account of Hudson's papers on the numerical relations amongst the Rotatory Powers of the Sugars. Chapter III contains a new section on the carbohydrate alcohols, while the subject matter included in Chapter VII under the heading of "The Natural Glucosides," has been considerably extended so as to include an account of the Anthoxanthin and Anthocyan Glucosides, and also a fuller description of the Digitalis Glucosides and the Saponins. The Synthetic Glucosides in Chapter VIII also receive fuller treatment than in the previous edition. Where there is so much that is good it may seem ungrateful to criticise, but the arrangement of the bibliography leaves very much to be desired. The references are all arranged at the end of the book under the headings of the chapters to which they are supposed to apply; but, by this method, endless time is wasted when, on reading the name of some author in the text, an attempt is made to find the relevant paper. Even supposing the reader has found out which particular portion of a given chapter he is reading, and has successfully tracked the cor-

responding section at the end of the book, several papers may stand to the credit of the author in question. Much annoyance and time would have been saved by numbering the references, and inserting such numbers in the text after the name of the author.

P. H.

The Preparation of Organic Compounds. By E. De Barry Barnett, B.Sc., A.I.C. [Pp. xvi + 273, with 54 illustrations.] Second Edition. (London: J. and A. Churchill, 1920. Price 10*s*. 6*d*. net.)

This book, which is now in its second edition, aims at "giving a general outline of the methods actually employed in preparing organic compounds, and thus providing not only a laboratory manual, but also a book which may be used as a companion volume to the usual theoretical text-books." The first chapter is devoted to a description of apparatus and methods of manipulation, and contains some quite useful hints on the adaptation of ordinary culinary vessels to fairly large-scale chemical operations; it also gives an account of the ordinary equipment of an organic chemical laboratory, accompanied, however, by some positively grotesque illustrations in which no attention whatever is paid to proportions or details; the Soxhlet apparatus on page 20 in particular being quite incapable of acting as it stands. The remaining twelve chapters each deal with the methods of preparation of a special group of organic compounds such as hydrocarbons, halogen compounds, alcohols, phenols and mercaptans, aldehydes, ketones and quinones, carboxylic acids, esters, nitriles, nitroso and nitro compounds, etc., etc. As stated in the Preface, the description of the processes given is less full than in most books on organic preparations, but the details given are sufficient to enable the average student to carry out the preparations successfully. This may be true in many cases, but very frequently the preparations start from a compound which is not an ordinary laboratory chemical, while no method of preparing this compound is given elsewhere in the book. This method of writing may be excusable in a large book of reference such as Weyl's *Methoden der Organischen Chemie*, to which the author expresses his indebtedness, but is hardly suitable for a laboratory manual such as the present volume.

Again, it is a little difficult to understand why the old-fashioned method of preparing formic acid from glycerol is quoted as an example of the preparation of a carboxylic acid by oxidation of an alcohol, and also why the modern method of preparing oxalic acid from formic acid and the catalytic preparation of acetone from acetic acid receive no mention.

The book is fairly well supplied with references, but unfortunately no dates are given, which fact of course very considerably detracts from their value.

When allowance has been made for these minor defects, however, it must be acknowledged that the book contains quite a lot of useful information.

P. H.

BOTANY AND AGRICULTURE

The Hardwoods of Australia and their Economics. By Richard T. Baker, Lecturer on Forestry, Sydney University. [Pp. xvi + 522, with 134 coloured plates and 192 figures in black and white.] (Department of Education, Sydney. Price £1 5*s*.)

The Australian continent is one of the greatest hardwood-producing areas in the world. It possesses probably somewhere about 500 species of dicotyledonous timber-trees which, though a low number considering the area

of over three million square miles involved, comprises some that are extraordinarily abundant. In this handsome quarto volume the wealth of Australia in this respect is put before us, and illustrated with a profusion and excellence that is beyond praise. The colour reproductions of freshly planed surfaces of the different woods are particularly beautiful, and bring home in the only possible way the variety of tone and texture on which largely depend their commercial possibilities. Photomicrographs of the woods of a large number of species are reproduced showing their histological characteristics as presented in transverse, and both radial and tangential longitudinal sections. The accompanying text furnishes taxonomic details and information respecting the geographical ranges of the species, descriptions of their timbers, and technological details such as uses, relative hardness, weight, and, not infrequently, data regarding breaking loads, modulus of rupture, etc.

In addition to the specific descriptions a number of chapters are devoted to the various physical properties and the economic applications to which the timbers of Australia lend themselves.

The fact that most impresses one on reading these pages is the enormous importance of the genus *Eucalyptus*. About 180, or nearly two-fifths, of all the different kinds of timber that Australia furnishes are the product of species of this group. To many the name *Eucalyptus* is only associated with the ethereal oil employed as a specific for "colds," and indeed the diverse members of the genus supply no inconsiderable part of the raw material for the perfumery trade. But, if less familiar, the importance of the Eucalypts as timber-trees can scarcely be over-estimated. The extraordinary variety and high commercial value of their wood cannot be equalled by any other group of trees, and the range of hardness, appearance, and technical qualities which they present are such as to furnish excellent substitutes for many timbers of commerce derived from a large range of genera and families.

The colour ranges from the pale, almost white, wood of *E. fraxinoides*, similar in character to European ash, through the deep yellow box-like wood of *E. melliodora*, to the mahogany-like *E. rostrata* or the dark red-brown of *E. reduncа*. The fire-resisting properties of Eucalpyts are well known, and in the case of *E. Fletcheri* it is about seven times that of the English oak. Their durability is correspondingly high, and the author cites the case of a slab of *E. paniculata*, used in the construction of a stable, which was in an excellent state of preservation after an exposure to the elements of over one hundred years!

To those who consider the increasing demand for timber, and the condition of the world's supplies, it is clear that the continued exploitation of virgin forests cannot be long maintained at its present rate. With the necessity for replanting, or natural regeneration, the length of time that must elapse before the timber matures is an important factor; for even when the amount of the original outlay is small the accumulated debt is often the determining influence for success or failure.

It is the phenomenal rapidity of growth of many species of *Eucalyptus* which at once marks them out as one of the great sources of timber supply for the future. It is perhaps scarcely too much to say that, properly exploited, the timbers of these trees may become, for the hardwood industry, what the coniferous timbers are amongst soft-woods.

Mr. Baker insists strongly on the necessity for reform in timber nomenclature, which, he says, "must be faced sooner or later by foresters, timber-merchants, and all connected with the trade, in order to circumvent the confusion that exists in so many instances in connection with the same species having a different common name in its various localities." Moreover, to add to the confusion, one name often does duty for several species, as, for example, "Blue Gum," applied equally to the heavy pale timber of *E. globulus*

or the much lighter, red wood of *E. saligna*. The only remedy is the adoption of scientific names, which, as the author pertinently remarks, has actually been accomplished in the *Eucalyptus* oil industry.

Limitations of space prevent our referring to the many other noteworthy timbers dealt with, but mention must be made of the *Acacias* and *Casuarinas* (she-oaks) and the beautiful woods of *Telopea oreades* and *Tarrietia argyrodendron* (crow's-foot elm).

From what has been said it will be realised that there is pabulum here for the timber-merchant, the forester, the technologist, and the botanist, and these pages should do much towards the promulgation of a more widespread acquaintance with the extensive resources of the Commonwealth.

E. J. Salisbury.

Graines et Plantules des Arbres et Arbustes indigènes et communément cultivés en France. By R Hickel. Part I, Conifères (pp. 182, with 93 figures); Part II, Angiospermes (pp. 349, with 85 figures and 2 plates). (Published by the Author, 11 bis, Rue Champ-la-garde, Versailles, 1911 and 1914; received 1919)

These two volumes together furnish a mine of information with reference to the seeds and seedlings of a large number of the native and cultivated European trees and shrubs. In all cases the descriptions have been based on the author's own observations from authentic specimens, and nearly a thousand species of seeds or seedlings are figured in original drawings that frequently illustrate the range of size and form.

Both in the group of the Conifers and in the Angiosperms the subject-matter is divided into two parts, one devoted to descriptions of the seeds, and the other to that of the seedlings; an arrangement which, though open to criticism, facilitates the employment of these pages for purposes of diagnosis. Some idea of the extent of the field covered may be gauged by the fact that the seeds of 147 species of Coniferæ and 515 Angiosperms are dealt with, and of seedlings 112 and 314 belonging to these respective groups.

It is a natural corollary of the principle of recapitulation that the juvenile stages of plants belonging to any one group tend to resemble one another very closely, even though the adult phases may be quite readily distinguished. For this reason seedlings are often difficult to identify, and not the least valuable part of the work before us are the numerous keys for the identification of the genera or species of the various seeds and seedlings described.

The author has mainly restricted himself to morphological characters, of which copious details are furnished in the separate descriptions devoted to each species. But though chiefly concerned with morphology, a number of biological facts are mentioned, especially in relation to such features as the period of seed-shedding, the retention of germinative capacity, and the season of emergence of the plumule.

For example, it appears that, contrary to many current statements, the seeds of Poplars germinate readily, whilst the germinative capacity of many trees diminishes rapidly unless the seeds are sown soon after their maturation. The deleterious effects of desiccation appear in some seeds, as for instance those of the Oak, to be very marked, whilst in others, such as those of the Leguminosæ, vitality may be retained for a period of years.

An examination of the tabulations of seed and seedling characters in the genus *Pinus* reveals the rather interesting fact that there is a rough correlation between seed size, the number of cotyledons, and the length of these latter. Thus of the 30 odd species examined, *Pinus contorta* had the smallest seed (length $2\frac{1}{2}$–4 mm.), and its seedling exhibited cotyledons from 15 to 18 mm. in length, and numbering usually from 3–4, but occasionally only 2. By

contrast the seed of *P. Sabiniana* is from 18–22 mm. in length, and the seedling possesses from 12–17 cotyledons, which attain a length of from 50–55 mm.

The second part concludes with a clavis to the genera of epigeal Dicotyledonous seedlings, which, it may be noted, outnumber the hypogeal by about 4 to 1.

To the botanist and to the professional forester alike, these volumes should prove of considerable value, both from their practical utility and their theoretical importance.

E. J. SALISBURY.

Joseph Dalton Hooker. By PROF. F. O. BOWER, Sc.D., F.R.S. [Pp. 62, with frontispiece.] (London: Society for Promoting Christian Knowledge, 1919. Price 2*s*. net.)

IT is but fitting that a life of Sir J. D. Hooker should find its place in the series of volumes dealing with pioneers of progress. But to do justice to his many activities during so long a period of years is no light task.

The author has, however, in these eight chapters, by devoting each to a different view-point, attained a considerable measure of success.

The chapters entitled "Kew" and "Authorship" serve to present Sir Joseph's administrative achievements and his chief contributions to science But in reference to the former, one regrets that greater stress is not laid on his work for economic botany, the more, that Hooker himself rightly attached the greatest importance to it. It is to his endeavours we owe the inception of the Plantation-Rubber industry of to-day, and it was under his direction that cinchona was introduced into India, so that quinine became available to the poorest native.

Four of his chief works, of which a brief account is given, indicate the scope and character of Hooker's literary contributions. These are the Flora Antarctica, the Flora of British India, the Genera Plantarum, and the Index Kewensis. Other chapters treat of his personal characteristics, his position as a man of science, and his connection with Darwin.

Short biographies of this type are the more welcome as appealing to a wider public than the more pretentious volumes dealing with the same subjects, and are moreover within the purse of all.

E. J. SALISBURY.

Applied Botany. By G. S. M. ELLIS, B.A. [Pp. viii + 248, with 67 illustrations and 2 maps.] (London: Hodder & Stoughton, 1919. Price 4*s*. 6*d*. net.)

ONE great essential in a book written with a view to applying scientific knowledge to practical life, is that it should combine simplicity of style with scientific accuracy. In the book under review the first condition is fulfilled, for the writer has succeeded in presenting his subject in a readable and attractive form which will appeal to the general reader and elementary student. Unfortunately the second condition is not carried out, for the book is full of inaccuracies and of half-truths that are more misleading than actual errors. The statement that "the seven elements . . . are essential to the growth of a green plant" (p.11) implies that other elements can be done without, but the indispensable carbon, hydrogen, and oxygen are not included in the seven enumerated. Again, nitrogen is *not* one of the elements found in the ash when the plant-body is burned (p. 58), for the act of burning drives off the nitrogen in combination, leaving only mineral substances behind. With regard to water-culture solutions (p. 59), it is impossible to dogmatise as to the changes

that occur in the food salts, as the reactions are complex and little is known about them. In spite of much research the true cause of clover sickness is still uncertain, and it is impossible to say definitely that it is caused by bacteria (p. 84), while finger-and-toe disease in turnips is the result of the attacks of protozoa. Similar inaccuracies prevail in the morphological chapters, as where the hairs of the cotton-grass are said to be the *perianth* (p. 199), and honesty and spurrey are included among winged *fruits* (p. 200). The list of criticisms might be extended indefinitely, but it is impossible to do more than select a few instances that indicate the slipshod writing of the whole book.

Illustrations are numerous, but the absence of index lines often leaves the point of the sketch in doubt (*e.g.* fig. 23), and in most cases a reproduction of a classical drawing would be more effective than the feeble original ones presented. Some figures fail in their object entirely, as fig. 3, which purports to show the difference between the exogenous origin of branches and the endogenous origin of roots, but in fact does not do so, while in fig. 6 careless revision of proofs has passed an indicated reversal of the true positions of xylem and phloem.

The scheme of the book is sound enough, but the presentation of the subject-matter is so faulty that it is impossible to recommend it for serious perusal.

W. E. B.

ZOOLOGY

An Introduction to General Physiology, with Practical Exercises. By W. M. Bayliss, M.A., D.Sc., F.R.S. [Pp. xiv + 238.] (London: Longmans, Green & Co., 1919. Price 7*s.* 6*d.* net.)

Professor Bayliss has given us, in his *Introduction to General Physiology,* a thoroughly delightful and valuable work.

It comprises the best portions of his well-known *Principles of General Physiology,* rewritten for the use of beginners in biology, and presents the physical and chemical properties of protoplasm, and of its products, viewed from the standpoint of Energetics.

One would like more about protoplasm itself, particularly in diagram. A visual image of its substance, though necessarily incomplete and inaccurate, would be helpful as well as encouraging to the beginner. More readily intelligible might be the terms "available" and "not available" in place of "free" and "bound," on pp. 12 and 13. Not quite clear is the account of the anions on p. 59. Could, for example, OH′, or even HCO'_3, replace much of the Cl′ without harm? Of great interest, on p. 85, would be an account of expired air as the product of diffusion, due to differences in tension, between inspired air and the air of the pulmonary alveoli. On such points as these, perhaps, opinions may differ.

There will, however, be no difference of opinion that the book is a perfect God-send to the student of biology. It gives him, between one pair of covers, selected for him and explained in its bearings on structure and on function, an immense fund of valuable information which he had, formerly, to seek in many books, to disentangle from irrelevance to his needs, and to interpret in relation to biology as best he was able.

To the teacher, also, its value is considerable. To him—the preface, the frequent references to the author's "Principles," and the details of his class work will be of especial interest and assistance.

The difficulties in compiling a book on such novel lines are, obviously, both numerous and great. Prof. Bayliss has recognised this, and, thanks to his foresight, has, in overcoming them, achieved a really conspicuous success.

W. L. S.

The Unity of the Organism; or, The Organismal Conception of Life. By WILLIAM EMERSON RITTER, Director of the Scripps Institution for Biological Research of the University of California, La Jolla, California. 2 Vols. (Vol. I, pp. xxix + 398; Vol. II, pp. xv + 408.) (Boston: Richard G. Badger, 1919. Price $5.00 net.)

FROM the earliest times in the history of science, biologists have speculated on the relation of the organism to its parts, and among both botanists and zoologists, as well as general biologists, there have been those who have held the organism as completely explicable in terms of its component parts. In this book the author reviews the evidence in favour of and against the various theories that have been put forward involving this point of view, and more than half the book may be regarded as a presentation of the case against the various elementalist theories of the organism, from which the author vigorously dissents, and the presentation of the case for the organismal theory, which would regard the whole organism as the unit.

A review of the various theories of the plant body that have held sway during the last hundred years or more was given three years ago by Fitting, and published as a little book under the title of *Die Pflanze als lebende Organismus*. The present work has a much wider scope, for the author deals not only with the organism generally, but considers the various elementalist theories, and shows them all to be untenable. Thus the Cell Theory, the Mitochondrial Theory of Heredity, and the Chromosome Theory of Heredity, are examined and dismissed one after another.

In the second part of the book the author examines the interdependence of the various parts of the organism. From growth and internal secretions he proceeds to the nervous system and the interdependence of its various parts, and the involving with it of other organs, and finally to psychical phenomena, the work concluding with an Organismal Theory of Consciousness, to the effect that all manifestations which in the aggregate are called life, including those of a consciously psychical nature, result from chemical reaction between the organism and its environment.

To the experimentalist, and especially to the physiologist, a great deal of this book will appear as a storm in a tea-cup. Few physiologists to-day doubt the correctness of the outlook that regards the organism as a unit, though apparently, from the citations and references given in this book, this outlook is not universal. Also it must be admitted that the writing suffers sometimes from vagueness. Thus the first section of Chapter VI is given the title "What the Cell-Theory is, Viewed Historically and Substantively," but after reading the section, the chief impression one obtains is that the cell-theory is a number of different things, all more or less indefinite.

Nevertheless, in spite of this, and in spite of a certain garrulousness, which is perhaps most noteworthy in the preface, the book is well worth reading, for the writer passes in review a great mass of material, the correlation of which is in itself a great achievement.

W. S.

The Morphology and Evolutional Significance of the Pineal Body, being Part I of a Contribution to the Study of the Epiphysis Cerebri and an Interpretation of the Morphological, Physiological, and Clinical Evidence. By FREDERICK TILNEY, M.D., Ph.D., and LUTHER F. WARREN, A.B., M.D. (American Anatomical Memoirs, No. 9, Wistar Institute, 1919.)

THIS publication, the first part of an extensive memoir on one of the most interesting regions of the brain, is concerned with the historical, histological, embryological, and comparative morphological aspects of the subject clearly indicated in the title, the functional and clinical aspects being reserved for a subsequent paper. From the zoological point of view, therefore, the paper may be considered in two separate parts, of which the present is the more important.

After a brief introduction and discussion of the nomenclature, we have a general review of the literature, starting with Galen. This is followed by three long sections dealing with the comparative morphology of the pineal complex, the comparative embryology of the complex, and the comparative anatomy and histology of the same. These sections go into the most important literature in more detail, summarise the previous findings under the various species of animals, and give an account of the author's own researches. This is a valuable piece of work, for it enables one to pass under review the results of numerous authors scattered in different periodicals. Here, however, a criticism is called for. In our opinion Dendy's work on *Sphenodon* is not sufficiently emphasised, and, what is less explicable, there is no reference to this author's work on *Geotria australis*, one of the Cyclostomes. This, however, is one of the latest and best memoirs on that subject,and, concerning as it does one of the lowest groups of Vertebrates, is of importance in considering the comparative aspect of the question. These descriptive portions are illustrated by a number of figures and plates. Many of the text-figures are excellent, others err somewhat on the side of being too schematic—this, however, is not the fault of the authors, since they are reproduced from other papers. In certain cases the reproductions of photomicrographs are good, but in others they are poor. It is a pity that the more important of them, at any rate, were not reproduced by collotype or similar process upon a better grade of paper, since half-tone prints on ordinary paper, we feel sure, do not do justice to either the original photographs or the preparations.

The concluding section is in the form of a discussion which, since it is preceded by such a mass of readily available evidence, is very valuable. It is preferable, as the authors point out, to consider the relationships of the pineal area or complex as a whole, and not to deal with isolated parts, such as the pineal gland and the parietal eye. The position is maintained that this apparatus cannot be considered as a vestigial one, and two of the most interesting conclusions are: " . . . the epiphyseal complex of vertebrates possesses a pluripotentiality whose fundamental inherent tendency is in the interest of glandular differentiation, but in a few instances, as in Cyclostomes, Amphibia, and in primitive Reptiles, the pineal organ may become further differentiated in the interest of a highly specialised sensory mechanism, which has or has had visual function;" and again, " The phylogenetic significance of the parietal eye in Vertebrates as the homologue of the median eye of Invertebrates should be accepted with much reservation."

On the whole this is a thorough and valuable contribution to our knowledge of these structures in the brain and the problems they present, and although it does not claim to have answered satisfactorily all the questions it raised, has undoubtedly cleared the ground, and should serve as a useful stimulus to further research. C. H. O'D.

A Geographical Bibliography of British Ornithology, from the Earliest Times to the end of 1918. By W. H. Mullens, M.A., LL.M., F.L.S., M.B.O.U.; H. Kirke Swann, F.Z.S.; and Rev. F. C. R. Jourdain, M.A., M.B.O.U. Part I. [In six bi-monthly parts.] (London: Witherby & Co., 1919. Price per part, 6*s*. net.)

Messrs. Mullens and Swann have already contributed so much first-class and invaluable work to bibliographical literature, that the present contribution, in conjunction with the Rev. Jourdain, will be heartily welcomed by all British ornithologists.

This work forms a supplement to their recent *Bibliography of British Ornithology* (1917). This was, however, purely biographical, while the present volume is geographical, *i.e.* each section is devoted to a county. Practically all the works referred to in the first volume, under the alphabetically arranged names of authors, again appear, but this time under the name of the county

to which they have particular reference. In addition, all local records' published in the many British ornithological and natural-history journals up to 1918, are systematically given under their particular counties. It is therefore possible for an ornithologist visiting a part of England new to him to have before him a complete list of all published references to the birds of the district. The extraordinary value of this to the serious student is self-evident.

To those who do not possess the first volume, the long list of general works of reference given in Part I, now before us, will also prove useful.

The book is well produced, but lovers of their private libraries will be sorry that it is not printed on the same paper as the companion volume, though the two will match in size and approximately in thickness. There are several printers' errors, *e.g.* on p. 12 appears Atchison, G.T., while on p. 13 appears Aitchison, G.T. The first is correct.

We can find no reference under Cumberland to *The Home Life of the Terns*, by W. Bickerton, although it contains a lengthy account of the famous Ravenglass Gullery. But omissions are so few that criticism on that score is unmerited, and we can but congratulate the authors on the successful beginning of an undertaking for which all British ornithologists will be heartily grateful.

WM. ROWAN.

MISCELLANEOUS

Vital Statistics. An Introduction to the Science of Demography. By GEORGE CHANDLER WHIPPLE. [Pp. xii + 517.] (New York: John Wiley & Sons; London: Chapman & Hall, 1919. Price 18*s.* 6*d.* net.)

THIS book is intended primarily for students in Schools of Public Health, and with this end in view it deals with the collection of data, with tabulation, and with graphical representation rather than with the working out of the data when obtained. Probability and correlation are touched upon, but we are inclined to think that this part of the book will not prove so enlightening as the earlier portion. In the first chapters there is most useful information for those collecting and tabulating data, and warnings as to the importance of stating exactly what limits are used in grouping, how ages are reckoned (whether age last birthday or age to the nearest year), and as to various other matters all of vital importance if good use is to be made of the data collected.

Throughout the book there are hints as to the intrepretation of results obtained from statistics which are very valuable. We would especially recommend the chapter on the *Statistics of Particular Diseases*, as it gives many examples of the danger of drawing conclusions from mortality rates without a most careful study of those rates. For example, a much higher death-rate from tuberculosis is found in Richmond than in New York City, but when a further analysis of the rates is made Professor Whipple points out that it is due to the far larger proportion of coloured persons in Richmond than in New York, and if the population be sub-divided into white and coloured, New York is found in both cases to have a higher death-rate from tuberculosis than Richmond. In this chapter, Prof. Whipple gives a diagram showing the growth of water filtration and the decrease in typhoid fever death-rates; such a diagram proves nothing, both have gone down with time, but that does not necessarily mean that one depends on the other.

The chapter on correlation does not appear to be so adequate. It is difficult to understand what is meant by the statement that "Pearson's coefficient (of correlation) is not quite the same" as Galton's, when Galton's coefficient is given as $\frac{\Sigma xy}{n\sigma_x\sigma_y}$. This is the *product moment coefficient* used in all cases when the correlation table is quantitative both ways, and the means of the arrays lie on a straight line. It is hardly fair to call it either Galton's

or Pearson's coefficient, since it was first given by Bravais in 1846, and in 1896 was shown by Prof. Pearson to be the *best* value of the correlation coefficient. Prior to 1896, Galton used average deviations in the array from their means. This average deviation, when divided by the probable error, was termed by Prof. Weldon *Galton's function*.

It seems rather a mistake to use the symbol *r* for the probable error, when *r* at present always signifies the correlation coefficient.

ETHEL M. ELDERTON.

Textbook on Wireless Telegraphy. By RUPERT STANLEY, B.A., M.I.E.E. [Two vols.: Vol. I, pp. xi + 471, with 253 figures; Vol. II, pp. viii + 357, with 227 figures.] (London: Longmans, Green & Co., 1919. Price 15s. per volume.)

THIS greatly enlarged edition of Major Stanley's well-known book is a welcome addition to the literature of wireless telegraphy. He starts from the electron theory of matter and develops his elementary section, dealing with the flow of current, on this basis. This, in itself, is an interesting experiment, and the experiment seems justified by the lucidity of the treatise. The first volume is very similar to the original book that Major Stanley published. It has been improved and minor corrections made. The chief interest of the new edition, however, lies in the second volume.

The author was in touch with all wireless developments during the war, and for the last four years was chief instructor in wireless to the British Expeditionary Force in France. He has thus been able to keep abreast of all the most recent developments, and the description he gives of the various types of valves, and of their circuits, is of the highest value.

The second volume opens with a chapter on electrons, in which the theories of current flow and electrostatics are expressed in terms of the electron unit. The second chapter deals with the theory of the hard valve. It starts by explaining the action of the original Fleming valve and develops the theory of the action of the three-electrode valve. Characteristics are given, and particular attention is paid to the well-known French valve, which has been used so largely by all countries during the war.

In chapter iii, the subject of valve detectors and detector relays is discussed, and one is glad to note that due justice has been done to Prof. Fleming for the valuable pioneer work that he did on the valve. The author states, "Fleming was the first to use the unilateral conductivity effect of an electron discharge, from a heated filament to a plate at a higher potential than the filament, so that his patent of 1904 may be considered as the parent one of all valves." The analogy of the tilted water-vessel, which is used to explain the action of the three electrode valve, when receiving a series of wave trains, is exceedingly ingenious. The next chapter deals with the use of the three-electrode valve as an amplifier, describing not only the simple series of valves that are used as amplifiers, but the valve in which a reaction coil is employed to make the valve nearly "self-exciting."

The "heterodyne" action now used so largely in valve receivers is also explained. In chapter v, the action of the valve for generating oscillations is dealt with very fully, and the various kinds of coupling between plate and grid circuit discussed. The following chapter deals with the general arrangement of low-frequency and high-frequency amplifying apparatus; then follows a short chapter on the sensitiveness of valve reception, and the next three chapters deal with the characteristics of the French valve, and other special continuous wave types of valves designed for varying conditions. Some of the Standard C.W. transmitter sets are next described, and the following three chapters give a detailed account of the various systems that are now used in

wireless telephony, including the Poulsen arc generator, and the Alexanderson alternator for generating continuous waves. The book concludes with a short account of earth-current signalling and miscellaneous valve apparatus. It is one which should be in the hands of every wireless man who has to deal with valves (and what wireless man does not in these days ?). It is far and away the most complete account that has yet been published of the valve, and its uses in wireless telegraphy, and marks a distinct step in the amount of published information about this most vital and important instrument. It is no exaggeration to say that the valve opens up more possibilities for signalling of all kinds than any other piece of apparatus that has been brought to perfection in the last half-century, and it is for this reason that Major Stanley's book is so opportune and so valuable.

The Oliver-Sharpey Lectures on the Feeding of Nations. A Study in Applied Physiology. By Ernest H. Starling, C.M.G., M.D., F.R.C.P., F.R.S., Chairman of the Royal Society Food (War) Committee, Honorary Scientific Adviser to the Ministry of Food. [Pp. ix + 146.] (London: Longmans, Green & Co., 1919. Price 5s. net.)

In these lectures the scientific principles on which the feeding of a nation should be based are explained with great clearness, and it is shown how the application of these principles to the problem presented by the limitation of the food-supply of this country during the latter part of the war enabled the difficult situation thus presented to be successfully handled. The conditions obtaining both in this country and in Germany are described, and the reasons for the system of rationing adopted in this country are set out. The importance of the food-supply of a nation in time of war was realised at a very early stage in the war by the Physiological Committee of the Royal Society, and it is largely due to the investigations initiated by the Food (War) Committee of the Royal Society that the necessary information was available when the limitation of the food-supply of the nation came about later. As Chairman of the Food (War) Committee, and as Scientific Adviser to the Ministry of Food, Prof. Starling was not only in intimate contact with the question of food-supply during the war, but was very largely responsible for the measures which were adopted with such success for feeding this country in the most critical period of the war. This little book should be read not only by physiologists and those interested in the study of food, but by all intelligent men and women.

W. S.

Moments of Genius. By Arthur Lynch. [Pp. xi + 257.] (London: Philip Allen & Co., 1919.)

Colonel Arthur Lynch has produced a very interesting book. He analyses twenty characters, namely, two types each of the Soldier, Orator, Stoic, Beauty, Artist, Poet (Southern and English), Philosopher, Mathematician, and Biologist. Examples of the three latter groups are Aristotle, Descartes, Abel, Galois, Schwann, and Darwin. Each character is presented in a short essay which is selected as the most dramatic moment of the subject's life, and the subject is figured to us in a few able lines. Colonel Lynch is evidently a man of very wide sympathies and knowledge, possessing much of the true poetical capacity for detecting the vital spot. He is also, what I personally always admire, a hero-worshipper. This is not what I have called a mean book written by, for, and about mean people--that is, the kind of book which our modern reviewers seem to delight in. Colonel Lynch's book is one of high ideals--after all, the only thing worth reading about. When men become more civilised they will understand that their real gods are their greatest and best men.

R. R.

Complete Manual of the Auxiliary Language Ido. Grammar, Grammatical Exercises, Key Ido-English and English-Ido Vocabularies. Revised by L. DE BEAUFRONT, President of the French Idistic Society. In conformity with the decisions of the Ido Academy. [Pp. xiv + 194.] (London: Sir Isaac Pitman & Sons, 1919. Price 5*s*. net.)

IN SCIENCE PROGRESS of January 1920 we published some correspondence on the respective merits of Latin and of Ido as an international language; and this little book will enable those who are not familiar with the latter to understand its scope and advantages. Ido is, of course, derived very largely from Dr. Zamenhof's Esperanto. The great beauty of both languages consists in the extreme simplicity of their grammar and in their euphonious Italian syllabication. In fact, they can be almost called Spanish or Italian without the complicated grammars. For instance, every single substantive ends in *o* and every plural substantive in *i*, while every adjective ends in *a* and every adverb in *e*. One could almost speak these tongues on this information alone if one remembers that nearly all the roots are Latin. Esperanto had the great defect that some consonants with circumflex accents over them were employed—thus at once limiting its adaptability for printing, because few founts of type contain such marked letters. Esperanto also differentiated the accusative case by the addition of the letter *n*; but Ido does this only in the case of accusatives placed before the verb. And there are other small differences. All the same we cannot say that Ido is much more euphonious than Esperanto as claimed in the Introduction; and we may doubt whether it cannot be improved still further, and whether the appellation "final international language" will be everywhere accepted. For example, would it not be better to use Latin and Greek roots entirely where suitable ones exist, in preference to taking occasional roots from German and English such as "warfo" for "wharf," and "welder" to weld, "westo" the west, and "whisto" for whist? This practice appears to us to be an unnecessary tax on the memory, because almost everyone knows the Latin and Greek roots and would naturally employ them in international speech. I once heard a great scholar maintain that Milton's Latin verse would outlive his *Paradise Lost*. Why? because Latin and Greek are likely to outlast all modern languages. And I think that there is much in this argument.

Those who have not examined little books like these laugh at artificial languages because they do not understand how extremely easy it is to learn them. But, more than that, these artificial languages must be a very good training for learning natural ones—as is quite justifiably claimed in the Foreword.

After all, I am inclined to think that English can be made the International language if only we would be wise enough to change our ridiculous irregular spelling. A large majority of our roots are from the Latin or Greek, and our grammar is extremely simple except for our irregular verbs. On the other hand, it is a harsh language and contains no less than thirteen different vowels, which are almost always too much for foreigners. How hopeless English spelling really is can be known only by those who have studied the innumerable new schemes of spelling which have been proposed during the last two centuries—and all of which have failed.

R. R.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Modern Geometry: The Straight Line and Circle. By Clement V. Durrell, M.A., Senior Mathematical Master at Winchester College. London: Macmillan & Co., St. Martin's Street, 1920. (Pp. x + 145.) Price 6*s*. net.

Mathematics for Engineers. Part II. The Directly-useful Technical Series. By W. N. Rose, B.Sc. Eng. (Lond.), late Lecturer in Engineering Mathematics at the University of London Goldsmith's College. London: Chapman & Hall, 11, Henrietta Street, W.C.2, 1920. (Pp. xiv + 419). Price 13*s*. 6*d*. net.

The Elements of Descriptive Astronomy. By E. O. Tancock, B.A., Assistant Master at Wellington College, Berks. Second Edition. Revised with additional matter on Practical Work for Engineers with small instruments. Oxford: at the Clarendon Press, 1919. (Pp. 158.) Price 3*s*. net.

Manual of Meteorology. Part IV. The Relation of the Wind to the Distribution of Barometric Pressure. By Sir Napier Shaw, Sc.D., F.R.S., Director of the Meteorological Office; Reader in Meteorology in the University of London. Cambridge: at the University Press, 1919. (Pp. xvi + 164.) Price 12*s*. 6*d*. net.

The Radiant Properties of the Earth from the Standpoint of Atmospheric Thermodynamics. By Frank W. Very. Occasional Scientific Papers of the Westwood Astrophysical Observatory. Number 3. Boston: The Four Seas Company, 1919. (Pp. 84.)

Researches in Physical Optics. Part II. Resonance Radiation and Resonance Spectra. By. R. W. Wood, LL.D., Professor of Experimental Physics in the John Hopkins University, Foreign Member of the Royal Society. New York: Columbia University Press, 1919. (Pp. viii + 184.) With 10 Plates.

Ions, Electrons, and Ionising Radiations. By James Arnold Crowther, Sc.D. University Demonstrator in Experimental Physics in the Cavendish Laboratory. Cambridge: London: Edward Arnold, 1919. (Pp. xii + 276.) Price 12*s*. 6*d*. net.

Mesures Pratiques en Radioactivité. Par W. Makower, Maître ès Arts, Docteur ès Sciences, Maître de Conférences et Chef des Travaux Pratiques de Physiques à l'Université de Manchester, et H. Geiger, Docteur en Philosophie, Maître de Conférences de Physique à l'Université de Manchester. Traduit de l'Anglais par E. Philippi, Licencié ès Sciences, Paris: Gauthier-Villars et Cie, Editeurs, Libraires du Bureau des Longitudes, de l'École Polytechnique, Quai des Grands-Augustins, 55, 1919. (Pp. vii + 181.)

Alcohol: Its Production, Properties, Chemistry, and Industrial Applications. With Chapters on Methyl Alcohol, Fusel Oil, and Spirituous Beverages.

By Charles Simmonds, B.Sc., Analyst in the Government Laboratory, London. London: Macmillan & Co., St. Martin's Street, 1919. (Pp. xx + 574.) Price 21s. net.

Notions Fondamentales de Chimie Organique. Par Charles Moureu, Member de l'Institut et de l'Academie de Médecine, Professeur au Collège de France. Sixième Édition. Paris: Gauthier-Villars et Cie, Editeurs, Libraires de Bureau des Longitudes, de l'École Polytechnique, Quai de Grands-Augustins, 55, 1920. (Pp. 552.)

Industrial Gases. By Harold Cecil Greenwood, O.B.E., D.Sc., F.I.C., Scientific Adviser, Munitions Inventions Department. London: Baillière, Tindall & Cox, 8, Henrietta Street, Covent Garden, 1920. (Pp. vii + 371.) With 23 figures. Price 12s. 6d. net.

Cours de Chimie a l'Usage des Etudiants, P.C.N. et S.P.C.N. Par R. de Forcrand. Deuxième Édition. Tome I, Généralités –Chimie Minérale. (Pp. viii + 437.) Tome II, Chimie Organique Chimie Analytique. Applications numériques. Paris: Gauthier-Villars et Cie, Quai des Grands-Augustins, 55, 1919. (Pp. 527.)

Chemistry in Every-day Life. Opportunities in Chemistry. By Ellwood Hendrick. London: University of London Press, Ltd., 18, Warwick Square, E.C.4, 1919. (Pp. xii + 101.) Price 5s. 6d. net.

The Dyeing Industry. Being a Third Edition of "Dyeing in Germany and America." By S. H. Higgins, M.Sc., Dyers Company Gold Medallist for Research, 1913, Technical Chemist and Works Manager. Manchester: at the University Press, 12, Lime Grove, Oxford Road. London: Longmans, Green & Co., 1919. (Pp. viii + 189.) Price 8s. 6d. net.

A Class-book of Organic Chemistry. By J. B. Cohen, Ph.D., B.Sc., F.R.S., Professor of Organic Chemistry, The University, Leeds. Vol. II, for Second-year Medical Students and others. London: Macmillan & Co., St. Martin's Street, 1919. (Pp. vii + 156.) Price 4s. 6d.

Tin Ores. By G. M. Davies, M.Sc., F.G.S. Imperial Institute Monographs on Mineral Resources with Special Reference to the British Empire. Prepared under the direction of the Mineral Resources Committee of the Imperial Institute, with the assistance of the Scientific and Technical Staff. London: John Murray, Albemarle Street, W., 1919. (Pp. xx + 111.) Price 3s. 6d. net.

Manganese Ores. By A. H. Curtis, B.A. (Lond.), F.G.S., Assoc.M.Inst., C.E., M.Inst.M.M. Imperial Institute Monographs on Mineral Resources with Special Reference to the British Empire. Prepared under the direction of the Mineral Resources Committee of the Imperial Institute, with the assistance of the Scientific and Technical Staff. London: John Murray, Albemarle Street, W., 1919. (Pp. x + 118.) Price 3s. 6d. net.

Forests, Woods and Trees in Relation to Hygiene. By Augustine Henry, M.A., F.L.S., M.R.I.A., Professor of Forestry, Royal College of Science, Dublin. London: Constable & Co., 1919. (Pp. xii + 315.) With 49 Figures in the text. Price 18s. net.

The Fungal Diseases of the Common Larch. By W. E. Hiley, M.A., School of Forestry, Oxford. Oxford: at the Clarendon Press, 1919. (Pp. xi + 204.) With Illustrations. Price 12s. 6d. net.

Wonders of Insect Life. Details of the Habits and Structure of Insects, illustrated by the Camera and the Microscope. By J. H. Crabtree, F.R.P.S. London: George Routledge & Sons. New York: E. P. Dutton & Co. (Pp. viii + 211.) Price 6s. net.

Physiology of Farm Animals. By T. B. Wood, C.B.E., M.A., F.R.S., and F. H. A. Marshall, Sc.D., Part I, General. By F. H. A. Marshall, Fellow of Christ's College, Cambridge, and Reader in Agricultural Physiology. Cambridge: at the University Press, 1920. (Pp. xii + 204.) With 105 Figures. Price 16s. net.

Altitude and Health. By F. F. Roget, A "Privat-Docent" Professor in the University of Geneva. London: Constable & Company, 10, Orange Street, W.C., 1919. (Pp. xii + 180.) Price 12s. net.

Action de la Chaleur et du Froid sur l'Activité des êtres Vivants. Par Georges Matisse. Paris: Émile Larosse, Libraire-Éditeur, 11 Rue Victor-Cousin, 1919. (Pp. ii + 356.)

The Life of Pasteur. By René Vallery-Radot. Translated from the French by Mrs. R. L. Devonshire, with an Introduction by Sir William Osler, Bart., M.D., F.R.S., Regius Professor of Medicine. Oxford University. London: Constable & Company, 1919. (Pp. xxi + 484.) Price 10s. 6d. net.

The Use of Colloids in Health and Disease. By Alfred B. Searle. With Foreword by Sir Malcolm Morris, K.C.V.O. London: Constable & Company, Ltd., 10, Orange Street, Leicester Square, W.C., 1920. (Pp. vii + 120.) Price 8s. 6d. net.

Food Poisoning and Food Infections. By William G. Savage, B.Sc., M.D. (Lond.), D.P.H., County Medical Officer of Health, Somerset. Cambridge: at the University Press, 1920. (Pp. ix + 247.) Price 15s. net.

A Manual of Practical Anatomy. A Guide to the Dissection of the Human Body. By Thomas Walmsley, Professor of Anatomy in the Queen's University of Belfast. With a Preface by Thomas H. Bryce, M.A., M.D., Professor of Anatomy in the University of Glasgow. In Three Parts. Part I, The Upper and Lower Limbs. London: Longmans, Green & Co., 1920. (Pp. viii + 176.) Price 9s. net.

Physiology and National Needs. Edited by W. D. Halliburton, M.D., LL.D., F.R.C.P., F.R.S., Professor of Physiology, King's College, London. London: Constable & Company, 1919. (Pp. vii + 162.) Price 8s. 6d. net.

Military Psychiatry in Peace and War. By C. Stanford Read, M.D. (Lond.), Physician, Fisherton House, Mental Hospital, Salisbury. London: H. K. Lewis & Co., 1920. (Pp. vii + 168.) With two Charts. Price 12s. 6d. net.

The Engines of the Human Body. Being the Substance of Christmas Lectures given at the Royal Institution of Great Britain, Christmas, 1916–17. By Arthur Keith, M.D., LL.D., F.R.S. London: Williams & Norgate, 14, Henrietta Street, Covent Garden, W.C.2, 1919. (Pp. xii + 284.) With 2 Plates and 47 Figures. Price 10s. 6d. net.

Pre-Palæolithic Man. By J. Reid Moir, F.R.A.I. Ipswich: W. E. Harrison, The Ancient House; London: Simpkins, Marshall, Hamilton, Kent & Co., Paternoster Row. (Pp. 67.) With 29 Plates.

An Introduction to Anthropology. General Survey of the Early History of the Human Race. By the Rev. E. O. James, B.Litt., F.C.S., Vicar of St. Peter's, Limehouse, Fellow of the Royal Anthropological Institute, London: Macmillan & Co., St. Martin's Street, 1919. (Pp. 255.) Price 7s. 6d. net.

Man, Past and Present. By A. H. Keane. Revised and largely re-written by A. Hingston Quiggin and A. C. Haddon, Reader in Ethnology, Cambridge. Cambridge: at the University Press, 1920. (Pp. vii + 582.) With 16 plates. Price 36s. net.

Aircraft in Peace and the Law. By J. M. Spaight, O.B.E., LL.D. London: Macmillan & Co., St. Martin's Street, 1919. (Pp. viii + 233.) Price 8*s.* 6*d.* net.

Applied Aerodynamics. By Leonard Bairstow, F.R.S., C.B.E., Expert Adviser of Aerodynamics to the Air Ministry. (Pp. xi + 565.) With 22 Plates and 255 Figures. Price 32*s.* net.

Telephonic Transmission, Theoretical and Applied. By J. G. Hill, Assistant Staff Engineer, General Post Office, London. London: Longmans, Green & Co., 39, Paternoster Row, 1920. (Pp. xvi + 398.) With 196 Diagrams. Price 21*s.* net.

Mystica et Lyrica. By Cloudesley Brereton. London: Elkin Mathews, Cork Street, 1919. (Pp. 126.) Price 6*s.* net.

Enjoying Life, and Other Literary Essays. By W. N. P. Barbellion. London: Chatto & Windus, 1919. (Pp. xvi + 246.) Price 6*s.* net.

Spiritual Pluralism and Recent Philosophy. By C. A. Richardson, M.A. (Cantab.) Cambridge: at the University Press, 1919. (Pp. xxi + 325.) Price 14*s.* net.

Musings of an Idle Man. By R. H. Firth, K.B.E., C.B., Colonel and late Deputy Director of Medical Services with the British Army in France. London: John Bale, Sons & Danielsson, 88, Great Titchfield Street, Oxford Street, W., 1919. (Pp. 354.) Price 7*s.* 6*d.* net.

Totem and Taboo, Resemblances between the Psychic Lives of Savages and Neurotics. By Professor Sigmund Freud, LL.D. Authorised English Translation with Introduction by A. A. Brill, Ph.B., M.D., Assistant Professor of Psychiatry, N.Y. Post-Graduate Medical School. London: George Routledge & Sons, 1919. (Pp. xii + 268.) Price 10*s.* 6*d.* net.

The Road to En-Dor. Being an Account of how two Prisoners of War at Jozgad, in Turkey, won their way to Freedom. By E. H. Jones, Lieut. R.A.F. London: John Lane, The Bodley Head, W.; New York: John Lane Company, 1920. (Pp. xiii + 351.) Price 8*s.* 6*d.* net.

Abridged Edition of The English Convict. New Issue, with an Introduction by Prof. Karl Pearson. A Statistical Study. By Charles Goring, M.D. B.Sc. (Lond.), Deputy Medical Officer, H.M. Prison Service. London: published by His Majesty's Stationery Office, 1919. (Pp. xvi + 275.) Price 3*s.* net.

John Murray III, 1808–1892. A Brief Memoir. By John Murray IV, with Portrait and Illustrations. London: John Murray, Albemarle Street, W., 1919. (Pp. vii + 106.) Price 3*s.* 6*d.* net.

Annual Magazine Subject Index, 1918. A Subject-Index to a selected list of American and English Periodicals and Society Publications. Edited by Frederick Winthrop Faxon, A.B. (Harv.). Compiled with the co-operation of Librarians. Boston: The F. W. Faxon Company, 1919. (Pp. 248.)

The Foundations of Music. By Henry J. Watt, D.Phil., Lecturer on Psychology in the University of Glasgow and to the Glasgow Provincial Committee for the Training of Teachers, Sometime Lecturer on Psychology in the University of Liverpool. Cambridge: at the University Press, 1919. (Pp. xiii + 237.) Price 18*s.* net.

The Century of Hope. A Sketch of Western Progress from 1815 to the Great War. By F. S. Marvin. Second Edition. Oxford: at the Clarendon Press, 1919. (Pp. vii + 358.)

Religion and Culture. A Critical Survey of Methods of Approach to Religious Phenomena. By Frederick Schleiter, Ph.D. New York: Columbia University Press. London: Oxford University Press, 1919. (Pp. x + 206.) Price 8*s*. 6*d*. net.

Spring-time and Other Essays. By Sir Francis Darwin. London: John Murray, 50A Albemarle Street, W., 1920. (Pp. xi + 242.) Price 7*s*. 6*d*. net.

Science and Life. Aberdeen Addresses. By Frederick Soddy, M.A., F.R.S., Dr. Lee's Professor of Inorganic and Physical Chemistry, University of Oxford. London: John Murray, Albemarle Street, W., 1920. (Pp. xii + 229.) Price 10*s*. 6*d*. net.

An Examination of William James's Philosophy. A Critical Essay for the General Reader. By J. E. Turner. Oxford: B. H. Blackwell, Broad Street, 1919. (Pp. vii + 77.) Price 4*s*. 6*d*. net.

Food Supplies in Peace and War. By Sir R. Henry Rew, K.C.B., Treasurer, International Statistical Institute, Hon. Foreign Secretary, Royal Statistical Society. London: Longmans, Green & Co., 39 Paternoster Row, 1920. (Pp. vii + 183.) Price 6*s*. 6*d*. net.

La Méthode Intuitive de M. Bergson. Essai Critique. Par M. T.-L. Penido, Docteur en Philosophie, Genève. Édition Atar, Corraterie 12 Paris: Libraire Félix Alcan, Boulevard St.-Germain, 108, 1919. (Pp. 222.) Price 4.50 frs. net.

Post-Graduate Teaching in the University of Calcutta, 1918–19. Calcutta University Press. (Pp. 161.)

The Natural Wealth of Britain. Its Origin and Exploitation. By S. J. Duly, B.A. (Cantab.). London and New York: Hodder & Stoughton, 1919. (Pp. x + 319.) Price 6*s*. net.

ERRATA

In the January number the following words in Mr. G. H. Richardson's correspondence need correction

On page 436, *instead of*

„ impera-
tive -ez

should be

imperative -ez

On page 437, *last paragraph but one, the passage reading* "organisation" as a condition is organ-iz-es-o; as an action, organ-iz-o; as a product, organ-iz-uro, *should be placed at the end of the preceding paragraph after the word* "numero," *and the word printed* Edileyo *should be* Editeyo.

On page 438, *last full line, the word printed* pocas *should be* povas, *and the last word*, l'existo, *should have an interrogation mark, thus:* l'existo?

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